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Flourishing and Discordance:
On Two Modes of Human Science Engagement with
Synthetic Biology

by

Anthony Stavrianakis

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

in

Anthropology

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Paul Rabinow, Chair

Professor Xin Liu

Professor Charis Thompson

Fall 2012

Abstract

Flourishing and Discordance: On Two Modes of Human Science Engagement with Synthetic Biology

by

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Doctor of Philosophy in Anthropology

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This dissertation takes up the theme of collaboration between the human sciences and natural sciences and asks how technical, veridictional and ethical vectors in such co-labor can be inquired into today.

I specify the problem of collaboration, between forms of knowledge, as a contemporary one. This contemporary problem links the recent past of the institutional relations between the human and natural sciences to a present experience of anthropological engagement with a novel field of bioengineering practice, called synthetic biology.

I compare two modes of engagement, in which I participated during 2006–2011. One project, called Human Practices, based within the Synthetic Biology Engineering Research Center (SynBERC), instantiated an anthropological mode of inquiry, explicitly oriented to naming ethical problems for collaboration. This project, conducted in collaboration with Paul Rabinow and Gaymon Bennett, took as a challenge the invention of an appropriate practice to indeterminate ethical problems. Flourishing, a translation of the ancient Greek term *eudaemonia*, was a central term in orienting the Human Practices project. This term was used to posit ethical questions outside of the instrumental rationality of the sciences, and on which the Human Practices project would seek to work.

A second project, the Socio-Technical Integration Research (STIR) project, based at the Arizona State University's Center for Nanotechnology in Society (ASU-CNS), was an explicitly 'method driven' project, whose rationale was for human scientists, through the use of a method, to act as mediums for the reflexivity, and self-observation, of research scientists relative to their on-going projects. The aim was for such interaction and self-observation to produce modulations of thought and practice within research settings. I used the method, from May-December 2009, within a bioengineering laboratory of a newly established Department of

Biosystems science and engineering (D-BSSE) at the Swiss Federal Institute of Technology (ETH).

The comparison on which I reflect is between one mode of engagement characterized by its encompassing ethical orientation, and a mode characterized by its methodology and orientation to the latent social aspects of research decisions, made within on-going work. With respect to their relation, I diagnose the problematic effects of parameterizing the goods of biology and the stakes of collaboration solely within the dominant ameliorative and industrial norms and values of the scientific field.

The general demand in the present, to modify the practice of science with respect to ethical questions, was in this case unable to be actualized. I argue that the projects in which I participated were structured in a double bind situation in which the transformation of the ethical field in which bioscience operates, was simultaneously demanded (by a range of funding agencies, political activists, bio scientists and human scientists) and undermined. I argue that the discord comes from incommensurable conceptions and embodied stances to the ethical ends and practices of knowing.

This blockage is set within a broader historical problematization of the relation between forms and practices of science, within research venues from the mid-19th Century to the present. The intellectual and ethical breakdowns arising from within the practice of collaboration in the present, between a specific set of bioscientific and engineering practices and two social science modes of engagement, are thus situated within a historical problematization of the relation of science and ethics.

“Whenever we undertake to pass judgment on an educational enterprise, the import of these two phrases serves as our criterion: we ask that education supply the means for a criticism of life and teach the student to try to see the object as in itself it really is.”

–Lionel Trilling

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Whilst the core of this dissertation comprises reflections on discordances in efforts at collaboration, within the human sciences and between the human sciences and biotechnical sciences, it is equally about the sustained effort at practicing anthropology, a science of human being, oriented to human goods and including such a science in the fulfillment of such goods. I wish to take this opportunity to express my gratitude to Erik Fisher, designer and convener of the STIR project, not only for including me in the project, but also for his willingness to engage me on questions of discordance and flourishing in collaboration.

I would like to take this opportunity to express my deep gratitude to my advisor, Paul Rabinow, not only for the opportunity to collaborate in this endeavor and for his intellectual guidance, but also for the formative effects of such participation and guidance. I heartily thank Gaymon Bennett and James Faubion for their support and collaboration, as well as my dissertation supervising committee, Professor Liu Xin and Professor Charis Thompson.

In gratitude for friendship and sustenance, I thank my family and friends.

Introduction

Orientation to an Anthropology of the Contemporary

“We have almost ceased to notice, to cite one striking example, the differences and oppositions between the diagnosis of the problems of our times which traces the persistent crises of a scientific and technological age to the fact that our moral and spiritual development has not kept pace with our scientific and technological advance and the diagnosis of our troubles as due to the fact that the social sciences have lagged behind the natural sciences and that our power to control nature exceeds our power to control man.”

–Richard McKeon.¹

This dissertation takes up the theme of collaboration between the human sciences and natural sciences.^{2 3} With respect to the epigram, what are the differences and oppositions between a diagnosis of spiritual paucity in the face of technological developments, and a diagnosis of a failure to bring the sciences, both human and natural, into an appropriate relation, given technical capacities to transform nature? The first diagnosis might be read as epochal and tragic; the moral crisis of technology characterized as persistent and the possibility of redemption deferred to faith. The second diagnosis poses the problems of a

¹ Richard McKeon, *Thought, Action, and Passion* (Chicago: University of Chicago Press), 1954, 3.

² Wilhem Dilthey’s use of the term *Geisteswissenschaften* encompassed what we now distinguish as the social sciences and humanities. Wilhem Dilthey, *Introduction to the Human Sciences* (Michigan: Wayne state University Press, [1883]1988). As Rudolf Makreel notes, it is possible and curious that Dilthey’s use of the term was a German translation of J.S. Mill’s use of the term “moral sciences,” Rudolf A. Makreel, “Wilhem Dilthey and the Neo-Kantians: On the Conceptual Distinction between *Geisteswissenschaften* and *Kulturwissenschaften*,” in *Neo-Kantianism in Contemporary Philosophy*, eds. Rudolf A. Makkreel and Sebastian Luft (Indiana: Indiana University Press, 2010), 254.

³ On the use of themes, see Richard McKeon, *Thought, Action, and Passion*, 8.

“A theme or a concept is an instrument in the development, defense, and refutation of doctrines and theories. The history of themes is longer in extent and broader in scope than the history of the doctrines that specify the theme in any field or in any form of action, since the development of themes includes the significances and implications which relate disparate doctrines, connect the histories of separated theories and sciences, and explain heterogeneous applications of developed doctrines in other fields than those in which they originally appeared. Some themes which were first elaborated by the Greeks have influenced later developments of doctrine by the pattern of interrelations they suggested or laid bare.”

Rabinow and Bennett have re-worked McKeon concept of a theme in the following manner: “A theme is an artful presentation of a problem space that groups heterogeneous topoi into a systematic frame.” <http://bios-technika.net/concepts.php#theme>

technological age as a problem of relations between modes of knowing, and furthermore, as a problem to be worked on. This thesis asks how technical, veridictional and ethical vectors in the relation between the human and natural sciences can be inquired into today. It takes up this theme as an anthropological problem; both terms require some orienting comments.⁴

The theme is problematic because of intellectual indeterminations in practice with respect to the mode, form and practice of collaboration between the human and natural sciences.⁵ The objects of collaboration, what collaboration was supposed to be on, were intellectual and ethical breakdowns arising from within a specific set of bioscientific and engineering practices. It is a specifically anthropological problematic, because the objective of such co-labor was to make indeterminations and discordances available for thought by way of concrete forms of the ‘human thing’s’ self-observation of its capacities to participate in the world.⁶

Following a path of thinkers since Kant, I choose to take up problems (the intellectual and practical troubles) of “cultural progress,” of which developments in the biosciences must be included, as problems of “education” and “use.”⁷ These terms, in English, are liable to be misunderstood as reducible to the synonyms ‘training’ and ‘utility.’ A broader semantic range of these terms will in fact operate as crucial variables for specifying what I think is specifically “ethical” about the intellectual and practical breakdowns, within and between the sciences, which require collaboration.⁸ This semantic range will be oriented by the term *Bildung*,

⁴ As will become clear this project is indebted to and a contribution toward Paul Rabinow’s sustained and trenchant development of a problem oriented anthropological mode of inquiry, see especially Paul Rabinow, *Anthropos Today: Reflections on Modern Equipment* (Princeton: Princeton University Press, 2003), 13-30. Rabinow’s intellectual formation at Chicago and particularly with Richard McKeon is important to take into account.

⁵ John Dewey, “Propositions, Warranted Assertability and Truth,” in *The Later Works of John Dewey, Volume 14, 1925-1953:1939-1941: Essays, Reviews and Miscellany* (United States of America: Southern Illinois University 1988, 2008.), 181. “Inquiry begins in an indeterminate situation, and not only begins in it but is controlled by its specific qualitative nature. Inquiry, as the set of operations by which the situation is resolved (settled, or rendered determinate), has to discover and formulate the conditions which describe the problems in hand. For they are the conditions to be “satisfied” and are the determinants of “success.” Since these conditions are existential, they can be determined only by observational operations.”

⁶ The human thing, in its 19th Century manifestation was known as “Man,” the problematic subject and object of the human sciences. Michel Foucault, *The Order of Things: An Archaeology of the Human Sciences*, (Tavistock Publications, 1966), 219-221

⁷ Immanuel Kant, *Anthropology from a Pragmatic Point of View* (Cambridge, Cambridge University Press, 2006), 3.

⁸ Richard McKeon, “Character and the Arts and Disciplines,” *Ethics* Vol. 78, No. 2 (Jan., 1968), 109-123.

“The tendency in general education to repair the dichotomy of words and things by treating facts in structures and to deny the dichotomy of facts and values by treating possibilities in actualities is hindered by the separation of problems of character and learning in higher education. If higher education is designed to develop competence in particular fields, even if the fields of scientific inquiry and those of moral and esthetic judgment are conceived to be distinct, competence in very profession and in each field of science, social science, and the humanities is made to consist in mastery of the facts and of the methods of treating the problems of the field; and the problems of attitude and purpose, taste and morality, feeling and will, adjustment and autonomy are separated from the problems of

understood as a subject's capacity to give form to an activity insofar as it requires a specific character to practice the activity; arguably, such a status is never determinately reached since the subject's capacity to practice an activity, is tested by situations and cases throughout "life," which famously has been called that which is capable of error.^{9 10}

Such a capacity requires the effort to assimilate an external diversity of objects, instruments, forms of knowledge, into a practice in the world. Again following Kant, the problem is anthropological because "the most important object" toward which the human thing can apply its "education" and "use" is the human thing itself.¹¹ The object is taken up not in physiological terms, the human thing as ('individual') biological being, but from a pragmatic point of view; "the investigation of what he (sic, a subject) as a free-acting being makes of himself, or can and should make of himself."¹² Neither is this object taken up in metaphysical terms; the synthesis of an external diversity into a practice does not first require foundations or "first principles" before any such activity can take place. The initial anthropological level of inquiry is the work of thought in, and working through of, the present at the level of experience. As Foucault instructs us in his insightful and powerfully original reading of Kant's *Anthropology from a Pragmatic Point of View*, such an investigation is simultaneously one of 'the world':

"The world being its own school, the aim of anthropology is to situate man [see f.n. 6] within this instructive context. It [anthropology] will therefore be both, indissociably: the analysis of how man acquires this world, which is to say how he manages to take his place in the world and participate in the game: *Mitspielen* [to participate]; and, at the same time, the synthesis of the prescriptions and rules that the world imposes on man, which train him, readying him to take control of the game: *das Spiel verstehen* [to understand the game]."¹³

cognition and objective knowledge. Education then is thought to have become primarily intellectual and in need of reform to provide interest and motivation and to take into account the whole man, and the rational processes developed in such education have no direct relation to action or appreciation, responsibility or taste."

⁹ Michel Foucault, "Life: Experience and Science," in *Aesthetics, Method, and Epistemology: Essential Works of Foucault 1954-1984 Volume 2*, ed. James D. Faubion, trans. Robert Hurley and others (New York: The New Press, 1998), 471.

¹⁰ On the specificity of the ethical character of the term practice please see, Alisdair Macintyre, *After Virtue: a study in moral theory* (Notre Dame: University of Notre Dame Press), 175. By practice I am going to mean "...activity through which goods internal to that form of activity are realized in the course of trying to reach those standards of excellence which are appropriate to and partially definitive of that form of activity with the result that human powers to achieve excellence and human conceptions of the ends and goods involved are systematically extended."

¹¹ Kant, *Anthropology*, .3.

¹² *Ibid.*

¹³ Michel Foucault, *Introduction to Kant's Anthropology* (Los Angeles, Semiotext(e), 2008), 54.

Two Modes: An Inquiry.

In this thesis I specify the problem of collaboration between forms of knowledge, on relations of education and use, with respect to a recent past of the relations between the human and natural sciences. I compare two modes of engagement, in which I participated between 2006–2011, which attempted to re-work the mode, form and practice of the engagement of the human and natural sciences. The hypothesis orienting the investigation was that these two projects were part of a common problematization.

Problematization is a concept Michel Foucault forged, and only partially developed before his death, to indicate sites for inquiry where relations of knowledge, politics and ethics were under reconfiguration. A problematization, he suggested, is

“the set of discursive or nondiscursive practices that makes something enter into the play of the true and false, and constitute it as an object for thought (whether under the form of moral reflection, scientific knowledge, political analysis, etc.)”¹⁴

In conversation with Paul Rabinow on the question of what constitutes a “history of problematics,” Foucault responded in diagnostic fashion, so as to indicate how such a problematization might be grasped as an object for the inquirer; what makes simultaneously possible differing responses to something having entered into the ‘the play of true and false’? As he said,

“To one single set of difficulties, several responses can be made. And most of the time different responses actually are proposed. But what must be understood is what makes them simultaneously possible: it is the point in which their simultaneity is rooted; it is the soil that can nourish them all in their diversity and sometimes in spite of their contradictions.”¹⁵

As such, problematizations have two sides; something enters the play of true and false and multiple possible responses are available to people in the situation. This indicates that my inquiry requires an identification of the parameters through which these relations of the sciences, their governmental, ethical and veridictional relations, were modulated, were broken down and given changes of medium, i.e. remediated. It will require specification of the truth, governance and ethical variables of the problem and work toward naming the common problematization. In short, the motion from problem to problematization, working in and through the

¹⁴ Michel Foucault, *Dits et écrits*: Vol.4 (Paris: Éditions Gallimard, 2001), 456-7.

¹⁵ Michel Foucault, “Polemics, Politics, and Problematizations,” in *The Foucault Reader*, ed. Paul Rabinow (New York: Penguin, 1984).

experience of multiple possible responses to a set of difficulties, is the arc of this thesis.

Briefly put, if the human sciences wish to engage with the natural sciences on the significance and ramifications of their work, how can such engagement count within a register of the true and the false? What are the problems on which they can engage? What forms of “moral reflection, scientific knowledge, political analysis, etc.” are appropriate? How can they be practiced so as to have effects? Finally, what mode of judgment is appropriate to such engagement?

The thesis, therefore, takes up the challenge of an anthropological comparison whose objects are not cultures. It is, to repeat for clarity’s sake, a comparison of *modes* of human science engagement with the biosciences on questions of ethics. The comparison is perhaps methodologically challenging due to an indetermination over the comparability of the objects. I will specify the projects in more detail shortly, but initially I will say that one project instantiated a self-consciously anthropological mode of inquiry, explicitly oriented to naming ethical problems for collaboration, which took as a challenge the invention of an adequate practice. This project, called Human Practices, was initiated by my thesis advisor Paul Rabinow in collaboration with a then graduate student in theology, Gaymon Bennett. Based within a bioengineering center (SynBERC), as one of four scientific thrusts of the center, the Human Practices thrust was oriented to the question of how to invent such an anthropological mode of collaborative inquiry on the ethics of bioengineering.

A second project, the Socio-Technical Integration Research (STIR) project, based at the Arizona State University’s Center for Nanotechnology in Society (ASU-CNS), was an explicitly ‘method driven’ project, whose stakes were not named in advance and whose rationale was for human scientists, through the use of a method, to act as conduits or mediums for the reflexivity, the self-observation, of research scientists relative to their on-going projects. The project employed a group of ten graduate students to use the method in approximately twenty field sites. I was one of these students and I used the method in an exploratory fashion in Berkeley, in the same research center in which our Human Practices team was working, and then systematically at a bioengineering laboratory of a newly established Department of Biosystems science and engineering (D-BSSE) at the Swiss Federal Institute of Technology (ETH).

Problems, Scene, Venues: A Manner of Comparison.

"It is not the 'actual' interconnections of 'things' but the conceptual interconnection of problems that defines the scope of the various sciences. A new 'science' emerges where new problems are pursued by new methods and truths are thereby discovered which open up significant new points of view."—Max Weber¹⁶

Traditionally, comparison in anthropology has been for the purpose of identifying general kinds of object for the purpose of classification, for example, kinship systems on which one could model the totality of social relations. For Alfred Radcliffe-Brown, the aim of a comparative anthropology was classification contributing to generalized knowledge of kinds of (social) structure.¹⁷ Significance under this method is generated by relations between, what he terms, logical properties of a class which are made up of the “actual interconnections of spatio-temporal elements” in social systems.¹⁸

By contrast, since the objects of this inquiry are not cultures, but rather indeterminations rendered as problems within possible problematizations, then the purpose of comparison will not be the same as Radcliffe-Brown’s. Rather, following Max Weber, I seek to inquire into the conceptual interconnections of the problems indicated by these different modes of inquiry.¹⁹ Such interconnection of problems requires that I attempt to take them up from a point of view capable of giving them significance. This then is the important point about comparison; the ability to compare the objects of inquiry, these modes of engaging science and question of ethics, is dependent on a problem, aiming at significance, constituted by a “point of view.”

My comparison, I acknowledge readily, is asymmetrical as the point of view out of which the comparison is being made is internal to, was developed from within, one of the sides of the comparison. This is not actor-network theory. I am a situated subject of a practice of inquiry and a form of life.

Intellectual Genealogies.

I situate myself in a particular intellectual genealogy. The genealogy I was able to connect myself to goes through two lines of thought, an American tradition

¹⁶ Max Weber, “Objectivity in Social Science,” in *The Methodology of the Social Sciences*, trans. and ed. Edward Shils and Henry Finch (New York: The Free Press, 1949), 67.

¹⁷ A. R. Radcliffe-Brown, *A Natural Science of Society* (Glencoe, Illinois: The Free Press, 1957).

¹⁸ *Ibid.*

¹⁹ Max Weber, “Objectivity,” 62.

of thought by way of the University of Chicago (McKeon, with whom we began and John Dewey) and a French tradition of thought, by way of Foucault. Both lines have a common source in asking how anthropological knowledge can respond to a fundamental problem of the significance of the human thing's search for knowledge of itself.

The orienting comment that I wish to make and which I repeat at a number of junctures through the thesis is that the problem of knowledge in this thesis is an ethical and not epistemological problem. Put simply, this has two sides to it: on the one side it is a question of how a subject is capable of being the subject of that which they claim to know. On the other side, it is a question of how interventions into the world, the activities of knowing and making, always presuppose the ends towards which one seeks to move.²⁰ Curiously, the two sides to the questions do have certain resonances with what John Dewey calls "thinking."

Whether the question of the comportment and capacity for the human thing's self-knowledge is fundamentally "Western," I will demur from answering. I will let it be said, merely, that work in comparative religion, philosophy and anthropology shows geographically, culturally and temporally plural returns of questions of the relation of knowledge and life, even if the structure of those questions are not universal or timeless.²¹

Science Studies.

Science Studies, in my reading, offers an alternative set of questions and stakes to the ones I will pursue in this thesis. I will give a very brief overview of what I consider to be the important historical developments in this mode of inquiry, and I will follow this schematization with a point of genealogical intersection, in the guise of feminist lines of thought which have contributed to the development of a wide field of Science Studies.

Chapter One takes up a moment in the relation between the natural sciences and humanities and human sciences before the emergence of a field called Science Studies.²² Chapter One links the two genealogical questions named above, of the

²⁰ I take up these points respectively in Chapters Two and Four. The normativity of the practice of knowing in Canguilhem I take from his essay, "Machine and Organism," in *Knowledge and Life*, ed. Stephanos Geroulanos and Todd Meyers (Fordham University Press, 2008).

²¹ To give an example, Francois Jullien's comparative work on Chinese and Greek thought, in *Vital Nourishment* he shows how Aristotle's and Zuangzi's philosophical thought depart from a common renunciation, even if they lead in diametrically opposed directions for the relation of thought and life. Francois Jullien, *Vital Nourishment: Departing From Happiness*, trans. Arthur Goldhammer (Cambridge: MIT Press: Zone Book, 2007), 15-16.

²² For a commanding and synthetic account of the field, its various lines of development and insights see Charis Thompson, *Making Parents: The Ontological Choreography of Reproductive Technologies* (MIT Press, 2005), 31-53.

subjectivational stakes of knowing and the ethical significance of the ends of a practice internal to the development of science and technology, to an actual socio-historical situation that I argue is crucial for understanding the contemporary moment's possibilities and limitations in collaboration between forms and practices of knowledge.

For a first mode of American sociology of science circa 1930, one that developed out of, as well as in contrast to, an earlier German *Wissensoziologie*, it was the value of the practice of science itself, with respect to the political conditions which could sustain that practice, which marked the significance of inquiry into science. This mode, associated most frequently with the work of Robert K. Merton, was criticized for having a normative but supposedly empirically unsubstantiated orientation to the norms governing scientific practice.

²³

A critique of this mode, from what would become the Edinburgh and Bath Schools of the Sociology of Scientific Knowledge (SSK), which posed questions about the cultural, ethical and political significance of science, was that the work of Merton and his students left the content of scientific knowledge untouched by sociological explanation. Merton's was a sociology of scientists not a sociology of science. For SSK and the "conventionalist" analysis they developed, the significance for an account of science came from treating scientific knowledge naturalistically, which meant offering the same social explanation for truth as for error.²⁴ This mode of analysis which explains belief by convention, eschews the normative significance of science and rests with a methodological significance of the demonstration of such convention.²⁵

There has also been an important "translational" mode of analysis, which derives its significance from a methodological intervention into the manner in which categorizations and displacements (the different effects and affects produced by things) is done. This intervention is fundamentally ontological in orientation.²⁶

²³ For German sociology of knowledge see Karl Mannheim, *Ideology and Utopia*, trans. L Wirth and E Shils (New York: Harcourt Brace Jovanovich, 1936), 239-240. Mannheim distinguished between existentially determined and the immanently determined "intellectual elements." For Merton, see Robert K. Merton, "Paradigm for a Sociology of Knowledge," in *The Sociology of Science: Theoretical and Empirical Investigations* (Chicago: University Of Chicago Press, 1979), 8 and R. K. Merton, *Science and Democratic Social Structure, Social Theory and Social Structure* (Glencoe, Free Press 1957), 551.

²⁴ For a classic account of the Wittgensteinian approach of Bloor, see David Bloor, *Knowledge and Social Imagery* (Chicago, University of Chicago Press, 1976).

²⁵ For the initial critique of Mertonian norms from what would become SSK, see, Barry Barnes and RGA Dolby, "The Scientific Ethos: A Deviant Viewpoint," *Archive of European Sociology* XI (1970): 8.

²⁶ See Annemarie Mol, *The Body Multiple* (USA: Duke University Press, 2002). Charis Thompson, *Making Parents*. Bruno Latour, *Reassembling the Social* (Oxford: Oxford University Press, 2008).

What we have then are three different kinds of project, cultural, epistemological, ontological. The reason I think this schema is worth iterating here in my Introduction is that with respect to the difference between the first two modes, the ‘convention’ problem of knowledge in SSK bypasses the question of the *worth* of truth, which in Mertonian political guise, was seen as escaping the ‘serious’ question of the content of knowledge. What SSK, in general, failed to take seriously was the normative orientation of truth seeking, even though the norms, values, and virtues of science are subject to change. At least one exception to this categorization that I have drawn on is Steven Shapin’s work on science as a late modern vocation.²⁷ As indicated, I think the problem of how to articulate claims about science must be tied to a subjectivational question of the mode of existence of a scientist relative to the objects they are trying to bring into the world: this is a symmetrical question which must be asked of the anthropologist as much as of the anthropologist’s object of observation. Without the subjectivational dimension, I do not see how a standard of significance can be named for the inquirer, in terms other than the methodological.

The application of a method cannot answer by itself the question, why this method? The question of the significance of inquiry into science raises the question of the purpose of science.

Situated Intersections.

If this is a partial account of both the genealogy which will be operative and a genealogy which will not be operative, it must be noted, however, that in the last decades the notion of “situated knowledge,” and the situatedness of inquiry, has come to play a particular role with respect to feminist accounts of scientific practices and their relation to life and living.

In Donna Haraway’s 1988 intervention into STS and feminist work on objectivity, she shows how a genealogy of feminist thought has responded to the “science question” in feminism, turning particularly on the epistemology and the status of objectivity as a determination of how a claim, act or enunciation gets to count as true. Haraway’s intervention turns on finding a novel response to the question I began with, of how to activate the work of thought at the level of the field of experience.²⁸ As Haraway writes, “In some critical sense that is crudely

²⁷ Steven Shapin, *The Scientific Life: A Moral History of a Late Modern Vocation* (Chicago: University of Chicago Press, 2008).

²⁸ This is one way of taking up Haraway’s intervention; I of course read her relative to my position and aim. I found it striking how she can be read in relation to what Kant termed the *Gemut*; the corporeal awareness of self-affection. The *Gemut* includes “the capacity to affect the unity of empirical apperception (*animus*) but not its substance (*anima*). *Gemut* does not designate a substance (whether material or ideal) but is the position or place of the

hinted at by the clumsy category of the social or of agency, the world encountered in knowledge projects is an *active* entity.”²⁹ Haraway indicates several elements of a specifically feminist response to this problematic: “Another rich feminist practice in science in the last couple of decades illustrates particularly well the “activation” of the previously passive categories of objects of knowledge. This activation permanently problematizes binary distinctions like sex and gender, without eliminating their strategic utility.”³⁰

Even if I have not actualized this intervention in a domain of problems that would be recognized as particular to feminist thought, nevertheless, the kindred response from the genealogy I am activating and that of Haraway, on the relation of the problematic relation of knowledge and care, needs to be indicated.³¹ When Haraway writes that “the issue is ethics and politics, perhaps more than epistemology,” I recognize that we are within a common world of problems, of veridiction, ethics and technoscience.³² How “the issue” of a mode of veridiction

Gemutskrafte (power or capacity for the awareness of self-affection), of sensibility, imagination, understanding and reason.” *Kant Dictionary*, S.v. “Gemut.”

The *Gemut* is according to Kant, “all life (the life principle itself) and its hindrance or furtherance has to be sought outside it, and yet, in the man himself, consequently in connection with his body,” *Critique of Judgment*, 29.

In the *Critique of Pure Reason*, Kant locates the origin of knowledge in two sources of the *Gemut*; 1) receiving representations and 2) spontaneity of concepts (knowing an object through reception of representations.) i.e. the *Gemut* is active and passive.

In Foucault’s reading of the relation of the *Gemut* to the *Anthropology*, “The only possible anthropology is that where, rather than being tied to the passivity of phenomenal determinations, the *Gemut* [the disposition through which one can exercise one’s faculties, AS] is instead animated by the work of ideas on the level of the field of experience. The *Geist* is therefore the principle of a de-dialecticized, nontranscendental dialectic oriented toward the domain of experience and playing an integral part on the play of phenomena itself. It is the *Geist* which offers the *Gemut* the freedom of the possible, stripping it of its determinations, and providing it with a future which it owes to nothing but itself.” Foucault, *Kant’s Anthropology*, 2008, 63)

Otherwise said; there is no “spirit” of our contemporary, as “spirit,” the social historical mediation of ideas, offers to our active capacity that conditions thought, action and passion, the possibility of being-otherwise.

²⁹ Donna Haraway, “Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective,” *Feminist Studies* Vol. 14, No. 3 (Autumn, 1988): 575-599.

³⁰ *Ibid.*

³¹ Feminist scholarship on science and technology seems to coincide with a transition from first to second wave feminism, characterized by the taking up of *de facto* as opposed to solely *de jure* obstacles to inequality. Although undoubtedly connected to first-wave concerns, it seems as though it is only in the 1970s that explicit scholarship on the theme of gender, sexuality and science emerged. Feminist thought was a crucial genealogy in which the limits of structuralist accounts of objects such as sex and gender were inquired into.

In terms of a number of key moments in feminist studies of science in the 1980s one might pick out the following chronological series: Evelyn Fox Keller, *Reflections on Science and Gender* (New Haven, Conn.: Yale University Press, 1985). Sandra G. Harding, ed., *The Science Question in Feminism* (Ithaca, N.Y.: Cornell University Press, 1986). Emily Martin, *The Woman in the Body* (1987). Sharon Traweek, *Beamtimes and Lifetimes* (Cambridge, Mass.: Harvard University Press, 1988).

³² For a more recent and trenchant intervention into the political and ethical stakes of technoscience which takes up the complex relations of feminist theory and ethnographic participant-observation, see Charis Thompson, *Making Parents The Ontological Choreography of Reproductive Technologies* (Cambridge: MIT Press, 2005). The text intervenes in both theoretical debates opened in the 1980s as well as ethnographically cultivating an ontological inquiry into the objects of production and reproduction at stake in assisted reproduction technologies.

is ethical and political, how I have made it my own, will, I hope, become clear through the thesis.³³

One further note on the relation of the anthropology I seek to develop and feminist thought is required to indicate one of the difficulties of a simultaneous acknowledgement of participation in a common world of problems and the need to take differing genealogies (and their stakes) of thought seriously.³⁴ The work of Marilyn Strathern is very important in indexing both what it is that experimental forms of anthropology can yield and the danger of the assimilation of feminist thought to an “ecumenical” dialogue in anthropology as a science. As she wrote in her path-opening text *The Gender of the Gift: Problems with Women and Problems with Society in Melanesia*:

“The significance of feminism is *the relative autonomy of its premises as far as anthropology is concerned*: each provides a critical distance on the other. Ideally, one would exploit the extent to which each talks past the other.”³⁵

Strathern is pointing not toward a critique at the level of substance but critique at the level of a different genealogy of anthropological knowledge, one which can take seriously the problem of incommensurability of forms of knowledge, the way in which they are not fully commensurable and their ‘partial connections.’³⁶ I am wary of an act of ‘assimilation’ of feminist thought to my project, and thus of actually reproducing a logic of domination which does not account for the particularity of the situatedness of the subjects and objects of knowledge.

³³ One point of commonality between the genealogy of a feminist mode of knowing articulated by Haraway, and the mode of knowing I seek to activate is the work of Melanie Klein and a British Kleinian, W.R Bion. The point of commonality is the import of the question of what an observation is. Haraway indicated that British Object Relations Theory, drawing mainly on the work of Melanie Klein, has been an important resource for conceptions of situated knowledge. For Bion, situations exist. Observations in a situation are said to occur in place x, at such and such at a time y. They are observations of things which occurred at this place and moment [*hic et nunc*]. An analytic situation has a space and a time. The analyst can make observations because of the existence of the situation in space and time. The conventional view of an observation is as that of a Container. Psychoanalytical observations, for Bion, and I would like to suggest anthropological observations, cannot be contained within a Conventional (scientific) Observation. The problem with the conventional view of observation is that the *objects* of anthropological/psychoanalytic observations are not *within* the analytic situation, or even in themselves. Conventional Observations (which one might include in the form and mode of ethnography) understood as a Container, cannot contain the objects observed by observers in the situation of analysis, because the object they are supposed to contain exceeds their own space and time. An observation is not a container but a probe. The similarity between Bion’s theory of observation and thinking to Dewey’s must not go unremarked. W. R Bion, “Container and Contained,” in W.R Bion, “A Theory of Thinking,” (1962) in E. Bott Spillius, ed., *Melanie Klein Today: Developments in theory and practice. Volume 1: Mainly Theory* (London: Routledge, 1988). I thank Jason Carpenter for his containment.

³⁴ On the relation of disagreement within “common worlds” I take the point from Luc Boltanski and Laurent Thévenot, *De la justification. Les économies de la grandeur* (Paris: Gallimard, 1991).

³⁵ Marilyn Strathern, *The Gender of the Gift* (Berkeley: University of California Press, 1988), 9.

³⁶ Marilyn Strathern, *Partial Connections* (Rowman & Littlefield, 1991).

Not only am I a situated subject, in the present, I am also a subject situated relative to the historical transformations that have produced the problematization, which I seek to inquire into. My situatedness relative to this past is the manner in which I sought out important characteristics, practices, and questions in that past. These past historical elements were not merely waiting to be unveiled, but rather had to be picked out, explicitly thematized and shaped relative to the problem I wanted to work on.

This methodological caution notwithstanding, I take up both projects in an anthropological manner, even if the latter project was not disciplinarily anthropological. By this I mean that I take up the STIR project as a practice of participation and observation, of self-observation of the human thing, relative to a practice whose objects can be inquired into with respect to their cultural significance. This is, therefore, not ethnography, but rather, an inquiry into the relation of modes of participant-observation, biological invention and the ethical questions which arise from observation of this practice.

What is at stake in the thesis, then, is a conceptualization and analysis of the practice of collaboration during fieldwork in these sets of experiments, within SynBERC/Human Practices and STIR/ASU/D-BSSE. These interconnected and collaborative projects and experiences were designed as an investigation into problems: what are the ways of forming a practice of inquiry into the ethics of biology and emerging technologies? In what way is this practice of participant-observation ethical and anthropological?

The Contemporary: Pathways to Problematization.

In our work in Human Practices, we made use of a heuristic for inquiry that we called a pathway.³⁷ It sought to connect isolated elements, chosen for their significance relative to a problem, and sought to interconnect them in a way that could make a problematization in the contemporary visible. Unlike genealogical strategies in the mode of history of the present, which aim to destroy the primacy of origins and of unchanging truths, pathways were developed so as to seek out some constitutive elements of problematizations. Unlike a historical-philosophic practice of problematization, which would seek to recover from throughout historical periods the core elements of how problems of ethics, politics and science

³⁷ “Our” work, is literally, work we did *together*. I will not offer “ethnographic description” of this labor, but the point nevertheless needs to be made, that we used tools, in a setting to think and work together. These were very simple tools: An office, a projector, comfortable seats which were ergonomically designed, a screen and a computer. These tools enabled us to work together, simultaneously, on the production of text. We, in different combinations, of the triad that made up the Human Practices subject position, did this on a regular basis for six years. Perhaps most fundamentally, these techniques were an essential component for collaborative work, in that they made possible a form of de- and re- subjectivation.

have made available differing subjectivational, jurisdictional and veridictional responses, our anthropological approach was delimited by the “contemporary.”

Rabinow has been exploring the term, “the contemporary” for a number of years, as a contrast term both to the present and to the modern. By ‘modern,’ I mean a form of consciousness which is aware of its historicity and projects the experience of this self-awareness into a *telos*. The contemporary, as I came to understand it, is agnostic relative to the question of historical consciousness, especially if one considers philosophy of history as the apex of modern historical consciousness and further that all philosophies of history are “gnostic.” By “gnostic” I mean that salvation—being saved—occurs through a privileged knowledge.³⁸ It is particular to philosophies of history, as one quintessential modality of ‘modern consciousness,’ that via knowledge, a future salvation is possible. The other major modality of modern consciousness, is what might be called ‘research,’ or as Martin Heidegger disparagingly put it, the ‘busy work’ of the merely ontic.

In either case, there is a core identity to the status of knowledge within the modern. As Foucault began his electrifying lectures in 1981, “... the history of truth enters its modern period, when it is assumed that what gives access to the truth, the condition for the subject’s access to truth, is knowledge and knowledge alone.”³⁹ So on the one hand, we have dialectical philosophy of history, of which Hayo Krombach has written; “the first and most basic philosophical point about dialectic is that it is, strictly speaking, not a method which we can apply like a tool from without to the domestic or international world in which we live. Rather, the whole of the socio-historical world, that is, the lived actuality of humanity is, within itself, and in a dialectical sense, methodically structured.”⁴⁰ On the other hand, there is ‘research,’ which has the same core status of knowledge, only without a salvific *telos*. Foucault guides us in indicating that knowledge, as the means to truth, once we enter this modern scene,

“will simply open out onto the indefinite dimension of progress, the end of which is unknown and the advantage of which will only ever be realized in the course of history by the institutional accumulation of bodies of knowledge, or the psychological or social benefits to be had from having

³⁸ C.f. Eric Voegelin, *Science, Politics and Gnosticism* (Washington: Regnery, 1968).

³⁹ Michel Foucault, *The Hermeneutics of the Subject: Lectures at the Collège de France 1981–1982* (Palgrave Macmillan, 2005), 17.

⁴⁰ Hayo B. E. D. Krombach, *Hegelian Reflections on the Idea of Nuclear War: Dialectical Thinking and the Dialectic of Mankind* (Palgrave Macmillan, 1991), 420, italics mine.

discovered the truth after having taken such pains to do so. As such, henceforth the truth cannot save the subject.”⁴¹

Rabinow sought to find other modes, forms and practices to take up the ethos, and not the epoch of modernity. It required a refusal of both a future salvation in terms of a privileged (socio-historical and prophetic) knowledge, and a refusal of the impossibility of practices of knowing being tied to “a pro-active taking care of, guarding, perhaps nourishing, the goods of one’s life, material and spiritual.”⁴² Such “taking up” of an ethos of knowledge and care, requires intellectual means to disaggregate these practices, as historically situated elements, so as to ask, how are they being re-made? To which ends? How to make a judgment about them? It is this orientation to the near future, rather than an epochal far future of a sublated dialectic, or a lost past, a golden age of nostalgia, that marks this anthropological mode.

To paraphrase Reinhart Koselleck, this conceptual work on “the contemporary” gives a form to different “spaces of experience” and “horizons of expectation.” If one does not take the forever-modern-time as the measure by which all phenomena are judged, and if one does not have faith in the dialectical structuring of the human thing’s lived actuality which is accessible to reason (the question of revelation remains) then in that case a different mode will have to be sought for inquiry into the phenomena of the world and the human thing’s relation to them.

Node 1. Collaboration: Biology, Ethics, Anthropology.

Inquiry, following John Dewey, begins with an indetermination, which calls for the specification of a problem to be worked on and rendered more determinate. The initial indetermination for this inquiry was the result of a contingent set of affairs. In 2006, after joining the graduate program in anthropology at Berkeley, I was invited by my advisor Paul Rabinow, to engage in this research endeavor called Human Practices in SynBERC. The indetermination was how to do it and, perhaps more fundamentally, what such participation should aim toward. Such an indetermination has, therefore, three vectors; the anthropological, the biological and the ethical. With respect to the anthropological vector, Rabinow had been an observer of the developing biosciences since the late 1980s in the US, France and Iceland.⁴³ He has assiduously inquired into the developments of research, forms of

⁴¹ Michel Foucault, *The Hermeneutics of the Subject: Lectures at the Collège de France 1981–1982* (Palgrave Macmillan, 2005).

⁴² Paul Rabinow, *The Accompaniment* (Chicago: University of Chicago Press, 2011).

⁴³ *Making PCR. A Story of Biotechnology* (Chicago: University of Chicago Press, 1996); *Essays in the Anthropology of Reason* (Princeton, New Jersey: Princeton University Press, 1997); *French DNA: Trouble in Purgatory* (University of Chicago Press, 1999); *A Machine to Make a Future: Biotech Chronicles*, with Talia Dan-Cohen, 2nd

life and emerging practices. Early in the new millennium, he was in a moment of his vocational pursuit, in which he was developing a set of conceptual tools to further a mode, form and practice of anthropological inquiry into problems of knowledge in the contemporary,

“to use them as a starting point to advance an experimental mode for the human sciences in which concepts and techniques could be made to function differently. By differently I mean better. By better, I mean in a more sagacious manner. By a more sagacious manner, I mean a wiser one ...”⁴⁴

This practice was part of an on-going question in anthropology as to how to develop an anthropological practice appropriate to fast moving, emergent, indeterminate objects of knowledge, such as the biosciences.⁴⁵

With respect to the ethical vector, Bennett had been a staff researcher for the Geron Corporation on their ethics advisory board in the late 1990s. Geron was one of the companies funding much of the early work in Embryonic Stem cells. The company brought together ethicists and biologists to meet a year before they derived the cells, to think in advance about what some of the repercussions might be. The downside to the arrangement for the ethicists with regards to the scientific work was that their capacity was purely advisory.⁴⁶ Having experienced the limits of the institutionalized role of the bioethicist, Bennett sought out intellectual resources in theology to think through these limitations. In theology, one of the limitations was that reasoned discourses about the divine, in academic venues, was removed from actual situations in which one might want to put thinking to work. Bennett sought out Rabinow with the aim of thinking through, anthropologically, the relation of biology, ethics and situated problems.⁴⁷

The biological vector was the substrate as well as efficient cause. In the wake of the Genome Sequencing projects, genomes are now sequenced, annotated

revised edition, (Princeton University Press, 2006. orig. 2004); *Marking Time: On the Anthropology of the Contemporary* (Princeton: Princeton University Press, 2007).

⁴⁴ Paul Rabinow, *Anthropos Today: Reflections on Modern Equipment* (Princeton, Princeton University Press, 2001).

⁴⁵ Paul Rabinow, George E Marcus, James, D. Faubion, and Tobias Rees, *Designs for an Anthropology of the Contemporary* (Durham, NC: Duke University Press, 2008.)

⁴⁶ Paul Rabinow and Gaymon Bennett, “From Bioethics to Human Practices, or Assembling Contemporary Equipment,” in *Tactical Biopolitics: Art, Activism, and Technoscience*, eds. Beatriz da Costa and Kavita Philips (Cambridge: MIT Press, 2007). Paul Rabinow and Gaymon Bennett, “Human Practices: Interfacing Three Modes of Collaboration,” in *The Ethics of Protocells: Moral and Social Implications of Creating Life in the Laboratory*, eds. Mark A. Bedau and Emily C. Parke (Cambridge: MIT Press, 2008).

⁴⁷ Ted Peter and Gaymon Bennett, eds., *Bridging Science and Religion* (Minneapolis: First Fortress Press, 2003).

Ted Peters, Karen Labacqs, and Gaymon Bennett, eds., *Sacred cells?: why Christians should support stem cell research* (Maryland: Rowman and Littlefield, 2008).

and compared. In the first half of the first decade of the new millennium, several groups of biologists, engineers, chemists and others were developing further means to break down genomes into units and functions and rebuild them in a more designed fashion. They called this endeavor, synthetic biology. This research endeavor was supported by federal funds and just as with the Human Genome Project, dedicated research for thinking through the “ethics and social implications” was included. What is important to recognize at this point is that Rabinow was invited to fulfill this function with respect to the bioengineering center in Berkeley, he sought to do this work in a collaborative and remediated anthropological fashion of participation and observation of the emerging biosciences, with Bennett, who wished to remediate his experience of how ethics could be practiced with respect to the biosciences.

My reason for wishing to enter the graduate program in anthropology at Berkeley was very specific. In 2004 whilst a visiting professor at the then recently opened BIOS Center of the London School of Economics, Rabinow gave a lecture titled “Genome, Risk, Care: On the Legitimacy of the Contemporary.” The lecture was a catalyst for me, as an undergraduate at the LSE, relative to two stultifying aspects of the research and study there: the seeming impossibility of posing questions of ethics within a social science institution, in anything more than the narrow sense of ‘bioethics’ (focused on limits) and the lack of a space in which to do common conceptual work on the intensification of relations between science, ethics and politics. Rabinow was working on both problems. He had at that time recently begun a collaborative endeavor called “the Anthropology of the Contemporary Research Collaboratory” (ARC) with several former graduate students, chiefly Andrew Lakoff and Stephen Collier. Their endeavor was to address a timely problem: how to work collaboratively in the human sciences on complex problems with the assumptions that the significance of those problems exceed any single project and that the significance of the problem cannot rest solely on the fetish and authority of field experience. The suggestion was that *problems* for anthropological participant-observation could be worked on better by collaboration.

The motivation for ARC and the problem to which it was a response was dissatisfaction with the individual *project* model in anthropology. As Collier, Lakoff and Rabinow wrote in 2006:

“The individual project model assumes that interpretive and authorial virtuosity is the mainspring of good work. At its best, it produces genuinely innovative and original scholarship. At its worst, it results in workshops, conference papers, collected volumes and monographs in which the emphasis is placed on individual performance, and in which there is not

much discussion or debate about what the key problems for the field are, and how to best approach them, nor is there evidence of shared norms that lead to better understanding of significant phenomena.”⁴⁸

Otherwise said, it was a problem of science, inquiry, ethics and the relation of these three terms to a practice of anthropology as one among the human sciences. For Collier, Lakoff and Rabinow, as they framed it, the question to which the Collaboratory was a response was the post-fieldwork question of what the relation is between individually produced knowledge and a broader set of problems pertinent to a more general field of knowledge. These problems are distinct from although to a degree require fieldwork.

In anthropology, as in many other disciplines, scholars work and write together. The impetus for ARC, however, was an insight into the need for an organizational form, mode and practice in which two things could be facilitated, which are not currently facilitated by the structure and experience of graduate student and professorial subject positions. First, there was a recognition of the need for collective work on concept formation for use in orienting work, decomposing and recomposing data. Second, there was reflection on a need for the formation of shared standards of judgment. The central methodological drive behind this effort was to develop ways of subjecting anthropological inquiry (and the subjectivation of anthropologists) to minimal ‘tests’ so as to be capable of discussing criteria of significance for knowledge sought or produced. What distinguishes this mode of work from a method proper is that the aim was not a fixed criterion relative to which the status of all knowledge produced by participant-observation could be judged. Rather, the aim was to subject the form of life devoted to knowledge about *anthropos* and knowledge about this creature’s *logoi* to examination. The purpose of such examination was to connect thought, as a practice, to an ethos of thinking.⁴⁹

Pathway Node 2. ELSI.

Both Human Practices and STIR, had as their background context and contrast the apparatus of bioethics oriented by the Ethical, Legal Social Implications (ELSI) model of the Human Genome Project and the ‘lab study’

⁴⁸ Stephen Collier, Andrew Lakoff and Paul Rabinow, “What is a Laboratory in the Human Sciences?” *ARC Working Paper* No. 1 (2006): 1.

⁴⁹ Michel Foucault, *The Hermeneutics of the Subject: Lectures at the Collège de France 1981–1982* (Palgrave Macmillan, 2005), 331-337. Paul Rabinow, *Anthropos Today: Reflections on Modern Equipment* (Princeton: Princeton University Press, 2003), 1-12.

approach of one vein of science and technology studies (STS).⁵⁰ It was in response to both bioethics and STS that these two modes of engagement sought to repose the question of how human scientists can work with the emerging scientists on the range of questions which emerge from such emergent and on-going research.

Briefly stated, the critical limitation of ELSI, which many scholars have analyzed, is that ELSI research was positioned “downstream” and “external” to the practice of research.⁵¹ The jurisdictional limitation of ELSI, the limitation of how its object of inquiry and intervention was constituted, was that the authority of ELSI researchers was circumscribed by their position outside of the scientific research. ELSI research was advisory and was limited to pointing out “issues.” These limitations produced several responses as to how human science researchers might better design “upstream” and “midstream” engagement with natural science and engineering research. “Upstream” means deliberation prior to the commencement of projects, and “midstream” refers to the effort to introduce questions during ongoing research. Both Human Practices and STIR were responses to this challenge.

A starting point for our inquiry in Human Practices was how to give form to a relation between those researchers developing a designed approach to making engineering use of cells and DNAs and ourselves, the human scientists, who were reflecting on the relationship between what is being made and the kind of ethical reflection appropriate to such knowing and making. Inquiry into this relation required a recognition that such work in Human Practices was part of a tradition of thought and intellectual practice including anthropology, ethics, philosophy and theology. Our inquiry posed the question of how these traditions, along with developments in the biosciences, were being re-assembled into a common problem in the present, a problem we designated with the name Human Practices.

Node 3. Biopower and Beyond.

The collaboration in Human Practices began with discussions over the limits of the concept of biopower for studying these problems relative to the emerging biosciences and what an outside to biopolitical approaches might be if one were oriented to the ethical stakes of such work. Briefly put, the concept of biopower developed by Michel Foucault articulates the historical emergence of a form of power over life. As he put it in the first volume of *The History of Sexuality*,

⁵⁰ Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts*, (Princeton: Princeton University Press, 1986).

⁵¹ Erik Fisher, “Lessons Learned from the Ethical, Legal and Social Implications Program (ELSI): Planning Societal Implications Research for the National Nanotechnology Program,” *Technology in Society* Vol. 27, No. 3 (2005): 321-328.

“power would no longer be dealing simply with legal subjects over whom the ultimate dominion was death, but with living beings, and the mastery it would be able to exercise over them would have to be applied at the level of life itself.”⁵²

In *French Modern*, Rabinow describes how urban planning in nineteenth century France turned the figure of biopower into practical mechanisms, constructing “the planned city as a regulator of modern society.”⁵³ Foucault forged a path of analysis which took as its object a series of mechanisms which would become critical for such regulation;

“the mechanisms introduced by biopolitics include forecasts, statistical estimates, and overall measures. And their purpose is not to modify any given phenomenon as such, or to modify any given individual insofar as he is an individual, but, essentially, to intervene at the level at which these general phenomena are determined, to intervene at the level of their generality.”⁵⁴

What is crucial is that these mechanisms are working on a particular kind of object, the well-being of the population. Population, a term from the early 17th century, is an object known through, among other kinds of calculation, normal distributions and subject to interventions on its health and welfare.

The question for Rabinow and Bennett was how the ‘outside’ to this object, the population, and this ethical and ontological orientation, the normed population and its vitality, could be posed relative to developments in the biotechnical sciences. Given Bennett’s prior experience and their shared concerns with regards problems and modes of ethical inquiry, Rabinow invited him to be the associate director of Human Practices and to enroll in the graduate program in anthropology. It was an opportunity to try out a collaborative project both within the human sciences—since as a Principle Investigator Rabinow would have funds to support students—and between the human and natural sciences.

Overview.

In 2004, then, I had had the intuition that something different, interesting and important was going on in California, but it is certainly correct to say that I stumbled into this situation, and gratefully accepted an invitation to participate. For

⁵² Michel Foucault, *History of Sexuality Vol. 1: The Will to Knowledge* (London: Penguin, [1976] 1998), 142-143.

⁵³ Paul Rabinow, *French Modern* (Chicago: University of Chicago Press 1995 (1989, MIT Press), 12.

⁵⁴ Michel Foucault, *Society must be defended: Lectures at the Collège de France, 1975-1976* (New York: Picador), 246.

myself, I had to get up to speed on where this problem, of collaboration between the sciences, came from. What were the extant and available relational modes and practices between the human and biosciences such that something different needed to be invented? It is this question I take up in Chapter One, so as to specify the problem to which this work was a response. Hence, the orientation of Chapter One sets up the problem to which the projects in Berkeley and Arizona were a response. It traces the mid-20th Century articulation of the apparent gulf and conflict between the humanities and natural sciences. This gulf, for which I take as an anchor point CP Snow's commonplace of "the two cultures," has been the site of remediations and re-workings of this problematic relation in the fifty years since his diagnosis. Furthermore, times have changed. Whilst Snow may have disingenuously characterized the natural sciences in a subordinate position with respect to the arts and cultural sciences in the 1960s, no one today could doubt the veridictional and jurisdictional position of authority, as well as political support, from which the natural sciences, and particularly the biosciences, speak.

The transformation of this *mise-en-scène* in a longer durational aspect is the task of Chapter Two. In that chapter, I name some genealogical elements that I think are crucial for moving from our initial problem to what we will come to recognize as the problematic experiences of trying to collaborate on this problem. The elements, following Kenneth Burke's dramatic pentad, are taken up as a ratio of a scene relative to a purpose; I pick out genealogical elements which help me to ask how the problem of knowledge can be taken up as an ethical problem.⁵⁵ I delineate the rise of the modern university in Germany, along with its social and cultural transformations, and reorientations, in order to ask, what is the context in which knowledge work happens and for what purpose? It is here that I will pause on the question of education and use, given the rise of a kind of work called "research." It is relative to Foucault's diagnosis of the problematic relation of truth and subjectivity, as well as my initial problematic of science and ethics that I take up this 'scene' of research. In effect I ask, what is the scene which connects the institutional, organized, conditions of a practice of knowing, to a subject's relation with and capacity to engage in such a practice? In delineating such a scene I situate the venues in which I worked.

Chapter Three brings these venues into a common frame so as to specify the organizational differences between Human Practices and STIR. This chapter gives concrete orientation to how these projects were situated and the purposes which they were designed to effect.

⁵⁵ Kenneth Burke, *A Grammar of Motives* (University of California Press, Berkeley and Los Angeles, California [first published 1945] California edition, 1969).

Chapter Four takes up the question of how these bioscientists and engineers are subjects of an ethical domain. This requires a specification of their practice, its rationale and the topography in which it is situated. The difficulty and discordance of engaging with this group of bioscientists on this activity qua ethical activity is given an initial determination.

Chapter Five takes up a second instance of such discordance, which is further specified relative to the previous chapter as an externalization and neutralization of ethical complexity. This instance turns on the problem of ‘preparedness’ with respect to the unforeseen ramifications of biological technologies.

Chapter Six takes up my second mode of engagement, STIR. The chapter focuses on my use of the “midstream modulation” protocol, a dialogical mechanism for the self-correction of the system rationality of scientific inquiry. The chapter seeks to assess this attempted mode of system-correction in terms of the limits to methodological forms of reflexivity. Such a methodological form, in this case, attempts to integrate the values and norms of system-environments into the research-system. Such a form, however, fails to take either dispositions of bioscientific researchers into account, or the relation of the human science inquirer to their object of inquiry. This, I suggest, is highly problematic if one is seeking to inquire into the limits of such extant norms and values of scientific research.

In Chapter Seven I return to the question of the common problematization; how can groups of human scientists who wish to reflect on the modes, forms and practices of contemporary research in biology develop such an experimental mode so as to specify ethical problems emergent from the biological research? Such an endeavor would require the specification of a mode of judgment, and it this I seek to make.

Anthropology & Ethics.

A word must also be said at the outset about ethics. This thesis is an anthropological inquiry into ethics which takes as its object two modes of engagement which sought to work on the problem of collaboration between the human- and bio- sciences. Since the object to be known, is also a practice in which I was a participant, a practice of trying to forge collaborative relations between forms of knowledge, it is not only the case (following the philosopher John Dewey) that knowledge of an object was an objective, but that an ethical objective

was to be made an object of reflection.⁵⁶ This is important to note because it indicates the effort to ask how inquiry into ethics is also ethical inquiry.

In the last forty years anthropology has had a series of ethical turns; the relation of anthropology to colonialism, the status of human rights, etc.⁵⁷ This ethical self-consciousness over the decades developed along two main lines; one line is what could be called “moral positionalism” and the other, an anthropology of morals. The former, perhaps most visibly embodied in the work of Nancy Scheper-Hughes, expressed the moral obligations of anthropologists with respect to the work they conduct. These “moral obligations” are for the most part extensions of topics which emerged in the 1960s. A moral anthropology, by contrast takes up morals as an object of study. As Didier Fassin has written recently of such a moral anthropology which has morals for its object, that it “explores how societies ideologically and emotionally found their cultural distinction between good and evil, and how social agents concretely work out this separation in their everyday life.”⁵⁸

One of the concerns of this latter approach is to obviate the anthropologist merely producing (or reproducing) her own moral discourse, at the expense of a “critical analysis” of that moral discourse.⁵⁹ One major concern with respect to such a distinction between moral discourse as object of inquiry and critical analysis as mode of inquiry, is that it reproduces the problem of “culturalism,” and hence by ethical extension moral relativism. This problem of anthropology and ethics was diagnosed perspicaciously by Rabinow already in the early 80s.⁶⁰

In his essay “Humanism as Nihilism,” Rabinow asked, why, given anthropology’s traditional reflection on the Other as the object of inquiry, as a way of knowing difference (in values, etc.), which are constituted by a meaning making system of relations, has anthropology not escaped the leveling of meaningful differentiation, which is the definition of nihilism? How, in other words, can anthropology take ethics seriously? This problem was diagnosed as the result of a double “bracketing” with respect to the object of inquiry. The first bracketing, was the result of the shift in object from “Man” to “Culture.”⁶¹ To posit such objects,

⁵⁶ “The name objects will be reserved for subject-matter so far as it has been produced and ordered in settled form by means of inquiry; proleptically, objects are the objectives of inquiry.” John Dewey, *Logic: The Theory of Inquiry*, (New York: Henry Holt, 1938), 119.

⁵⁷ Dell Hymes, ed., *Reinventing Anthropology* (University of Michigan Press, 1972).

⁵⁸ Didier Fassin, “Beyond Good and Evil?: Questioning the Anthropological Discomfort with Morals,” *Anthropological Theory* (2008; 8): 334.

⁵⁹ *Ibid*, 339

⁶⁰ Carlo Caduff, *Anthropology's Ethics: Moral Positionalism, Cultural Relativism, and Critical Analysis*, *Anthropological Theory* (2011): 465.

⁶¹ See f.n. 6.

cultures, required the bracketing of truth. The truth of the existence of many cultures required a relativism of the truth content of those cultures. This prior truth is the “underlying universal boundary conditions of what it meant to be human.”⁶² The content of these boundaries can then be positioned relative to one another.

The second bracketing was the result of a further transformation in the object of anthropological inquiry. With respect to key figures in American anthropology such as Clifford Geertz, Rabinow diagnosed the shift in object as turning on the re-introduction of historicity and symbols. A historicized temporality was re-introduced into anthropological inquiry, not by way of the evolutionary program of Anglo-German anthropology, but rather by thinking through the historical specificity of the means by which meaning is made within these universal boundary conditions. The unit for such inquiry was the symbol, the key concept for narrowing the overly broad concept of “Culture,” which was the remainder from the first bracketing in American anthropology. Historically specific conditions for experience and action become the crucial parameters of how to produce knowledge about these cultures, oriented by the symbol concept.

The symbolic anthropologists argued against the underlying universal boundary conditions of the relativists, which were seen by these anthropologists as unspecifiable. In the place of this content-less universal condition, the human thing was placed once again within a developmental (but not evolutionary) account of the role of culture in the development of the human thing.⁶³ The cultural character of human action, for the symbolic anthropologist, became irreducible to any underlying universals. The way to analyze Cultures was thus through their symbols, which are concrete embodiments of ideas and judgments, and complexes of symbols are extrinsic sources of information. As such, the anthropological task is one of the translation of such symbolically constituted actions into the language of anthropology.⁶⁴ This then is the second bracketing, the seriousness of such symbols can be posited for the Other but only insofar as they are serious for them within their Culture.

There have been several responses to this predicament of culture. One is explicitly Foucauldian in inspiration, turning on the analysis and practices of

⁶² Paul Rabinow, “Humanism as Nihilism: The Bracketing of Truth and Seriousness in American Cultural Anthropology,” in *Social Science as Moral Inquiry*, eds., N. Haan, R. M. Bellah, P. Rabinow, and W. M. Sullivan, 52-75 (New York: Columbia University Press), re-printed in *The Accompaniment* (Chicago: University of Chicago Press, 2011).

⁶³ See, Clifford Geertz, “The Impact of the Concept of Culture on the Concept of Man,” in *The Interpretation of Cultures* (New York: Basic Books, 1973), 33-54.

⁶⁴ Godfrey Lienhardt, “Modes of Thought in Primitive Society,” *New Blackfriars* Volume 34, Issue 399 (June 1953): 269–277., Talal Asad, “The Concept of Cultural Translation in British Social Anthropology,” in *Writing Culture*, eds., James Clifford and George E. Marcus (Berkeley: University of California Press, 1986).

“modes of subjectivation,” but which evades the main problematic diagnosed by Rabinow. This response has two aspects to it; the first aspect, perhaps best exemplified by texts such as Saba Mahmood’s *The Politics of Piety*, turns on the identification and analysis of the mode of subjectivation of the subjects who constitute the object of inquiry. The mode of ethical subjectivation is what Foucault indicated as “the way in which an individual establishes his relation to the rule and recognizes himself as obligated to put it into practice.”⁶⁵ It constituted one parameter of his four-fold orientation to ethics as a domain of inquiry.

Such a practice of subjectivation is complemented analytically by an ethical substance, an object of ethical reflection, the end toward which such a practice moves or aims, and the exercises or *askēsis* of such a practice. This is a powerful analytic tool for showing ethical variability in terms of its concrete practice. One step further, and as its complement, such an analytic tool can be used to take up the inquirer’s mode of subjectivation relative to the object of inquiry. Nevertheless, for an anthropology of ethics, the mode of subjectivation alone, whilst it offers a conduit out of the silo of Culture, is not adequate to an analysis of the complexity of the ethical field, a complexity which must tarry with Rabinow’s diagnosis of the problem of the practice of truth and seriousness, and of judgment, within anthropology.

Ethicality and Themitality: Breakdown and Reproduction.

James Faubion has developed a systematic response to the impasse of positionalism and relativism, by way of an extension and re-working of Foucault’s parameters for inquiry into the ethical field.⁶⁶ Faubion asks two questions which are crucial to any anthropology of ethics: how does one become a subject of a specifically ethical domain? What would an anthropology of this domain, and not only this subject, consist in? As indicated, in the last decades, the discipline of anthropology has turned toward questions of ethical life and practice with renewed vigor, not least thanks to the influence of Foucault and his historically located diagnostic specification of the parameters which constitute a subject as “ethical.”⁶⁷

⁶⁵ Michel Foucault, *The Use of Pleasure: The History of Sexuality Vol 2*, trans., by Robert Hurley (Penguin London, 1992), 27.

⁶⁶ James Faubion, *An Anthropology of Ethics* (Cambridge: Cambridge University Press, 2011). See also, Anthony Stavrianakis, “After Subjectivation: Homeostasis and Change in Ethical Domains,” *Current Anthropology*, Forthcoming.

⁶⁷ Paul Rabinow, “Introduction: The History of Systems of Thought,” in *Essential Works of Foucault (1954-1984)*, Volume 1: Ethics (Essential Works of Foucault (1954-1984), eds., Paul Rabinow (New York: New Press, 1994). Asad Talal, *Genealogies of Religion: Discipline and Reasons of Power in Christianity and Islam* (Baltimore: Johns Hopkins University Press, 1993). James Faubion, “Toward an Anthropology of Ethics: Foucault and the Pedagogies of Autopoiesis,” *Representations* Vol 74 (2001): 83-104. James Laidlaw, “For an Anthropology of Ethics and Freedom,” *The Journal of the Royal Anthropological Institute* Vol. 8, No. 2 (2002): 311-332. Saba Mahmood, *The*

This historical location was ancient Greece and sexuality was Foucault's object of reflection for the work of becoming a subject of an ethically parameterized "position."⁶⁸ Such a subject position is specifiable in its ethical determination by way of what he called an "ethical substance."⁶⁹ Such a substance is the object of conscious self-reflection by and through a subject of freedom.

As Faubion indicates, Foucault's formulation may smack of Hegel and the condition of *Sittlichkeit*;⁷⁰ ethical substance, however, is no mere re-articulation of the conditions of the embodiment of the principles of right conduct.⁷¹ The condition of *Sittlichkeit*, like the Aristotelian *ethos*, assumes the model of the character of what one has either come to be or should be.⁷²

As will become clear, collaboration is the ethical substance on which I am reflecting and on which I worked. Specifically, this indicates that there is not a model of collaboration to enact or perform. Rather, following Foucault's further parameters, I pose the question of the work to be done on such a substance—and its possible success or failure—as an open one, rather than as a norm necessarily given or as an accomplished fact.⁷³ Which *teloi* are such subjects striving toward? What practice or training (*áskēsis*) has to be done on the subject by the subject? How do subjects recognize that they have obligations and can go about practicing them (what Foucault called subjectivation)?

For Faubion, whilst Foucault's parameters are generative, they are not adequate to an analysis of the "complexity" of the ethical field.⁷⁴ Such complexity is characteristic of the situation in which "the occupant of one or another ethically marked subject position finds himself or herself or itself to be yet a second ethical

Politics of Piety: The Islamic Revival and the Feminist Subject (New Jersey: Princeton University Press, 2005). Michael Lambek, "Value and Virtue," *Anthropological Theory* 8 (2008): 133-157.

⁶⁸ Michel Foucault, *History of Sexuality Vol. 2: The Use of Pleasure* (Westminster, MD: Knopf Doubleday, 1985); *History of Sexuality, Vol. 3: The Care of the Self* (Pantheon Books, New York, NY, 1986).

⁶⁹ Foucault, *The Use of Pleasure*, 26-27.

⁷⁰ Faubion, *Anthropology of Ethics*, 21-38.

⁷¹ Georg W. F. Hegel, *Elements of the Philosophy of Right*, ed., Allen W. Wood, trans., H. B. Nisbet (Cambridge: Cambridge University Press, 1991), C.f. Part Three.

⁷² The "tension" over the question of the best form of life notwithstanding, whether the life of practical wisdom or the life of contemplation is best, see Aristotle, *Nicomachean Ethics*, 1097b: 9-16 for an account of such a model: "[T]he complete good [i.e., happiness] seems to be self-sufficient. Now what we count as self-sufficient is not what suffices for a solitary person by himself, living an isolated life, but what suffices also for parents, children, wife and in general for friends and fellow-citizens, since a human being is a naturally political [animal]....[W]e regard something as self-sufficient when all by itself it makes a life choice-worthy and lacking nothing; and that is what we think happiness does."

⁷³ Faubion, *Anthropology of Ethics*, 39.

⁷⁴ Tobias Rees, "As if "Theory" is the Only Form of Thinking, and "Social Theory" the Only Form of Critique: Thoughts on an Anthropology BST (Beyond Society and Theory)," *Dialectical Anthropology* Vol. 35, No. 3 (2011): 341-365.

subject.”⁷⁵ Faubion’s extension and reform of Foucault’s parameters of inquiry pays attention to the fact that whilst specific subject positions can be adequately inquired into by way of Foucault’s parameters, in order to inquire into the reproducibility and change, harmonization and conflict of subject positions, one needs to have an account of the ethical field in which a plurality of ethically marked positions exist. Such plurality indicates, first of all, that such subject positions are not individual and that individuals can be subject to multiple positions. Furthermore, such plurality demands an account of the field of their co-existence, reproducibility and transformation.

Of interest with respect to the STIR project is that Faubion’s model-theoretic and diagnostic intervention into the anthropology of ethics depends on two distinctions. The first is a systems-theoretic distinction between a system and its environment, enabling him to revise Foucault’s four-fold and its attention to subjects-in-the-making, whilst maintaining an orientation to the “homeostatic,” systematic and reproductive character of the ethical domain. As he writes,

“Foucault’s approach is not identical to but still compatible with a systems-theoretic framework grounded in the distinction between an organized process capable of reproducing or rearticulating its organization in something longer than the shortest of short runs and the environment or environments in which it does so”⁷⁶

This systems theoretic approach offers a generative conceptual orientation for indicating a basic parameter of “life,” which Faubion follows Foucault in following Canguilhem in identifying as; “the maladaptive mismatch between the demands of the organism and the demands of its environments.”⁷⁷

A systems theoretic frame, which builds on the work of German sociologist Niklas Luhmann, is thus his device through which to take up ethical discourses and treat them as “distinctive semiotic fields.” To do so, as he puts it, avoids presupposing the very conclusions that should be the product of inquiry. This approach allows him to ask, “what ethical discourse distinctively communicates and what ethical action distinctively effects”?⁷⁸

The STIR project itself depends on a conceptual distinction between the ‘socio-technical’ system and its environments, which will include incitements to reflect on the norms, values and virtues of the milieus in which those systems are embedded.

⁷⁵ Faubion, *Anthropology of Ethics*, 14.

⁷⁶ *Ibid*, 5.

⁷⁷ *Ibid*.

⁷⁸ *Ibid*, 10.

Faubion hones a further distinction, after that of the system and its environments, between “the more ecological and dynamic and the more homeostatic aspects of ethical autopoiesis.”⁷⁹ He names the difference between them as “the ethical” and “the themitical.” The ethical is characterized by the transformation of the normative character of discourse and action, and the themitical—Faubion’s neologism from the Greek *themitos*, meaning, “that which is allowed by the gods”—by its stabilization and reproduction.

Faubion develops this second distinction, of the themitical within the ethical, against a supposition of systems theory of a systems’ closure from its environment. Undoing such a supposition puts in question what has come to be understood as the mutual contradiction of the dynamic and the homeostatic characters of autopoiesis.

Whilst acknowledging their distinction, Faubion wishes to use such distinction in order to inquire into the constitutive dynamic of the ethical reformulation of codes and practices and the homeostatic reproduction of such codes and practices. This point is echoed, as we will see, in the orientation of the STIR project, insofar as the possibility of ‘self-changing’ and of ‘change from within’ a system is posited as non-contradictory with the reproduction of norms and values, or the transformation of broader norms and values of the milieus in which such systems operate.

In accord with anthropologists such as Jarrett Zigon, Faubion agrees that the ethical field is often marked by moments of “the failure of the reproduction of the routine”;⁸⁰ he nevertheless, against Zigon, seeks to avoid the reduction of the ethical moment to that of breakdown.⁸¹ Faubion’s critique of the view that places the themitical as mutually contradictory with the ethical is that such proposed contradiction renders the former as unthinking habit and the latter as the “moment of reflection.” Such a stipulated exclusivity of the ethical from the themitical disconnects the subject’s relation to itself, as well as its virtues, from the norms and values of the subject’s milieu.

Faubion points out that the conceptual distinction as well as the intimate relation of the ethical and themitical indicates,

“the fundamental connection between the distinctive grounding of ethical discourse in a common semantic code and the distinctive programming of

⁷⁹ *Ibid*, 20.

⁸⁰ *Ibid*, 82.

⁸¹ Jarrett Zigon, *Moral Breakdown and the Ethical Demand: A Theoretical Framework for an Anthropology of Moralities*, in *Anthropological Theory* Vol.7, No.2 (2008).

that discourse, through one or another regimen of the justification of ethical evaluation.”⁸²

The key criterion for the relation between subjects in a domain which could be called ethical is that there is a demand of ethical regard, which specifies a *standard* for such regard and a mode of judgment. To put it in other words, an ethical field of relations has a certain semiotic and practical organization that are the conditions of its reproducibility. A subject must be capable of knowing to whom and how to extend a certain kind of valuation, attention and practice, to be able to reproduce it and furthermore to be able to put it in question. I take up this point “in situ” in Chapter One.

Following this diagnostic parameterization, we should acknowledge three things; if in addition to the subjectivational dimension of the ethical field, there is a need for the explication of the mode of judgment, this requires first of all the establishment of “other-regard.” Second of all, it requires a measure of significance, which Faubion indicates with reference to a meditation on Weber’s analysis of charismatic authority and its cognate, ‘chrism’; the becoming of this subject of ethical regard, which is often accompanied not only of the ‘act’ but the reflexive and pedagogical work of becoming this subject.⁸³ Finally, there is the establishment of subject positions in their reciprocal governmental relations.

The second indicator is very important. An anthropology of ethics, in Faubion’s working through and conceptual rendering, must take as a point of orientation that practices, and the way in which practices are spoken about, are subject to change, transformation and reproduction: who is regarded and how? How a subject is placed relative to another subject? How practices are evaluated, specifically, through their measures of worth?

Relative to the specific problem of this thesis, can reflection on the ‘ethical’ transform breakdowns or interruptions of the “themetical”? One of the major concerns in the thesis will be the manner in which “regard,” measures of worth and the constitutive relations between subject positions were problematic in the two modes of engagement I will describe and analyze. If I privilege breakdown, I do so now in full view of the durability and resistance of systemic autopoeisis to perturbation.

⁸² Faubion, *Anthropology of Ethics*, 85.

⁸³ A minor digression, this is one of several fundamental differences between the rite of “chrismation,” or “confirmation” in the Eastern Orthodox and Western Catholic Churches, whereby chrismation, the act of anointment in the East follows immediately after baptism, whereas in the Western Churches it is more common for the chrism to be partaken at the age of and with the work of reason.

Thus, in the account that follows, I will cast doubt as to whether there is a dialectical relation between the homeostatic and dynamic aspects of ethical autopoiesis, even in a weak form.⁸⁴ To jump to this diagnosis, however, would be to get ahead of myself. What I think is compelling about the above specification of a general problematic of anthropological inquiry into ethics with respect to my specific problem, is that it highlights the great difficulty in distinguishing between indeterminations over how to act and indetermination over evaluative predicates, or ways of judging.⁸⁵

Is it possible to distinguish ethical action or judgment from the analysis of ethical action or judgments? Following Alisdair MacIntyre's orientation in his *Brief History of Ethics*, such a distinction is possible only if the vocabulary of ethics can be taken as given and determinate.⁸⁶ In a situation where the very meaning of such vocabulary is either in doubt, or under-specified, the specification of the meaning of terms is partly constituted by previous meanings but also partly designed to try and clarify or rectify some aspect of the breakdown in the situation.

Subject Positions.

What I wish to show in this thesis are some parameters of the ethical field of human scientists engaging with bio-scientists and the conflict over, disagreement and work of thinking about the standards and ends which govern such a field of relations. The subject positions from which such engagement happened, was neither in Human Practices, nor in STIR, purely "individual." In Human Practices vis-à-vis the organization SynBERC, a collective subject position was made available through the high level conceptualization which oriented the project. This conceptualization, as I will describe, included taking collaboration as the ethical substance of such a position and naming a *telos* toward which it would move, such that one might enter the ethically marked position of such a subject.

Since collaboration was the substance of this position, by definition the occupancy of such a position was a relation. In the short-term, the relation was between three anthropologists (one of whom was also a theologian). It is the work of this subject position in the ethical field of the relation of sciences that is the object of this inquiry. As such, I will from time to time have recourse to the pronoun "we." The context of this "we" must always be borne in mind.

⁸⁴ Faubion suggests that a working postulate of the ethical domain is the weakly dialectical relationship of ethical value and thematical normativity. Faubion, *Anthropology of Ethics*, 114. This is echoed in Fisher's insistence on the STIR protocol as being a form of dialectics; Fisher, personal communication.

⁸⁵ Alisdair MacIntyre, *A Short History of Ethics* (New York: Publisher: Macmillan Pub Co 1966) p.13.

⁸⁶ *Ibid.*

It is the relation to a second subject position, my position within STIR, which will make available a larger terrain of comparison from which to observe this ethical domain. What distinguishes this second subject position is that there was a very different form of work on oneself demanded to enter the subject position of the STIR project. The STIR studies were singular, insofar as I went alone to Basel Switzerland under the mandate of STIR and with a method to use; although even then, I will give an account of the STIR project which highlights my indeterminate and problematic (in a neutral, non-pejorative sense) relation to the governor of the STIR project, Erik Fisher, which constituted my position vis-à-vis the work of STIR.

It was the use of the method which marked my position and constituted the work on myself to enter the subject position of the STIR project. Chapter Seven will take up the contrasting and problematic ends towards which the exercise of thinking in such subject positions-in-becoming could move.

Áskēsis.

Thinking, following Dewey, occurs in a situation of trouble and situations of trouble are not individual. The “only way out” from a situation of discordance, Dewey observes,

“is through careful inspection of the situation, involving resolution into elements, and a going out beyond what is found upon such inspection to be given, to something else to get a leverage for understanding it. That is we have (a) to locate the difficulty, and (b) to devise a method for coping with it. Any such way of looking at thinking demands moreover that the difficulty be located *in* the situation in question.”⁸⁷

The orientation to the work in Human Practices was explicitly ‘pragmatic’ in its approach in this Deweyan sense. Implicitly, the work in STIR was similarly orientated; an attempt to meet the demands of a situation with respect to a certain localized difficulty and the attempt to devise a ‘method’ to cope with it. Such orientation is to a practice of inquiry that took up Dewey’s demand to put “intellectual instrumentalities” to work in situations which had been diagnosed as problematic. One difficulty, is that Dewey is vague on what he means when he says that inquiry takes place in ‘situations’ and how one would be able to diagnose that there is in fact a situation in need of reconstruction.⁸⁸ This will also constitute a key part of the difference between STIR and Human Practices.

⁸⁷ John Dewey, *Essays on Experimental Logic* (Chicago: University of Chicago Press, 1916), 12.

⁸⁸ Rabinow, *Marking Time: On the Anthropology of the Contemporary* (Princeton: Princeton University Press, 2007), 7.

Dewey is however clear on one indeterminate situation in particular which he names in his 1948 Introduction to *Reconstruction in Philosophy*, and offers a way to conclude this orienting Introduction:

“the entrance into the conduct of the everyday affairs of life of processes, materials and interests whose origin lies in the work done by physical inquirers in the relatively aloof and remote technical workshops known as laboratories.”⁸⁹

The indetermination, for Dewey, comes from the incapacity to inquire into and reflect on the effects of the increase in technical means. For Dewey the consequence of this incapacity to develop an adequate practice of inquiry is “a compromise taking the form of a division of fields and jurisdictions.”⁹⁰ This division is between the material and the ethical. The problem of those situations in which this division is instantiated, is the failure to subject our common institutions, which involve ever more elaborate technical control of matter, and the habits and morals underlying them, to inquiry. If one were to do this inquiry, Dewey explains, one would see both logically and morally that the invention of new means does more than alter the ease of achieving the ends we think we know.

From my position, as novice participant and observer, there were two important aspects to the work between Rabinow and Bennett, which oriented this thesis work and which was fundamental to my orientation to the biology under investigation, and the two teams of human scientists in which I participated; the first was that collaborative work be problem oriented and experimental. The second was that this experimentation be oriented to an ethical end, which Rabinow and Bennett had named from the start as “flourishing” a translation of the Greek term *eudaemonia*.

Flourishing was a term we used to posit the reason for our mode of participant-observation. We used the term to ask how the ethical outsiders to the instrumental rationality of the sciences could be re-activated and reconnected to new practices of scientific inquiry. This is not to say that flourishing is per se opposed to instrumental goals, rather, we used the term to ask how measures broader than justifications by instrumentality could be introduced into seemingly emergent spaces of work in the biosciences. Relative to our interconnected projects, flourishing was an end toward which we were trying to work, through the activity of anthropological and ethical inquiry on the ramifications of bioscience and engineering.

⁸⁹ John Dewey, *Reconstruction in Philosophy*, enlarged edition, (Boston: Beacon Press, 1948), xiii.

⁹⁰ *Ibid.*

The thesis can be read, therefore, as a movement from an initial indetermination, a problem (of collaboration between the sciences) oriented by an ethical end, to a determination about the problem of shared standards for such ethical orientation in efforts of collaboration.

Chapter One

A Problem

“My problem is to see how men govern themselves and others by the production of truth. I repeat, once again, that by production of truth I mean not the production of true utterances, but the establishment of domains in which the practice of true and false can be made at once ordered and pertinent.”

–Michel Foucault. ⁹¹

How have physical sciences and technologies become an object of study for the human sciences in the last sixty years (1950-2010)? In what ways has this object been constituted as a problem? In this chapter I identify a problem of the purpose of knowledge that is embodied in a possible way of life.

Foucault used the term ‘focal points of experience’ (*foyer d’experience*), to highlight his efforts to analyze how “forms of a possible knowledge (*savoir*), normative frameworks of behavior for individuals, and potential modes of existence for possible subjects are linked together.” ⁹² Whist I take up only indirectly Foucault’s triad, I take forms, modes and norms of knowledge, life and the question of governance, of oneself and others, as broadly constituting the subjectivational, veridictional and jurisdictional parameters to follow in asking how ‘science’ became a focal point of experience for human scientific reflection in the recent past.

Otherwise said, Foucault’s three variables indicate the questions of how knowledge practices as an object of inquiry, can be taken up in terms of (1) its mode of producing and authorizing speech acts that are taken to be true and false (veridiction); (2) how the activities and the practices of ‘knowing’ are produced through a mode of existence (subjectivation); (3) the way in which a legitimate

⁹¹ Michel Foucault, “Questions of Method,” in *The Foucault Effect: Studies in Governmentality with Two Lectures by and an Interview with Michel Foucault*, eds., Graham Burchell, Colin Gordon, and Peter Miller (Chicago: University of Chicago Press, 1991).

⁹² Michel Foucault, *The Government of Self and Others: Lectures at the College de France 1982-1983* (New York: Picador, 2010), 3.

field of objects is conducted, including the conduct of such conduct (jurisdiction).
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With respect to these variables, how have normative frames of the purpose and conduct of science developed in the post-war period? I take what I think is an exemplary moment of breakdown in the relations between the human and natural sciences as an anchor point for this subjectivational, veridictional and jurisdictional topic. This moment is the 1960s and the commonplace I take up is that of “The Two Cultures”; the idea that different forms of knowledge, broadly cultural and physical, are antagonistically opposed.

I begin by identifying what I think is at stake in this breakdown, which is the ways of being for possible subjects of knowledge, in which the questions posed are: what is the purpose of knowledge? What stance can one take to the knowledge one wishes to produce? These questions, with respect to the commonplace of conflict between ‘letters and sciences,’ are in the present.⁹⁴ At the heart of this breakdown between natural scientific and cultural inquiry is the question of the standards of judgment, the purpose and form of life of inquiry, broadly conceived as any scientific or educational endeavor.

By identifying different veridictional modes with respect to the conduct and content of science, different normative frames for such conduct of scientific practice and the ways of being that can care for a purpose of knowing, I am setting up how I take up two human science projects, Human Practices and STIR, as responses to these differing vectors of the problem of science. I conclude by orienting STIR and Human Practices with respect to the preceding discussion.

Modes of Veridiction.

A mode of veridiction is the authorized manner in which a statement can get to count as true, or to use John Dewey’s term, ‘warranted.’ A mode of veridiction is the manner in which statements about an object of inquiry get taken seriously by others. Following Dewey, I take it that if one is trying to produce knowledge, in this case about how a particular area of knowledge is produced, “something of the order of a theory or hypothesis, a meaning entertained as a *possible significance* in some actual case, is demanded, if there is to be *warranted* assertibility in the case

⁹³ This is of course what he has elsewhere called “governmentality,” on which there is an enormous secondary literature, largely not relevant for my purposes. Much less frequently attempted has been to trace other possible foci of experience in which veridictional, jurisdictional, and subjectivational forms, norms and modes have coalesced.

⁹⁴ For an alternative way of ‘sublating’ the problem of different stances to purpose of knowing in the human and natural sciences, see Bruno Latour, *Pandora’s Hope: Essays on the Reality of Science Studies* (Cambridge: Harvard University Press, 1999).

of a particular matter of fact.”⁹⁵ Dewey stipulates something very important, which bears resemblance to the epigram from Foucault quoted above;

“This position undoubtedly gives an importance to ideas (theories, hypotheses) ... But it is not a position that can be put in opposition to assertions about matters of particular fact, since, in terms of my view, it states the *conditions* under which we reach warranted assertibility about particular matters of fact. There is nothing peculiarly “pragmatic” about this part of my view, which holds that the presence of an idea—defined as a possible significance of an existent something—is required for any assertion entitled to rank as knowledge or as true; the insistence, however, that the “presence” be by way of an existential operation demarcates it from most other such theories.”⁹⁶

It is this ‘existential operation’ that I seek to specify through Foucault’s ‘focal points.’ I see this existential operation in Snow and Leavis’ engagement. It is by insisting on the presence of such an operation, that I outline the question: How is it that a possible significance is named so as to order what can count as serious in statements about science?

In this chapter I go back to May 1959, to a moment that became pivotal in the “science versus humanities” commonplace; the spirited conflict between CP Snow and FR Leavis. What struck me as important in their intellectual collision is that this was perhaps the closest genealogical anchor for the theme and problem I named in the Introduction.

I am therefore returning to a moment contemporary with the publication of Thomas Kuhn’s *The Structure of Scientific Revolutions* and I seek to open up a different connection between the past and the future relation of the sciences and humanities, different to the one actualized by Kuhn’s work.⁹⁷ I will save an exploration of this point for future work and justify my choice of returning to the relation of Snow and Leavis solely through the story we can narrate through the opposition of their forms of life, their modes of veridiction and the manner in which they think a relation between knowledge and life should be governed.

⁹⁵ John Dewey, “Warranted Assertibility, and Truth,” *The Journal of Philosophy* Vol. 38, No. 7 (Mar. 27, 1941): 169-186, 170 Italics in original.

⁹⁶ *Ibid.*

⁹⁷ See also, Steven Fuller *Thomas Kuhn: A Philosophical History for our Time* (Chicago: University of Chicago Press, 2000). On the normativity of Kuhn’s line of thinking on what became science studies, see “Kuhnification as Ritualized Political Impotence,” 318-378.

Stance.

In May 1959 the English novelist and physicist Charles Percy Snow delivered the Rede lecture, in Cambridge.⁹⁸ The content of the Rede lecture Snow characterized as giving voice to a spirit of the time. Snow wrote the following on reflection four years after the event:

“In our society we have lost even the pretense of a common culture. Persons educated with the greatest intensity we know can no longer communicate with one another on the plane of their major intellectual concerns. This is serious for our creative intellectual and above all, our normal life.”⁹⁹

The reason this was so problematic for Snow was that the breakdown in “common culture,” the increase in specializations and the lack of inter-literacy between the humanities and natural sciences forged a gulf between reflections on two conditions of existence: the ‘individual’ and the ‘social’ (his distinction). The problematic relationship (and gulf) between these kinds of reflection, he suggests, constituted a difficulty in 1959: This difficulty, in his view, was a consequence of the exercise of authority by the humanities in the academy and the ‘corridors of power.’¹⁰⁰ It was not simply fragmentation that was a problem, but rather what Snow saw as the cultural domination by reactionary literary types against the fruits of industry and science. This domination constituted a blockage point for bringing into the world the goods assured by the technical and instrumental rationality of the sciences.¹⁰¹

In the Rede Lecture, Snow makes the following argument: There is a mutually exclusive distinction between “arts and sciences,” in terms of their objectives. The sciences focus on the remediable things of life with the objective of practical intervention. The arts, which Snow references alternately as the “literary” and “traditional” culture, on the other hand, have as their content the irremediable condition of individual existence. Their objective, according to Snow, is contemplation.¹⁰² The arts, in his assessment, have never understood the benefits of science. They have remained blind to—or willfully ignorant of—the social history

⁹⁸ A little remarked fact is that the Rede lectures—after Sir Robert Rede Chief Justice of the Common Pleas – from 1668 to 1856 comprised lectures in rhetoric, logic and philosophy. By 1858 the series had fallen into desuetude. A new statute was drafted and its approval began a new series of a single annual lectureship appointed by the Vice-Chancellor of Cambridge. In 1959 the Vice-Chancellor was Sir Herbert Butterfield, an historian most known for his *Whig Interpretation of History*.

⁹⁹ C.P. Snow, “The Two Cultures: A Second Look,” in *The Two Cultures*, ed., Stefan Collini (Cambridge: Canto, 1993), 60.

¹⁰⁰ C.f. C.P. Snow, *Corridors of Power* (London: Macmillan, 1964).

¹⁰¹ Snow’s argument regarding the problematic relation of the two cultures was tied to the socialist political project of scientists such as JD Bernal: “Science” for Bernal was tied to a program of social planning that could be executed by science and technology. Technocracy, in Snow’s rendering, is an ethically superior socio-political project.

¹⁰² See below. I revisit this distinction and the question of contemplation, practical life and reflection.

and positive social effects of the industrial revolution. The “traditional” culture, in this assessment, is hostile to the future. His claim is that as with the industrial revolution, “the arts” (modernist literature in particular) are doing the same with the scientific revolution of the 20th Century.

The stance of those who embodied this ‘traditional’ form of thought and life was problematic for Snow because instead of intellectual and political authority being exercised by those who are able to inflect the social condition of life, it was those culturally oriented to the humanities, with a corresponding elite socio-economic position, who were filling the positions of power in the academy and government. “Traditional culture” had to be contested if the future Snow hoped for were to be brought into the world. This attack was to be made through—and would result in—the right kind of education. As he wrote in the Rede lecture,

“We have to educate ourselves or watch a steep decline in our lifetime.”¹⁰³

For Snow, the condition of individual existence is irremediable.¹⁰⁴ That is to say, it is the kind of thing over which the variables of decline and progress do not apply. It is the content of reflection of centuries of introspective thought, which he chose to characterize with Pascal’s “*on mourra seul*” (each of us dies alone.)¹⁰⁵ In Snow’s understanding, this tends to be the substance of reflection for those who have claimed for themselves the title of intellectuals, a term he used in the pejorative, rather than those professionals who create and work in the medium of knowledge.

In contrast to the individual condition of existence, the ‘social condition’ is remediable: The human thing has been categorized as both *Homo Sapiens* and *Homo Faber*. According to Snow, “we cannot avoid the realization that applied science has made it possible to remove unnecessary suffering from a billion individual lives.”¹⁰⁶ The scale and temporality at which *Homo Faber* operates is of exponential magnitude. Snow was giving one articulation of a mid-20th Century shift in power relations, problems and the manner in which those problems were to be tackled. For the sake of ‘mind,’ ‘nation,’ ‘civilization’ and ‘globe,’ in this ascending order, Snow wished to reverse priorities in British education. Snow was, furthermore, in a position to influence such reversal. Guy Ortolano, drawing on Boytinck’s, *C P. Snow. A Reference Guide* introduces him in the following manner:

¹⁰³ Snow, *The Two Cultures*.

¹⁰⁴ Snow, *The Two Cultures*, 76.

¹⁰⁵ Blaise Pascal, *Pensee*, Dezobry et E Madeleine, libraries-editeurs, 1852 Paris; p. 488

¹⁰⁶ Snow, *The Two Cultures*, 78

“C. P. Snow was at the peak of his public stature in the late 1950s and early 1960s. The son of a clerk in a Leicester shoe factory, by 1930 he had followed his interest in science to a fellowship at Christ's College, Cambridge. After suffering a setback in his research, Snow pursued dual careers as a scientific administrator and popular novelist. He was knighted in 1957 for his work during the Second World War as the Director of Technical Personnel in the Ministry of Labour, and in 1964 he accepted a peerage and a position in Wilson's new Labour government.”¹⁰⁷

Instrumental positions included serving as the scientific advisor to the Barlow Commission that recommended doubling the number of science graduates in the UK. The Barlow commission envisioned post-war reconstruction in terms of education along the axes of “expansion” and “democratization,” with “science in the driver’s seat.”¹⁰⁸ This is a brief sketch of the mid-20th Century position of an individual and his orientation to the problem of knowledge. We will discuss the broader institutional and global vectors of the scene in the chapter that follows. For the moment, what concerns us is that Snow embodied an argument and stance toward the vectors of knowledge and history. He characterized a disposition towards a problem and it is this I wish to focus on now through the reaction it engendered.

Snow’s Rede lecture of 1959 had its dialogical counterpoint only three years later, in the Richmond lecture of FR Leavis, English Don of Downing College, Cambridge. Historian Guy Ortolano takes up the Rede and Richmond lectures, the dozens of print responses and the discursive ramifications of the tone and terminology of the debate, as a series of episodes in a 20th Century struggle. The significance of the episode is taken up by Ortolano not as a disciplinary dispute, but rather as an ideological dispute internal to “competing visions of Britain’s past, present, and future.”¹⁰⁹

This episode cannot be read through Snow alone but rather in the dialogical relation between Snow and Leavis. Around these two figures rallied disciplinarily divergent collections of individuals. That is to say, the two cultures, of the two cultures debate, are in fact not cultures, in the classical anthropological sense referred to by Snow, in his reappraisal *The Two Cultures: A second look*. What is important to note is that the distinction made by Snow, between scientific and literary culture, is internal to his formulation of the problem of modernization.

¹⁰⁷ Guy Ortolano, “Two Cultures, One University: The Institutional Origins of the ‘Two Cultures,’” *Albion: A Quarterly Journal Concerned with British Studies* Vol. 34, No. 4 (Winter, 2002): 606-624, 607. Cf. Paul W. Boytinck, *C.P. Snow. A Reference Guide* (Prentice-Hall 1980), vii-viii.

¹⁰⁸ Ortolano, “Two Cultures, One University,” 619.

¹⁰⁹ *Ibid.*

What Leavis offers is a genuine counterpoint to the way in which Snow framed the problem and to the ethos it stood for.

Scrutiny.

In 1962, at the University of Cambridge, Leavis gave the Richmond Lecture, “*Two Culture? The Significance of CP Snow.*” According to MacKillop’s portrait of Leavis, the ‘science versus the humanities’ commonplace had emerged as a significant one in the two years after Snow’s lecture, particularly in applications by students for acceptance to the various colleges of Cambridge. According to MacKillop, two episodes occurred that catalyzed Leavis’ interest in Snow and his rendering of a conflict of the faculties. One episode occurred in the *New York Times* and the other concerned *Delta*, a poetry magazine produced at Cambridge in the 50s and 60s.

The former episode involved a piece by Angus Wilson writing in the *Times* in which he had written of the works of Leavis and Snow in a common frame. This frame specified both Snow and Leavis as writers who were writing within a realist mode against literary modernism. Leavis objected vehemently to being associated with someone he considered to be a literary fraud and part of the wave of ‘mass society’ that he had been writing against since his PhD thesis on *The Relationship of Journalism to Literature*.

Leavis continued his defense of his view of intellectual life begun in his thesis, in *Scrutiny*, the journal he had founded and edited from 1932 to 1953. Leavis’ co-editors Knights and Culver wrote in issue one:

“Scrutiny, then, will be seriously preoccupied with the movement of modern civilization. And if we add that it will direct itself especially upon educational matters the reader will realize that there may, after all, be a fairly close approach to practice.”¹¹⁰

The literary criticism and mode of thought Leavis sought to defend was against precisely the cultural force that Snow represented and had articulated as his counter point to scientific culture. For Leavis, Snow was the embodiment of the cultural problem and relation of, on the one hand, naïve faith in technocracy and on the other the simultaneous and complementary degraded intellectual standards of what passed for literary culture.

In his “*A Sketch for an English School,*” Leavis named what should be treated as the intellectual agonist of such cultural development. He formulated an approach to the examination of:

¹¹⁰ *Ibid*, 616.

“the problems of modern civilization with an understanding of their origins, a maturity of outlook, and, not a nostalgic addiction to the past, but a sense of human possibilities ... that traditional cultures bear witness to and that it would be disastrous to lose sight of for good.”¹¹¹

This commitment to the predicament of modernity is a necessary background to understand the second episode in the run up to his Richmond lecture. The *Spectator*, the right of center political and cultural magazine, rebuked *Delta*, for being ‘Leavisite,’ which apparently meant ‘pro-literary criticism’ and ‘anti-history.’¹¹² In a curious confluence of political left and right, both Snow’s left technocratic supporters (literary and scientific), and the right of center conservatives had rendered Leavis as ‘against’ history and therefore progress.

Leavis replied to the *Spectator*;

“How does one get access to the historical past?—that, surely, is the great problem. But to you [the *Spectator*] it is no problem.”¹¹³

Implicit in the Richmond lecture was that it was no problem for Snow either. This problem is what separates Leavis’ professed gravity from Snow’s naïveté. It is through this problem that Leavis sought to attack Snow’s own anti-historical cultural position and which furthermore attempted to combat the false dialectic he stood for. The Richmond lecture has, without exception to my knowledge, been rendered as a regrettable *ad hominem* attack; Snow was derided as a literary phony and his stature as a public intellectual undeserved. Attention to Leavis and this *ad hominem* attack shows us that the problem of the relation of the humanities or human sciences and the natural sciences is less about a contrast between disciplines, for instance Chemistry and English literature and more a conflict over an attitude, an embodied stance, to the present.

The Significance of Character.

Snow’s categories were underspecified, but as social facts they were significant; hence the subtitle of Leavis’ lecture and reply to Snow “*The significance of CP Snow.*” Snow’s stature and his standing were tied to the kind of argument and the mode his argument could be made in. The authority of Snow’s speech came from this stature; hence to critique the cultural significance of Snow’s speech, Leavis used Snow’s stature as the conduit. This was taken up by commentators as unacceptable and regrettable. This reading is itself regrettable, as

¹¹¹ *Ibid*, 617. FR, Leavis, *Education and the University: A Sketch for an 'English School'* (Cambridge University Press; 2nd edition, 1979), 113.

¹¹² Ian MacKillop, *F.R. Leavis: A Life in Criticism* (London: The Penguin Press, 1997).

¹¹³ *Ibid*; 311.

it does not take seriously Leavis' strategy or the seriousness with which he took to be the cultural significance of the adulation that Snow's original formation of the problem received.

Snow, as we saw, through his lecture and as one can read in his novels, was an exponent of a technocratic critique of British decline. The critique seeks to embolden what he claims is marginalized; science, technology and expertise. The irony is that he was able to make this supposed critique at the moment that a political commitment to these forms of knowledge flourished. This is what David Edgerton has called 'anti-history.' By anti-history, Edgerton means a genre of historical thought and presentation, which erases the subject and the actual conditions which allows that subject to say the things that they say.

Snow's lecture was 'anti-history,' because it fits into a genre of writing in which the activities of instrumental rationality are claimed as being contrary to the spirit of the time, which is necessary such that progress can be arrogated to those practitioners of instrumental rationality, who, Snow suggested have "the future in their bones."¹¹⁴

The failure of the present is then presented by Snow as the consequence of a dominant tendency which is against progress. The erasure in Snow's lecture is the articulation of a claim: what Britain, and then by ethical extension, the rest of the world, does not have enough of is the institutional, educational arrangements for the production scientific and technological solutions to problems of life. He was able to make this claim, from a significant cultural platform at the very moment in which such arrangements were being produced.

Snow's genre of writing was designed to efface the history of the rise of technology in the UK. Snow's lecture was precisely so emblematic and discursively catalytic, however, because the late 1950s was in fact a technocratic moment. As a veridictional claim, it was precisely construed as worthy of attention, and repetition for the next 50 years, because he was describing something present, something on the rise. He was not a historian, but rather a savvy producer of a narrative to catalyze a particular economy of worth and form of life.¹¹⁵

With respect to such a form of life, he called for a meritocracy in which the merit that rules is expertise; a set of skills transmitted through training. With respect to an economy of worth, he spoke in the name of industry against the nostalgic and traditional. The nostalgic and the traditional are forms of worth he

¹¹⁴ David Edgerton, *Warfare state: Britain, 1920-1970* (Cambridge: Cambridge University Press 2005), 192.

¹¹⁵ I take the idea of the multiplicity of "orders of worth" within common worlds from Luc Boltanski, and Laurent Thévenot, *De la justification. Les économies de la grandeur* (Paris, Gallimard, 1991).

considers embodied in ‘Modernist’ literature which abandoned the production of (progressive) narratives about society. For Snow, the sciences operate within a realist narrative of progress. Such progress, although not explicitly stated, is in fact ordered in his narrative according to a *standard* of normalization.

Normalization and Blackmail.

Briefly, and for the purpose of conceptual clarification, normalization is a type of rationality that emerged in the natural sciences and statistics between the mid-18th and 19th Centuries and was mobilized in education, medicine and public health.

Normalization orders aspects of people and things according to a dynamic standard of regular distributions for a homeostatic purpose. The aspects of people and things which are ordered in such a manner are what have been called social facts. These facts are produced by technicians of knowledge production for the ordering and regulation of a population (a school, a sick population, the ‘poor’ etc.) It is the technicians of such techniques of the production of knowledge and the ordering of such knowledge, the ones who associate, display, and coordinate this distribution of social facts, who give the regular distribution its normative content.

It is from an array of social facts about the population of interest that a norm, which is one among many parameters of the field to be regulated, can be selected and taken up as the standard against which the series is normalized, for example, a constant determined by averages, or a Gaussian distribution.

Norms are not descriptions of action or how action is governed. They are forms of rationality and practice. To say they are “not descriptions of action” is to say that it is not knowledge about a subject position capable of acting. If the normative is taken up as a question of a subject capable of action, then we are in a mixed analytic space of values and norms, or values identifiable at the level of the collectivity to which individual action is oriented for its homeostatic function.

No doubt norms can be given a moral inflection, which they are given in Snow’s account. A ‘value’ can be attributed to experience of proximity to, or distance from, a selected norm against which a series (of social facts) of interest is normed. However, norms are not primarily moral; they are evaluative standards to orient the observation of and intervention into the relation between people and things. It is the contest over the normativity of norms that demarcates what one might call the anxiety over the gulf between forms of knowledge and life.¹¹⁶ It is

¹¹⁶ C.f. Georges Canguilhem, “The Formation of the Concept of Biological Regulation in the Eighteenth and Nineteenth Centuries,” in *Ideology and Rationality in the History of the Life Sciences* (Cambridge: MIT Press, 1988).

precisely this indetermination of technical know-how and the significance of such know-how in the activity of living that I will reflect on.

Snow's argument was made and heard through a moral monopoly by way of an industrial form of worth and a metric of normalization. What is interesting about this is that Leavis's response was to attempt to refuse the blackmail embedded in this monopoly; are you for or against science? Leavis recognized that intellectually it was a ridiculous question. His concerns were the form of life and activity made possible through education.

These concerns are, nominally, the same as Snow's. It is this commonality which meant they could argue. Leavis, however, had a positive program of critique. He wanted to show the outside and limitation to the form of worth championed by Snow. He made practical proposals for the re-organization of higher education around the principal that our ways of thinking have become *descriptive* rather than *creative* and the form of life that we attempt to describe and re-describe to ourselves is vacuous and materialistic.

Is this nostalgia? Snow claimed so and yet Leavis was never in actual fact a traditionalist. He was a critic; he led a life of critique. The problem for him was the "disunity of life" and it was problematic precisely because in a unified communal life, reflection on life could affect the lived experience of a community. Leavis' primary exemplar for this is Shakespeare. This may strike us as a little weak, yet the significance of Shakespeare's thought for Leavis was that it could offer a shared standard of experience. Such a 'shared standard' was articulated in Leavis' work by way of the relation of literature to political life. This is not the direction I will follow, however Leavis is a resource for me within their debate insofar as he offers an outside to Snow's standard of judgment in evaluation of the contribution of science to life.

Unlike Snow, Leavis didn't care for 'the social' as the domain of jurisdiction. For Leavis the location of the problem of conduct was in thinking, the task was to create modes of speaking the truth, of creating works of art and knowledge, which might remediate the disunity of life. Such remediation would seek standards other than the technocratic and justifications other than those of the 'the social.'

A Form of Life?

We have, in the disagreement between Snow and Leavis, a disagreement about the relation of a form of life, to knowledge and the flourishing of that form of life. I think it is important to cover a little of the ground through which I am bringing together these three terms, life, knowledge and flourishing, so as

ultimately to be able to bring Snow and Leavis into a common frame with respect to their differing stances on how these three terms can be brought into an living relation. The textual anchor point is Aristotle's *Ethics*. I think differing stances to knowing and making embodied in Snow and Leavis offer us a situation through which to extend Aristotle's articulation of the fundamental problem of knowledge, life and flourishing into our contemporary; to re-articulate the problem in terms which the humanities and social sciences are living with today.

It is a commonplace to say there are two differing conceptions of flourishing in Aristotle's account of the human good (*anthropinon agathon*).¹¹⁷ Or otherwise put, there are two differing responses to the question: what is the best form of life for a human being? One answer is the life of practical wisdom and the other, the life of contemplation. This may strike us as similar to Snow's distinction; the arts contemplate the irremediable things of life and the sciences intervene of remediable things in practical life. Revisiting Aristotle, however, will help us to re-think Snow's distinction and put the question of knowledge, life and flourishing into a more generative common problem, one which takes up the dimension of character.

Snow rejects the life of "contemplation" as being against progress. Let us agree with Snow that we cannot accept Aristotle's well known valorization of the life of contemplation over the excellence of a "practically wise life."¹¹⁸ Nevertheless, within "practical life," what are the consequences of giving over ethical valorization solely to the productive capacities of *technē*, of craft or making? What are the consequences of a failure to develop modes of reflection and judgment which can provide standards other than those appropriate to technical thought?

Snow takes as his object of reflection scientists and engineers whose orientation is to make life better through what they know and make. Aristotle makes a distinction between making and action. Both making, which includes technical crafts, and acting, are tied to reason. The 'characteristic' bound up with

¹¹⁷ My thanks to Dorothea Frede and James Stazicker for their guidance in Aristotle's *Ethics*.

¹¹⁸ My judgment on Aristotle's valorization of contemplation over *phronesis* in Book X of the *Ethics* is that it leads to a dual and paradoxical *figure* of the "human good." The paradox is one of it being simultaneously proper to a human life and a life of divinity, which is not proper (*idion*) to the human being. My position is in contrast to the Aristotelian philosopher, Richard Kraut, who thinks that there is no paradox, or incompatibility in Aristotle's conception of the two best forms of life. He makes the argument on the basis of Aristotle's claim in Book I that "happiness" (his rendering of the term *eudaemonia*) consists in one good, the virtuous exercise of the 'theoretical' part of reason. The cultivation of this part of reason is developed most fully in a life of contemplation, which is itself modeled on the highest form of contemplation which is the gods' auto-contemplation. Kraut resolves the anthropological problem of dual conceptions of the human being's *essence* by way of a hierarchy of existence. He does not acknowledge the anthropological indetermination of the conflict of essence and existence. See Richard Kraut, *Aristotle on the Human Good* (New Jersey: Princeton University Press, 1989).

each, however, is different. For my purposes what is important is that we can take from Aristotle that the origin of making, just as for action, lies not in the thing made or the effect of an action, but in the person.

Aristotle tells us that an “art (*technē*) is necessarily concerned with making but not with action.”¹¹⁹ What is to stop up from taking up *technē* and technical know-how as part of the broad question of “living well”? If such know-how is necessarily concerned with making, should we read Aristotle as saying it necessarily not concerned with action? I am supposing that we need not. This starting point is built on in Chapter Four where I show, at the level of practice, why I think this is the case.

The distinction between kinds of life, their orientation and the means by which they are lived, matters for Aristotle insofar as the majority of the *Nicomachean Ethics* is devoted to the question; what is the highest of all goods which is achievable in action? Bracketing his valorization of the life of contemplation over the life of practical wisdom, his general answer is, “the good life.” ‘The many and the wise’ are in agreement on this and this good life involves living well and doing well. But what kind of activity is living well and doing well? “The many,” according to him, think it involves pleasure, profit, eminence and health.

In Aristotle’s account, it is by taking seriously the “good” of the political life, i.e. a communal life which requires the governance of self and others, that we are able to pose the question of virtue as an outside to pleasure, profit, eminence and health. Virtue is a question of the excellence of a practice, which admits of excesses, deficiencies and ‘the middle’ or mean, of which *technē* certainly count as such a practice. Work on the human good, or reflection on ethics, is preparatory for concerns about how people can live together. But as Aristotle explains, even this seems incomplete as possession of virtue is not enough; one actually has to exercise virtues.¹²⁰

Horos.¹²¹

In Book 6 of the *Ethics*, Aristotle suggests that one of the objects of ethics is choices in and ways of doing an activity. The mode in which reflection on such objects occurs, he suggests, should be through virtue oriented to correct reasoning.

¹¹⁹ Robert C. Bartlett and Susan D. Collins, trans., *Aristotle’s Nicomachean Ethics* (Chicago: University of Chicago Press, 2011), line 1140a 18.

¹²⁰ These concerns are revisited in relation to Dewey’s reflection on *technē*, education and progress in America in the next chapter.

¹²¹ S.v. “*Opos*,” *Liddell and Scott Greek-English Lexicon*; “a limit, rule, standard, measure; in Mathematics, *opoi* are the terms of a ratio or proportion, Arist. Eth. N. 5. 3.”

Correct reason is characterized by the aims or targets of the one who possesses such reason. Crucially, Aristotle uses the term “*horos*,” sometimes translated as boundary, but much more acutely translated in my judgment, as Rackman does, by the term standard, or even better, as “measure.”

At line 1138b20 Aristotle suggests that “there is a certain standard determining these modes of observing the mean.”¹²² Fundamentally, it is not enough to *know* what the having the state of the mean is, i.e. to know about virtue, rather it is necessary to know by what measure the mean is brought about.¹²³ Such knowing cannot be disaggregated, in Aristotle’s account, from the development of character relative to actual situations in which the mean can be activated.¹²⁴ The fact that he never actually names this standard substantively has left us with long durational problems of knowledge, life and flourishing.

With these preliminary comments in place, we can re-visit our starting point. Leavis and Snow are arguing for differing modes of existence for a possible subject of knowledge with respect to the available forms knowledge and practice, which depend to differing degrees on our technical abilities, capacities and available means, but which cannot be reduced to those means without an evasion of the ethical problem of the relation of knowledge and life, considered as a question proper to reflection on the standards which human beings seek to activate in their relations of living and knowing.

Two possible consequences to articulating different modes of existence are: 1) a failure to agree on standards of cultivation to orient one to the human good within a situation will lead to ethical incommensurability; 2) if one evades the question of standards of judgment or measures of action, and if one were to follow Aristotle, one would be without a guide as to the range of excesses and deficiencies in a practice.¹²⁵

¹²² Aristotle, *Nicomachean Ethics*, trans., Harris Rackham (Cambridge: Loeb Classical Library, 1934.)

¹²³ Sandra Peterson asks, “what is the standard or criterion to which right reasoning looks?” As Peterson explains, there have been several responses to this question among Aristotle scholars: Some like Akrill think he has raised a question which he fails to answer; others like Rowe, think he ultimately makes the point, by not answer explicitly the question of what this standard is, that there is an *horos* calibrated the question of a flourishing life, but it cannot be given in advance of cases and situations. I agree with all three in rejecting the idea that the *horos* could be contemplation.

Sandra Peterson, “Horos (Limit) in Aristotle's "Nicomachean Ethics,"” *Phronesis* Vol. 33, No. 3 (1988): 233-250; J.L. Ackrill, “Aristotle on Eudaimonia,” in *Essays on Aristotle's Ethics*, ed., Amelie Rorty (Berkeley: University of California Press, 1980). John Cooper, *Reason and Human Good in Aristotle* (Cambridge: Harvard University Press, 1975), 101-103. C.J. Rowe, The Eudemian and Nicomachean Ethics: A Study in the Development of Aristotle's Thought, *Proceedings of the Cambridge Philological Society Supplement* No. 3 (1971).

¹²⁴ Sandra Peterson, “Horos.” “Aristotle's answer, that character is necessary and even approaches sufficiency for making this method useable, are given at various points after the question what is the horos at 1138b.”

¹²⁵ I am pre-figuring the problem of the bind one may get into when contrastive measures, excesses and deficiencies combine.

Leavis and Snow came from similar petit-bourgeois backgrounds, and neither could take for granted, nor wished to take for granted the determinative role of class in social life as the way by which such shared standards could be given, even if socio-economic and cultural class position may determine certain economic aspects of the possibilities of living. Their simultaneous responses to the problematic relation of knowledge and life introduced contrastive orders of worth. They each sought, nonetheless, to make 'education' the means by which thought could re-make the ethical and political stakes of living. The focal point for their interconnection, is the search for a measure by which technical progress can be judged in relation to life.

Genealogical Anchor Point: Matthew Arnold.

In response to Leavis' Richmond lecture, Trilling wrote the essay "*Science, Literature, Culture*." He begins by outlining that in 1883, Matthew Arnold gave his three *Discourses in America*, "Emerson," "Numbers," and, according to Trilling the least offensive, "Literature and Science." These were three flares from a self-proclaimed 'cultural critic.' Although not an aristocrat (he supported himself and a family as Her Majesty's Inspector of Schools), he defended education against the "natural tendency" of an industrial democracy to favor "practical" studies. The last of the three lectures, Trilling informs us, was prepared the year before for his Rede lecture of 1882; seventy-seven years separates "*Literature and Science*" from Snow's "*The Two Cultures*." The fact that there is no mention by Snow of his predecessor, against whom he is arguing *de facto* even if not in intention, is perhaps a mark of the historical amnesia which Gyorgy Markus names as a characteristic of the semiotics (but not hermeneutics) of the natural sciences, the form of knowledge for which Snow was arguing in favor. ¹²⁶

Arnold is a genealogical anchor point for taking up the relation of Leavis to Snow. Arnold, was a poet, literary critic and defender of the claim that rather than a mere commentary, literary criticism is a genuine form of thought within which thought and language can be brought together for the purpose of creation. Like Snow, Arnold had an antagonist for his Rede lecture, in the person of TH Huxley. The debate between Arnold and Huxley, which pre-figures the one between Snow and Leavis, cannot be understood as a contest between science and culture, the latter term being understood by Arnold in the sense of high culture or the most excellent products of human thought and creation. The former term, pace Wolf, is understood by Arnold as the systematic study of *any* question, by way of its original sources.

¹²⁶ Gyorgy Markus, "Why There is No Hermeneutics of the Natural Sciences," *Science in Context* Vol. 1, No. 1 (1987): 5-51.

The question was rather the service to which knowledge is put; which kind of question, can which kind of knowledge answer? The question of purpose then poses the question of distinction. If the arts and sciences can be taken up in a common frame as a product of “culture,” what then separates the culture of science and the culture of literature? In Trilling’s reading of Snow’s technocratic anti-history,

“it is the future, and not mere ignorance of each other’s professional concerns, that makes the separation. The future that the scientists have in their bones is understood to be nothing but a good future; it is very much like the History of the Marxists, which is always the triumph of the right, never possibly the record of defeat. In fact, to entertain the idea that the future might be bad is represented as being tantamount to moral ill-will...”¹²⁷

Pathway: Experience & Capacities.

Fifty years on, the position of the natural sciences and engineering today has changed. Snow proposed to the humanities a new intellectual formation that would take seriously the effects of the rise in technical capability of the natural sciences and engineering. Today there may well still be a broad dismissal, or denunciation, by some humanists of these technical capacities. The significance of Snow’s question from the vantage point of today, however, is given the scale and magnitude at which the natural sciences operate, what are the consequences of a failure on the part of these sciences and technologies to find a ‘formation’ capable of reflection on the limits of their practice? On the side of cultural inquiry, the simultaneous and necessary question is what is it necessary to know and through what mode, such that one can speak back to practitioners and institutions of scientific knowledge production so as to pose the question of such limits?

Snow takes issue with those humanists who dismissed the development of technical capacities which were designed to solve problems of what he called “elemental needs,” especially “when one has been granted them and others have not.”¹²⁸ As we will see, he offers two micro-portraits of figures in the realm of cultural knowledge production, one of which leads away from the question of eternal recurrence of the conditions of existence and towards a resuscitated humanism.

¹²⁷ Lionel Trilling, “Science, Literature and Culture: A comment on the Leavis-Snow Controversy,” *Higher Education Quarterly* Vol. 17, No. 1 (November, 1962): 16.

¹²⁸ Snow, *Two Cultures*.

Humanism for Snow was being demonstrated not in the capacity to reflect on the individual condition of existence but in the capacity to make life better. He astutely recognized molecular biology as a crucial vector in this landscape. His enthusiasm shows a faith in technology and modernization. Moreover, his distinction between the timeless (and hence for him unchanging) character of the irremediable individual condition of existence and the remediable socio-historical conditions of existence is itself ultimately modernist. The timbre of his paean, however, is tempered with a set of questions, which are important for what I will suggest will become our problem at hand.

Firstly he asks, how has 20th Century cultural thought responded to changes and challenges of 20th Century industrialization? He offers a contrast between the individualism of Lionel Trilling's *The Modern Element in Modern Literature* and the social ontology of György Lukács' *The Meaning of Contemporary Realism*.¹²⁹
¹³⁰ For Trilling, in Snow's misreading, the alienation of modernity and industrialization offers the conditions for the liberation of the individual. The modern is existential backdrop for the expression of and reflection on a timeless condition of the human being's capacity to be affected, to experience. Lukács takes up this individualism as itself a modernism in contradistinction to realism. He differs from Trilling in his characterization of the ramification of this division in terms of social ontology. For realists, Lukács writes, the human thing is the Aristotelian political animal (*zoon politikon*) in which ontology cannot be separated from socio-historical environment.

Lukács' modernist, like Snow's intellectual, understands the human thing as "by nature solitary, asocial" and "unable to enter into relationships with other human beings."¹³¹ The contrast is that whilst the realist may have occasion to portray the particular solitariness of an individual, it is not an ontological solitariness of the modernist writer.

In literary realism the human being, understood as a 'political' being, exists in situations. A situation, for Lukács, is "a phase, climax, or anti-climax in the life of the community as a whole."¹³² Any particular event can only be read against the developmental historical change of the totality, be it *polis*, nation, world or other form in which collective living is made possible.

True to Lukács' political philosophy, the modernists' false understanding of history is explained through two aspects: Firstly, the hero of the modernist story is

¹²⁹ In *Partisan Review*, reprinted as "On the Teaching of Modern Literature," in *Beyond Culture* (The Viking Press, New York, 1965).

¹³⁰ György Lukács, ' *The meaning of contemporary realism* (Merlin Press, 1963).

¹³¹ *Ibid.*, 20.

¹³² *Ibid.*

strictly confined within the limits of his own experience. Secondly, these limits of experience render the revelation of the human condition as the only possible development of character. This is what Snow refers to as the “individual condition,” which is sensational. The modernist sees potential only abstractly, in which limitless possibilities are taken as more significant than the actual. “Life” can never match the limitless possibilities of potential resulting in the modernist vacillation between what Lukács calls “melancholy and fascination ... soon tinged with contempt.”¹³³ This understanding of modernism is contrasted with a form of thinking in which capacities can become actualized through situations. This form, for Lukács, attends to those actual situations in which parameters of action are non-subjective, even if action nevertheless includes motive or intention.

In the false dialectic between Romanticism and Social Realism, Snow puts himself in a sublated technical apolitical and morally superior position. The humanities and social sciences are given a role by Snow insofar as they can assist in bringing about this synthetic ethical technocratic outcome. In his *Second Look*, aware of his own lack of reflection on the role of the “social sciences,” although not aware of the limited veracity of his polemic against modernism, “social history” is given pride of place as a warranted form of knowledge production about things human.

If today, a half century on I am focusing on forms of knowledge which deal with the human *qua* being in relation with others, a social being, then I am in a position to ask a question: How can the human sciences (*Geisteswissenschaften*) participate in the historical situation of the human thing?

Snow’s discomfort with both Lukács and Trilling stems from a problem recognized in his later reflection on the lecture. Although not explicit, he intimates the problematic breakdown in a relationship to which neither individual experience nor philosophy of history are adequate solutions:

“How far,” he asks non-rhetorically, “is it possible to share the hopes of the scientific revolution, the modest, difficult hopes for other human lives *and at the same time* participate in the kind of literature which has just be defined?”¹³⁴

“Literature” need not have a narrow definition. What is the relation between a hope in technical solutions to the problems of the day and a participation in reflection on the significance for life of the changes they bring about?

¹³³ Lukács, *The Meaning of Contemporary Realism*.

¹³⁴ Snow, *Two Cultures*, 96-97, my italics.

Cæsura.

What I have offered so far is a mapping of a set of complex relationships: We began with Snow's lecture in 1959. We saw how he gave a false dichotomy that was shown in a "third" position, Leavis, which offered a different reading of the problem at hand. This position has historical precedent in Arnold but it is clear that to set them up as a new dichotomy—critique or progress—is once again to re-inscribe a prison-house of subject positions, which forestalls rather than opens up the possibility of inquiry.

Snow's readings of Leavis as anti-science and of Trilling as modernist are necessary to the *Two Cultures* as a piece of anti-history. Leavis' response is crucial to understanding the inter-personal dimension, and the subjectivational work that accompanies the practical effects of being able to speak with authority and in a manner which gets to count as true.

Leavis' reading of Snow as naïve and irresponsible, however, does not do justice to the subject position Snow is in; a position which is inherently ethical and political. From this position, however, he did not observe the limitations and externalities to the manner in which he has set up the problem. Likewise, Snow's reading of Trilling does not justly observe the fact that literary culture, rather than modernist and merely alienated, does have effects on how problems are named, formed, perceived and worked on. As Trilling wrote in 1962:

“Certain literary men raised the ‘Condition of England Question’ in a passionate and effective way and their names are still memorable to us—Coleridge, Carlyle, Mill.”¹³⁵

There is a very interesting *nota benne* by Trilling on Snow's characterization of Emerson and Thoreau as “entertaining fancies”; of being, as Snow would then characterize Trilling, romantic, counter-modern and tragically individualist. Trilling writes,

“Emerson doesn't deserve Sir Charles's [Snow's] scorn on this point. His advice to the American scholar was that he should respond positively to the actual and the modern, and he was inclined to take an almost too unreserved pleasure in new forms of human energy and ingenuity. As for Thoreau, his quarrel was not with factories but with farms-and families.”¹³⁶

¹³⁵ Trilling, “Science, Literature and Culture,” 18. One should also take novels such as Brontë's *Shirley* into account when talking of how the condition of England question was raised.

¹³⁶ *Ibid*, 19.

Trilling defends literary thought as part of the inheritance of a culture which valorizes thinking such that one can change. This then is Trilling's highly effective reformulation of the problem of the debate between Snow and Leavis. He writes,

“...when Sir Charles speaks of the need to break the ‘existing pattern’ and to go on to a right education, he does not touch upon any such standard of judgment.”¹³⁷

There is no proposal for what the relation of knowledge is, other than, humanities students should learn the second law of thermodynamics and the scientists should be “trained not only in scientific but in human terms.”¹³⁸ This will become the serious challenge of finding a practice in which the problematic relation of knowledge, life and flourishing can be posed.

The obvious difficulty with beginning from Snow's starting point, and the reason why I returned, if a little clumsily to Aristotle, is that “the human” and knowledge of what the human is, has been reduced by Snow to the merely technocratic and biological; enough food, shelter and continuing prosperity.

These things are real, but are only some among a range of ethical terms by which one can judge the human good. What role knowledge production plays in the human good is far bigger than simply a conduit for the production of artifacts which promote health and wealth. How then to inquire into the relation of science and the human good and through which standards of judgments? These questions and their interrelation is the core problem of this thesis.

Équipement.

Our work in Human Practices drew on a curiously little explored term that was central to Foucault's last three lecture series and which was clearly fundamental for his on-going re-thinking of the relations of truth and subjectivity. It is fundamental as well for taking seriously the relations of knowledge, life and ethics which I have introduced. This term is *équipement*, a translation of the Greek, *paraskeuē* (παρασκευη). Etymologically, the English term “equipment” stems from the 12th Century French term designating the manner through which a ship is readied for a voyage. *Paraskeuē* is composed of the terms *skeuos*, meaning instrument and *para*, meaning in this context, by the side of, or next to. One of the earliest meanings of the term was the manner in which ships and soldiers were prepared for war.

¹³⁷ *Ibid*, 20.

¹³⁸ Snow, *Two Cultures*.

Following June Allison's philological exegesis, the term appears as a technical textual term in Thucydides' *History*, where its semantic range is shown as both product and practice: *paraskeuē* means both the practice of preparing oneself and others for a future event as well as the state of that preparation.¹³⁹

The challenge of entering the Human Practices subject position was to take up the challenge of producing such a practice that might orient the relation of the sciences to the problem of human flourishing within scientific settings, our own and others, and to produce the *équipement* needed to carry out such an undertaking.

A commonplace in 2006, articulated from government funders as well as researchers, was that there was a need for new types of connection between science and ethics. This is true for the STIR project as well. In each project, the indetermination was how to collaborate productively between the human and natural sciences.

Initially, as observer and neophyte, I was aware of the need to reflect on the relation of inquiry to the objects that collaboration presupposed. This was not simply an awareness that inquiry is embodied, or that the inquirer is always situated relative to the objects of study.¹⁴⁰ Rather it was an observation that inquiry, relative to the object in question, requires reflection on the manner in which one does it, the problem relative to which one does it and the question of which capacities and venues one would need in order to be capable of approaching the problem in the manner sought. Such reflection would be the manner through which a 'shared standard' might be forged.

Équipement was honed as a concept which could assist us, in the development of collective subject position in Human Practices, and in reflecting on how we were conducting our research. In Foucault's discussion of the term in his 1981 lectures he asked, "how can the subject act as he (sic) ought, not only inasmuch as he knows the truth, but inasmuch as he says it, practices it, exercises it?"¹⁴¹ *Paraskeuē* are not merely a supply of true propositions, but in Foucault's terms "statements with a material existence." Statements which have *logos*, which are justified by reason, must be turned into ethos.¹⁴²

Equipment is also a practice in the moral philosopher Alisdair Macintyre's sense of the term and not merely a technology.

¹³⁹ June Allison, *Power and Preparedness in Thucydides* (Baltimore: Johns Hopkins University Press, 1983).

¹⁴⁰ Pierre Bourdieu, "Participant Objectivation," *Journal of the Royal Anthropological Institute* Vol. 9, No. 2 (June 2003): 281–294.

¹⁴¹ Michel Foucault, *The Hermeneutics of the Subject: Lectures at the Collège de France 1981–1982* (Palgrave Macmillan, 2005), 318.

¹⁴² Importantly, Allison distinguishes between equipment (*paraskeuē*) and action (*ergon*). Just as every action must have a suitable *logos*, so too must the process of equipping."

“By practice I am going to mean ...activity through which goods internal to that form of activity are realized in the course of trying to reach those standards of excellence which are appropriate to and partially definitive of that form of activity with the result that human powers to achieve excellence and human conceptions of the ends and goods involved are systematically extended.”¹⁴³

A technology is a particular relation of means to ends, whereby means and ends can be adequately defined without reference to each other. A *practice* of inquiry is a means of acting in which the ends are internal to it and in which the standards and forms are generated internally.

The STIR project took seriously the idea that ‘change can happen from within’; organizations and their institutional practices have the means to reflect on how they do what they do. The project took seriously the ends of the bio-scientific practice. An orienting idea in the STIR project was that someone else not of that organization, could act as the *means* for stimulating that self-observation of practice. Reflection on that self-observation, which in the STIR method takes the form of the reflection on decisions made during research, are of value to those participating for different reasons. What these different reasons are, are intentionally not named in advance: Hence its proposed efficacy as a method, or technology.

This is going to be a very important point with respect to the STIR project, which was self-consciously a technique for entering the laboratory so as to stimulate self-observation on the part of research scientists about their practice. Such a technique and its cultivation, is separable from the multiple ends towards which it can be used. The thesis will move in a direction such that we can comparatively pose the question of standards through which a judgment about knowledge and life can be made.

¹⁴³ Alisdair Macintyre, *After Virtue: A Study in Moral Theory* (Notre Dame: University of Notre Dame Press), 175.

Chapter Two

Modern Scene

Internal Condition 1: “I think the modern age of the history of truth begins when knowledge itself and knowledge alone gives access to the truth.”

–Michel Foucault ¹⁴⁴

External Condition 1: “Bureaucratic administration means: authority (Herrschaft) by dint of knowledge—that is its specific fundamental character.”

–Max Weber ¹⁴⁵

Internal Condition 2: “... scientific insight as such—though of a lower order—is creative ... To the university is reserved that which one can discover in and through oneself: insight into pure academic knowledge. For this act of self, freedom is necessary, and solitude helpful.”

–Wilhelm von Humboldt ¹⁴⁶

External Condition 2: “The American's conception of the teacher who faces him is: he sells me his knowledge and his methods for my father's money, just as the greengrocer sells my mother cabbage. And that is all.”

–Max Weber ¹⁴⁷

In Chapter One, I demarcated some lines of inclusion and exclusion so as to specify the problem of knowledge as an ethical problem. ¹⁴⁸ To take up the ethical character of the problem of knowledge—and at an anthropological minimum—one has to pay attention to the actuality—and not only the logical conditions—of how knowledge is produced. This will require attention to practices, which will further entail the discussion of purposes, and reasons, in addition to rules, norms and

¹⁴⁴ Michel Foucault, *The Hermeneutics of the Subject* (Palgrave Macmillan, 2005), 17.

¹⁴⁵ Max Weber, “The Types of Legitimate Domination, Monocratic Bureaucracy,” in *Economy and Society* (Berkeley: University of California Press, 1978), 225. Translation modified by reference to William Clark, *Academic Charisma and the Origins of the Research University* (Chicago: University of Chicago Press, 2006.)

¹⁴⁶ Wilhelm von Humboldt, *Der Koenigsberger unter Der Litauische Schulplan*, 1809 cited in Clark, 2006, 445.

¹⁴⁷ Max Weber, “Science as a Vocation,” cited in Clark, 2006.

¹⁴⁸ A classic treatment of the philosophical-anthropological “problem of knowledge” taken up in neo-Kantian epistemological terms is Ernst Cassirer, *The Problem of Knowledge: Philosophy, Science and history since Hegel* (New Haven: Yale University Press, 1969).

methods of practicing.¹⁴⁹ This puts into focus the question of what the purposes of knowledge are, how inquiry can be practiced, and, to close the circle of questions, how these practices can be judged in relation to purposes. Relative to the preceding chapter, this question of purposes highlights the organizational forms in which different kinds of search and research can be brought into relations of antagonism, cooperation, indifference and, possibly, collaboration, as well the institution of such relations.

In this chapter I circumscribe a scene through which this problematic relation will be reflected on. This is to say, this problem is set within a *mise-en-scène* of fieldwork in collaboration between bioengineers and anthropologists on problems of bioscience, engineering and ethics.¹⁵⁰ With the term ‘scene’ in the *mise-en-scène*, I have in mind one element of Kenneth Burke’s dramatic pentad; act, agent, agency, scene, purpose and interestingly, he sometimes includes a sixth, manner.¹⁵¹ ¹⁵² Briefly, *act* refers to an action conducted by an *agent*, *agency* to the technical, material and mental means, *agent* to the person or thing that does the *act* and the *purpose* is a description of the ‘motive’ of the act. The *scene* is the figurative or physical context in which things occur. A scene is as much historical as it is material.

What is important for the use of the pentad is that the terms are analytic. They break down situations into component parts, which in practice, and in their actuality, are composite. Thus, no single element can really be analyzed alone; the terms, in Burke’s usage, always exist in ratio. The ratio I am interested in, relative to my problem of knowledge as an ethical problem, is the scene-purpose ratio; what is the context in which knowledge work happens and for what purpose? What is the scene which connects the institutional, and organized, conditions of a practice to a subject’s relation with and capacity to engage in such a practice?

¹⁴⁹ James D. Faubion, *Modern Greek Lessons* (Princeton University Press, Princeton New Jersey, 1993), xiv – xvi.

¹⁵⁰ George Marcus, “*The Uses of Complicity in the Changing Mise-en-Scène of Anthropological Fieldwork*,” *Representations* 59 (1997): 85-108.

¹⁵¹ Burke, Kenneth A *Grammar of Motives* (University of California Press, Berkeley and Los Angeles, California [first published 1945] California edition, 1969).

¹⁵² Manner, as understood through the Renaissance artistic term “*maniera*,” is a problem of relating multiple compositional norms and how to give form to them. Ernst Gombrich, *Studies in the Art of the Renaissance: Volume 1: Norm and Form* (London: Phaidon 1966.)

Style, by contrast, referred to the overall craft of an individual painter, their techniques, brushstrokes, and palette, among other things. Vasari, (1979, [1550]). For anthropologists, such as Alfred Kroeber, the ‘individual style’ of a culture is a principle of integration and general quality of civilizations indebted to a teleological account of historical forms and content (for which he draws on Spengler’s *The Decline of the West*). Cf. Alfred Kroeber, *Style and Civilization* (Cornell University Press, 1957) However, the distance between the two terms, if I can posit one for the sake of a clarifying distinction, is that whilst *style* refers to the Boasian, or Spenglerian ‘particularity’ of the cultural whole *manner* refers to the intellectual and characterological problem of the relation between compositional norms and of a life in which a subject can practice such composition. Cf. Franz Boas, *Anthropology and Modern Life* (1928).

In delineating a *scene* which I think situates the venues in which I worked, I will have recourse to a variety of historical elements. These will include the influence of ideas, the building of institutions and the development of practices. The scene is composed of the development of the European and American university since the 19th Century. This is a moment in European and American history in which the purposes of knowledge were once again in question as technological developments were bringing about a second industrial revolution. The point is not to rehearse the history of the 19th Century, but rather to pick out important genealogical elements.

Which elements? The projects that I will describe and analyze in the latter half of this thesis were based in US research centers and in a Swiss technical university. These are the *venues* within the *scene* that I am interested in. They are of interest because the ‘research center’ and ‘technical university’ are two generally occurring forms in which knowledge work happens. They both have common origins in the mid-19th Century changes to the understandings of what a university is for. The organizations I will describe pose the question of how research as an activity has been given form in the context of three important vectors: the vocation of scholarship, techno-scientifically driven capitalism (and the ambivalence this has produced over “culture”), and the related question of the public service and utility of knowledge. The description and analysis of these venues which function in the scene of these vectors will be propaedeutic to the question, in the latter part of the thesis, of what role the human sciences can have in and in relation to these spaces of knowledge work.

The specific area of research I am interested in is what in 2006 was considered to be an emerging domain of bioscientific and engineering practice; “synthetic biology.” I will take up this object of research in the chapters that follow. Let me briefly say, however, that synthetic biology is a term of art of the very recent past, which designates an approach to engineering biology. The institutions and transformations in tertiary education and research within which it might yet flourish, however, are dependent on scientific, political and economic changes of (at least) the last thirty-five years. These institutions themselves have origins in transformations in the venues dedicated to human and natural sciences in the 19th Century.

The Research Centers I worked in and with are based at UC Berkeley and Arizona State University, and are both multi-university US National Science Foundation funded centers; one is an Engineering Research Center dedicated to synthetic biology and the other is a Center dedicated to the governance of nanotechnology in society. The Department I worked in is a satellite campus–situated in Basel, Switzerland–of a world class technical university; the

Eidgenosiches Technisches Hochschule (Swiss Federal Institute of Technology). This department is dedicated to Biosystems Science and Engineering (D-BSSE).

This is not a comparison of two national cases, the US and Switzerland.¹⁵³ In this respect I am not describing the traditional *mise-en-scène* of ethnography, even in its comparative guise. The US and Switzerland are two national contexts in which I was part of projects attempting to bring ethics and science into a more determined relationship. As such, in this chapter, I sketch a scene of genealogical elements which will act as both a setting and a path for the acting out of our problem. The contexts, the US and Switzerland, are constitutive but not determinant. This is not a comparison of national contexts read as cultural objects. The Swiss and American contexts share some common—mainly German—historical elements. My aim at this point is to trace these elements, as well as the relevant autochthonous elements, as preparation for our problem of how human and natural scientists can invent a mode, form and practice of collaboration on significant social, ethical and political problems. This is not a predicament of culture, but rather a cultural problem of the significance of science, circumscribed in this inquiry by several Euro-American organizations, and practices of bio and human science, as well as their institutional assemblage.

In this orienting scene, I will first delineate an important element in the emergence of the university in Europe; the relation of the German concept of *Bildung*, self-formation, to the pursuit of research. For Germans of the early 19th Century the development of a “research” tradition was heavily influenced by changes (as well as continuities) in the Pietist concept of *Bildung*, in addition to the demands of newly emerging nation states. These two elements themselves underwent transformation in post-Civil War America and the new Swiss confederation. In the second part of the chapter, I will sketch the institutional changes that affected the rise of science and engineering in the US and Switzerland in 20th Century.

19th Century *Bildung*: A Theological, Pedagogic and Reflexive Concept.

In eighteenth century Europe, the university was a marginal scholastic institution for the transmission of a closed body of knowledge, usually by means of reading aloud.¹⁵⁴ Whilst it is true that the humanist break with scholasticism was catalyzed by the *philosophes* of 18th Century France, these individuals were not, however, the driving force behind the re-habilitation of the university in the 19th

¹⁵³ Ulrich Beck, “Toward a New Critical Theory with a Cosmopolitan Intent,” *Constellations* 10, No. 4(March 2003): 453–468.

¹⁵⁴ Daniel Fallon, *The German University: A Heroic Ideal in Conflict with the Modern World* (Colorado: Colorado Associated University Press, 1980), 6.

Century. That task fell to the Prussian state and then eventually Imperial Germany. Whilst it is true that “Enlightenment” and later Napoleonic France were influential in the development of European institutions of higher education, it was a German form of the institutionalization of knowledge, which acted as the primary model.

The specificity of the German form can be understood by way of the German reception of and reaction to “*Aufklärung*,” Enlightenment, which involved a simultaneous obedience to community and a conception of self-cultivation through reflection. *Aufklärung* as a form of education was not reducible to either culture or knowledge. Rather, there was a keen awareness in 19th Century Germany, that enlightenment may conflict with education.¹⁵⁵ The warning sign for many was revolution. Enlightenment had to be channeled into the right kind of education; hence the importance of educational reform vis-à-vis enlightenment ideas. *Bildung* was the concept which guided this reform. *Bildung*, furthermore, differentiated the way of being which could move toward “freedom.” It did not include equality in the manner of French political and philosophical thought. It was not a rejection of the *ancien regime*, but rather its re-formation.

Reinhard Koselleck writes that,

“While Enlightenment appealed to reason, by which humans should allow themselves to be guided, and to nature, knowledge of which would provide permanent rules and laws for all spheres of experience, and while of both of these tasks simultaneously established social, economic, political, and collective historical goals, *Bildung* challenged a large multitude of human possibilities.”¹⁵⁶

In line with Koselleck’s identification of *Bildung* as a theological, enlightened pedagogic and self-reflexive concept, Louis Dumont describes the manner in which the prior theological dimension of self-cultivation prepared the setting for a particularly German reception of the demand to dare to know;

“Reformation has immunized Germany against Revolution... The deep imprint left on German minds by the Lutheran Reformation provided the channel through which the external and institutional elements of the Enlightenment and Revolution could be internalized.”¹⁵⁷

The Pietist origins of the concept give us a contextual element for understanding how *Bildung* was supposed to enhance the best characteristics and preserve against

¹⁵⁵ Moses Mendelsohn, “Ueber die Frage: Was heist aufklären?” *Berlinische Monatschrift* 4 (1784): 193-200.

¹⁵⁶ Reinhart Koselleck, “On the anthropological and semantic structure of *Bildung*,” in *The Practice of Conceptual History: Timing History, Spacing Concepts* (Stanford: Stanford University Press, 2002), 180.

¹⁵⁷ Louis Dumont, *The German Ideology* (Chicago: University of Chicago Press, 1996), 24.

the worst characteristics of French Enlightenment. The Pietist stress on personal involvement and individual self-obligation gave a signification to *Bildung* as co-terminus with the private use of religion and a public use of reason.¹⁵⁸

It would be overly simplistic to think that the “subject” of *Bildung*, which is often translated as self-cultivation refers in any simple sense to “individuals.” Rather, the “large multitude of human possibilities” that Koselleck refers to, and that Dumont says are “internalized,” are relationships which form capacities, for subjects, in the singular and plural. The relational pair is not the ‘individual’ and the ‘collective.’ The subject of *Bildung* is not a “monad,” or the Renaissance microcosmic individual.¹⁵⁹ There are two dimensions to the subject of *Bildung*: innate capacities to be developed (*Ausbildung*, equivalent to development or training) and the *transmutation* which comes through “adaptation” to an environment (*Umbildung*; a re-making, re-forming, re-constructing). As Stahl wrote in 1934,

“The idea of *Bildung* according to the philosophy of humanity requires that those capacities be harmoniously developed to set forth *in their particularity as in their totality*. The important factor is that particularity be preserved.”¹⁶⁰

This can be understood as the German reaction against French Enlightenment. For many German thinkers, not least Wilhem von Humboldt, the revolution betrayed liberty by instituting ‘political life’ as an autonomous sphere. The instantiation of an autonomous political sphere was a threat to many German thinkers’ conception of liberty, since what counts as ‘political’ for many of these thinkers, not least the German idealists, was not adequately circumscribed by ‘representation’ in a political sphere. The demarcation of the political from the ethical, meaning both the customary and the normative, was of great concern.¹⁶¹ A consequence of refusing to constitute politics as an autonomous sphere was the need for a conception of the constitutive relationship of tradition, custom, institutions and the practice of reflection in augmenting institutions, forming new policies and acting. This conception was produced by turning a strictly religious conception of “*Bildung*,” into a pedagogical concept.

There is much we cannot bring with us in a discussion of *Bildung*’s historical origins relative to our problem, e.g. the necessary connection of the

¹⁵⁸ Immanuel Kant, „Beantwortung der Frage: Was ist Aufklärung?“ *Berlinische Monatschrift* 4 (1784): 481-494.

¹⁵⁹ Ernst Cassirer, *The Individual and the Cosmos in Renaissance Philosophy* (Chicago: University of Chicago Press, 2010[1927]), 93.

¹⁶⁰ Stahl, 1934, quoted in Dumont, *German Ideology*, 32.

¹⁶¹ See also, Reinhart Koselleck, *Critique and Crisis: Enlightenment and the Pathogenesis of Modern Society* (Cambridge, Mass. : MIT Press, 1988 [1959]).

subject to the cultural whole, the *Volk*, an ideological connection which has historically served to mark particular kinds of subject outside of an order of worth.

Nevertheless, I do not think this therefore requires that we renounce this historical, philosophical concern with the relation of a subject to its capacities, not least since we are concerned in particular with the subject and capacity for knowledge. I will offer one presentation of the term, *Bildung*, and its particularly German 19th Century formulation of the relation of the subject and its capacity to know, through Wilhem von Humboldt's writings.

Humboldt has two formulations of *Bildung*: a) the "subject in its unity" has to assimilate as much as possible of the "external diversity" b) *Bildung* is the highest and most "proportioned formation of strengths into a whole."¹⁶²

The concern for *Bildung* is a concern for the human being's relation to the *total* rather than the *universal*. Science, normatively speaking, is universal; its results are applicable regardless of its content as it is the consequence of a *method* universally applicable and assured and ensured by formal criteria regulative of the content.

The significance of the production of knowledge and the internalization of the diversity of knowledge, and its synthesis into a meaningful life, for the 19th Century Humboldt, however, was a question of the particular, the historical and the whole of humanity in its historical and temporal specificity. This historical and temporal specificity is what characterizes the problem of totality.

Specificity is provided by the relations between people and things in a context, a conception of culture, which of course became foundational for anthropological accounts of knowing and knowledge, parameterized by the concept of *Kultur*. I consciously evade the demand to parameterize an anthropological account of knowing by way of the culture concept and instead seek to activate Humboldt's reflection on the *relation* constitutive of knowledge; that the pursuit of *Bildung* be an activity of *Genuss* (joy).

The relation between *Bildung* and knowledge is a relation between an objectivation and subjectivation; what is crucial for the notion of *Bildung* is that they are given a relation. As Weber would say as the cultural wane of *Bildung* progressed,

"In science, only the person who serves his task has 'personality.' And this is true not only of science."¹⁶³

¹⁶² Wilhem von Humboldt, „Theorie der Bildung des Menschen“ [1793], quoted in Dumont, German Ideology, 1996, 93.

¹⁶³ Max Weber, "Science as a Vocation," *The Vocation Lectures* (Indiana: Hackett, 2004 [1917]), 10.

Knowledge, Truth and the Subject: Toward Research.

“... the history of truth enters its modern period, when it is assumed that what gives access to the truth, the condition for the subject’s access to truth, is knowledge and knowledge alone.”¹⁶⁴

It seems to me that the transformation in the purpose of knowledge with the rise of “research” and the modern university as an environment in which such research is practiced, is a decisive moment for our manners of being a subject and the relation of these manners of being to the practice of knowing. Research, I will suggest is an extension of one arm of a bifurcation which animates Foucault’s reflection on a subject’s relation to the truth she claims to know; the bifurcation of philosophy and spirituality.

Philosophy is,

“the form of thought that asks, not of course what is true and what is false, but what determines that there is and can be truth and falsehood and whether or not we can separate the true and the false. We will call philosophy the form of thought that that asks what it is that enables the subject to have access to the truth and which attempts to determine the conditions and limits of the subject’s access to truth.”¹⁶⁵

Spirituality is,

“the set of these researches, practices, and experiences, which may be purifications etc, which are not for knowledge but for the subject, for the subject’s very being, the price to be paid for access to the truth.”¹⁶⁶

This means to truth, by way of knowledge alone, is institutionalized, organized and regularized in the mid-19th Century; internally to the sciences, the means to truth by way of knowledge are regularized by the structure of the object to be known, institutionalized internal to the sciences by the rules of method and externally organized through emergence of the disciplines and norms of research.¹⁶⁷

Foucault, seeming to echo Weber’s 1917 diagnosis, said that knowledge, as the means to truth,

“will simply open out onto the indefinite dimension of progress, the end of which is unknown and the advantage of which will only ever be realized in

¹⁶⁴ Michel Foucault, *The Hermeneutics of the Subject*, (Palgrave Macmillan, 2005), 17.

¹⁶⁵ *Ibid.*, 15.

¹⁶⁶ *Ibid.*

¹⁶⁷ *Ibid.*, 18.

the course of history by the institutional accumulation of bodies of knowledge, or the psychological or social benefits to be had from having discovered the truth after having taken such pains to do so. As such, henceforth the truth cannot save the subject.”¹⁶⁸

Very importantly, and important for our discussion of the relation of *Bildung* to knowledge and research, this separation between knowledge and the practices for a subject to be capable of truth was not instantiated with the rise of research science, but with theology. Much more recently than theology was scientific knowledge still tied to questions of character, disposition, spiritual exercise.¹⁶⁹

Under a regime of veridiction of “research,” the subject is capable of knowledge, but this knowledge is not a truth that will save the subject. Nevertheless, even under “research,” there is a demand to be the kind of person capable of facing the fragmentation of knowledge and of asking with no assurance of what the answer is, what the purpose of knowledge is?

***Bildung* and Research.**

One historiographical narrative of discontinuity is that in the 1830s there began a period of change in the conception of *Wissenschaft*, the systematic work of inquiry, reaching its apotheosis in Weber’s diagnosis in *Science as a Vocation*.¹⁷⁰ Two interrelated organizational effects of this ‘transformation’ are important for our scene; firstly, disciplinary differentiation, as a consequence of institutionalization of the university; secondly disciplinary differentiation along novel methodological lines, which include transformations in the natural sciences (labs, new theories, etc.) as well as the rise historicism.¹⁷¹

Historian T.A Howard makes two good points in favor of what I would call a weak rather than strong discontinuity thesis between kinds of *Wissenschaft* pursued before and after the 1830s.¹⁷² The temptation might be to see conceptions of knowledge, and its pursuit, before 1830s as one of a completely “unified” relation of subject and knowledge, typified by the figure of *Geist* in the *Phenomenology of Spirit*, and then afterwards as a fragmented and differentiated

¹⁶⁸ *Ibid*, 19.

¹⁶⁹ Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago Press, 1994); *The Scientific Life: A Moral History of a Late Modern Vocation* (Chicago: University of Chicago Press, 2008.)

¹⁷⁰ R. Steven Turner, “Growth of Professorial Research in Prussia, 1818 to 1848-Causes and Context,” *Historical Studies in the Physical Sciences* Vol. 3 (1971): 137-182; “The Great Transition and the Social Patterns of German Science,” *Minerva*, Vol, 25 (1987):56-76.

¹⁷¹ See particularly part three of Ernst Cassirer, *The Problem of Knowledge*.

¹⁷² Thomas Albert Howard, *Protestant Theology and the Making of the Modern German University* (Oxford: Oxford University Press, 2006), 57.

series of domains of knowledge, along with a fragmented and differentiated subject of knowledge.

Wissenschaft, however, even for Humboldt, implied a ‘never completely solved problem’; as he wrote, “It is a further characteristic of higher institutions of learning that they treat all *Wissenschaft* as a not yet wholly solved problem, and are therefore never done with research.”¹⁷³ This is entirely in line with Weber’s counsel in his lecture to the students in Munich in 1917, which one could read as the apotheosis of the characterization of the search knowledge as fragmentary and incapable of giving meaning to a subject.

Weber then of course goes on to pose a problem, that I think is one that persists today, “Why do we carry on an activity which can never be completed?” Does the fact that it is both our ‘fate’ and our ‘goal’ as Weber puts it, to be scientifically transcended, for the results of our inquiry to not hold timelessly but rather to be marked by their time, does this require a renunciation of science as having “meaning”?

The question points to Howard’s second indicator, that whilst a certain Idealist unity did give way to specialization, this did not irrevocably split forms and practices of knowledge away from one another. Regardless of its form or practice, science can be affirmed ‘for its own sake.’ Furthermore, science may be “of use to the one who poses the question correctly”: how and to what end is my basic question.¹⁷⁴

Technē: Switzerland.

The institutional, national and pedagogical origins of the technical university in Switzerland in which I worked, the *Eidgenössische Technische Hochschule* (ETH), deserve some consideration relative to our detour in the conceptual trajectory of *Bildung* as related to a life of science. The “*Eidgenossenschaft*” is the Oath (*Eid*) of those first three Swiss cantons who entered into fellowship (*Genossenschaft*) to form a defensive alliance against Habsburg domination. During the brief *Republica Helvetica* (1798-1803) and then again at the moment of formal constitution of modern Switzerland (*Confederatio Helvetica*) in 1848 Federalist progressives had lobbied for two federal educational institutions to cut across cantonal and linguistic divisions. When first suggested in 1798, under the influence of Napoleonic France, the newly opened French *Ecole Polytechnique* (1794) was one model, as would be the new University of Berlin (1808). Phillip

¹⁷³ .Wilhelm von Humboldt, “Über die innere und äussere Organisation der höheren wissenschaftlichen Anstalten in Berlin,” [1808/1809], <http://edoc.hu-berlin.de/miscellanies/g-texte-30372/229/PDF/229.pdf>

¹⁷⁴ Max Weber, “Science as a Vocation.”

Albert Stapfer, Minister of Education for the brief *Republica Helvetica*, considered it necessary to build a comprehensive institution for the teaching of arts and sciences useful to the Republic, influenced by both these models.

The idea foundered for both political and technical reasons. The technical reason of interest is that no consensus could be found as to how to produce such a ‘comprehensive’ institution. The available forms kept the highly technical and the liberal academic disciplines separate. The problem of *Bildung*, understood as the proportionate strength of diverse forms of knowing and doing, was alive and unresolved.

By the time the constitution of the *Confederatio Helvetica* was written, politician Stefano Franscini had vigorously advocated for the establishment of three Federal institutions: a teachers college, a liberal arts university and a polytechnic. As a resource poor and land locked country, the Swiss federal government prioritized technical education to give the edge to their industrial strategy of high specification production and value-added quality. It is however important to note that from the beginning as well as today, the *Eidgenössische Technische Hochschule* has a school of humanities and social sciences. The school housed Jacob Burkhardt for a time as well as historian Karl Meyer. The curriculum involved breadth requirements, but it is clear that this is not *per se* a solution to question of what form the relation between these kinds of knowledge should take.

In 1851, the Swiss Federal Council planned to establish a polytechnicum for economic progress in order to catch up industrially with its neighbors. Rapid modernization was to be pursued through technical expertise, formed in a laboratory for social, economic and environmental experimentation and engineering. In 1855, ETH was only a teaching institution. In 1880 laboratories were added and by 1908 the first Ph.D. was granted; three years later the Polytechnic became a *Technisches Hochschule* (a technical institution of higher learning), whereas the other federal institute in Lausanne, EPFL, is still a polytechnic, or *fachschule*.

After the Great War, relations with industry grew and the role of the ETH in the modernization project increased. Joint funding of institutions began and so too ETH’s part in the “*Geistige Landesverteidigung*,” the ‘spiritual’ or ‘intellectual’-national defense. The role of the ETH as symbol for Swiss modernization is hard to overplay. The very fact that the Institute has its own history department, that is to say, a department dealing with the history of ETH, goes some way to highlighting ETH’s self-conception. One great catalyst for this role in Swiss cultural life, in addition to the economic and political, is the position of the ETH relative to this *Geistige Landesverteidigung*. This was a self-defense against the rise of both

Nazism and Communism. The point is that technical education was seen as the means of both advancing economic progress, as well as national pride, economic and military defense, and through this economic prosperity to form a democracy characterized by “harmony” and not by “revolution”; better living through chemistry, mechanical engineering and natural sciences, the disciplines of the first six Ph. D degrees awarded.

One of the characteristics of the Department in which I conducted fieldwork is that it was in Basel and not Zurich, the first ETH Department to be set well outside the city. I will address the context for this later, but first it is worthwhile getting a sense of the intellectual and work setting of this city, relative to the national project of modernization embodied by ETH and set to work in the capital.

Lionel Grossman’s cultural history *Basel in the Age of Burkhardt: a study in unseasonable ideas* gives an account of the place of higher learning, technical education and industry within the city. According to Gossman, Engels, passing through in 1841, felt the anachronism of Basel and its political conservatism, in contrast to the dynamism of Zurich. Zurich’s dynamism helped bring about confederation in 1848, in which Basel reluctantly took shelter. From this position she gave a last glance at her reflection in the historical mirror of her self-perception as a *polis*. Gossman puts it in the following way:

“It would not be farfetched to argue that the state at Basel, in contrast with the France of Louis XIV or the Prussia of Frederick the Great ... was not an end but only a means of securing adequate conditions for the real business of life, which was not after all politics, as it had been in the ancient republics that Basel liked to compare itself with, but the private sphere of trade, work and family life.”¹⁷⁵

Writing of these means for securing the conditions for what counts in ‘life,’ Gossman describes the manner in which this concentration on practical affairs of trade “paid off in the form of a flourishing economy that procured spectacular rewards for a few and some rewards for almost all.”¹⁷⁶ The key sectors of the Basel economy were the manufacture of silk ribbons, introduced in the early 1600s by two Huegenot refugee families (the Battiers and the Passavants) and the chemical industry, which dominates the city today, developed in the mid-nineteenth century out of the local dye works.

Interest in education as an element in civic life in Basel was rejuvenated contemporaneously with the overthrow of the Napoleonic order. The early 19th

¹⁷⁵ Lionel Gossman, *Basel in the Age of Burkhardt: A Study in Unseasonable Ideas* (Chicago: University of Chicago Press, 2000), 31.

¹⁷⁶ *Ibid.*

Century was a moment in which the political, cultural and economic effects of the Revolution had to be reckoned with; Germans, such as we saw with Wilhelm von Humboldt, contrasted an indigenous, non-revolutionary, cultural route to maturity with the bourgeois individualist ramifications of the revolution in France. Nevertheless, those “bourgeois” values emanating from Enlightenment, like science, analytic reason and the technical domination of nature, were recognized as not mutually exclusive with the political conservatism of the parochial *communitas*, and were recognized as necessary for the community’s prosperity. The Basel elite were producing businessmen, rather than professionals or academics, and whilst trade was always the city’s main cultural achievement, the effect of the neohumanist reaction to Enlightenment in Basel produced a desire from the city’s political powers to balance practical know-how with a more classical pedagogy.

I am interested in this story of late 19th Century pedagogical models for a ‘flourishing community’ in part as a way of understanding the oft repeated trope that ‘Baselers’ in the early 21st Century wished “not be left behind,” technically and scientifically speaking. This wish follows from the tumultuous moment in the mid-19th Century where denunciations and radical protests aimed at shutting down the University of Basel were followed just a few years later with the establishment of the ETH, which the city state of Basel (*Basel-Stadt*) had unsuccessfully tried to have located within its limits. The project of modernization fell to a centralizing force in Zurich, through *purely technical means*, rather than to a city-elite in Basel, organized around the principles of neohumanism.

The granting of the ETH to Zurich was, however no doubt for reasons in addition and more forceful than pedagogical philosophical ones, namely, political centralization. That being said, the division of thought-labor, and the inferiority of the humanistic universities of Basel and Zurich with respect to its younger technical cousin, marked the flight of specialization in the winds of the mid-19th century. Of neohumanism, Gossman writes:

“the neohumanist ideal of education as a full development of the entire human personality was clearly equally incompatible with the *ancien regime* division of human beings into ranks and orders, each of which has its function in the state, and with the abstract individualism of bourgeois society, which posits the formal equality of all, but may actually require a high degree of specialization of each particular individual. To that need, various Enlightenment and Napoleonic programs of technical education had attempted to respond. Neohumanist education was thus a complex project,

hostile both to the ancient regime and to the new bourgeois order announced by the French revolution.”¹⁷⁷

Higher education in German speaking lands today is being brought into line with a European agreement on educational forms and standards. This has been settled in the Treaty of Bologna. Within a week of arriving in Switzerland, and every week until I left, mention was made of the de-formative effects of this Treaty on the future of education.

Utility.

The Swiss had to wait a half century after the founding of the University of Berlin to found their national university. The United States waited a further two decades to ‘reconstruct’ a nation with new institutions of higher learning. In the US, the university as organizational form emerged in distinction to the pious Protestant college, during a period of reform from the 1860s.

During this period, three forces came to bear on these institutions: europhilic discontent, available national wealth and alarm over declining college influence.¹⁷⁸ These forces produced a motion away from evangelical piety within US education.¹⁷⁹ The motion was produced by individuals receptive to European scientific and educational developments, as later demonstrated in the euphoric reception of German Universities at the World Fair Chicago (1893) and St. Louis (1904). These developments, as well as the interest in German models of research, were deemed both necessary and productive as a counterweight to what were considered to be the cruder tendencies manifested in the surrounding American educational setting.¹⁸⁰ These tendencies included what Larry Veysey has characterized as, the “disfavor of practical men for learning.”¹⁸¹

The mid-19th Century US did, however, see an increase in the number of those interested in educational developments in Europe. As Veysey writes, Henry P Tappan, on assuming the presidency of the University of Michigan, had “prematurely announced the role of German institutions as models for American higher education.”¹⁸² Whilst perhaps premature, this model was a powerful orienting marker. Germany provided an orientation and guide to the purposes of “pure” research, an orientation that retained a fundamental subjectivational core.

¹⁷⁷ *Ibid*, 74.

¹⁷⁸ Laurence Veysey, *The Emergence of the American University* (Chicago: University of Chicago Press, 1965).

¹⁷⁹ *Ibid*.

¹⁸⁰ *Ibid*, 2. See also Cyrus Townsend Brady, *The True Andrew Jackson* (USA: J.B. Lippincott Company, 1906), 133-155.

¹⁸¹ Laurence Veysey, *The Emergence of the American University* (Chicago: University of Chicago Press, 1965).

¹⁸² *Ibid*.

Utility and public service, in the post-Civil War reconstruction era, were considered to be the truly “American” additions to the German educational ideal. Up until the late 1860s “science” in positive and material senses, was a threat to religiously oriented educators. Accommodations had to be made such that the pursuit of scientific knowledge could be rendered culturally and politically defensible; that is to say, so that these new institutions could gain material support. As Hofstadter and Metzger show, there were a series of accommodations made which one could characterize as constituting a situation of what came to be known in the 20th Century as ‘value pluralism.’ Old institutional and organizational unity rooted in religion, exercises of recitation and classical learning, gave way to a proliferation of subjects, each considered as equal. ‘Toleration’ would reign in the American democratic university, as long as everyone left everyone else alone. Toleration for diverse subjects of study was frequently justified on the basis of an orientation to the “real.” Veysey meticulously documents the rise from 1880s of references to “real life” correlated to conceptions such as *efficiency* and *efficacy*.

Reform of the university in the service of the real had aims of civic virtue, preparation for work, and the rational solution to public problems. This pluralism was entirely compatible with progressivism; JB Johnson, dean of engineering at the University of Wisconsin wrote in 1899,

“creature comforts, ante-date culture and sweetness and light are not to be found in squalor or poverty. Scientific agriculture, mining, manufacturing and commerce will, in the future, form the material foundations of all high and noble living.”¹⁸³

There was a danger, however, that pluralism married to a materialist conception of progress would descend into discursive value relativism. As DS Jordan wrote in 1899, “it is not for the university to decide on the relative values of knowledge. Each man must make his own market.”¹⁸⁴ This relativism was back stopped by a permanent demand for justification in economic terms.¹⁸⁵ John Dewey described the situation in 1902 plainly:

“Institutions of higher learning are ranked by the obvious material prosperity, until the atmosphere of money getting and money spending hides from view the interests *for the sake of which* money alone have a place.”¹⁸⁶

¹⁸³ Quoted in Veysey, *The Emergence of the American University*, 1965.

¹⁸⁴ *Ibid.*

¹⁸⁵ Cf. Veblen, *The Higher Learning in America: On the Conduct of Universities by Business Men* (New York: Huebsch, 1918).

¹⁸⁶ Quoted in Veysey, *The Emergence of the American University*, 1965, 346, (my emphasis).

This relation is crucial to Dewey's understanding of education as an ethical and political phenomenon. It is one in which the question of the *sake* for which a practice is done must always be held in view and thus returns us to our discussion of the relation of knowledge, life and flourishing from the previous chapter. As Veysey eloquently put the problem of pluralism and judgment;

“A policy of adjustment to ‘real life’ permitted no independent definition of excellence. Indeed it failed even to provide a standard for judging competing definitions of ‘real life.’”¹⁸⁷

Dewey and *Bildung*.

Dewey, as an American who engaged seriously with both the German intellectual inheritance and the ‘real situation’ in America, is our most well-placed and relevant scholar. In his *Democracy and Education* of 1916, community cooperation over ends is considered possible only through the mechanism of ‘communication.’ What Dewey means by communication, however, is not the communication of settled positions from a pre-political sphere, which can then be arbitrated politically, through the force of a communicative rationality. Rather, the meaning of the term “to communicate,” within that book, is to cultivate and to be capable of being reciprocally cultivated. I see, in other words, the residue of the concept of *Bildung* from Dewey's youthful engagement with Hegel. The context of work is increasing industrialization, complexity, specialization of knowledge, social atomization and individualization. There are no pre-political associations as used to exist in townships. Rather, a common goal and purpose for our institutions has to be projected, in order to then make the organizational formation appropriate for the political and ethical solution to the problems that arise from complex, specialized work, under conditions of fragmentation.

Under conditions of complex, differentiated, industrialized societies, the revival of “democratic publics,” for Dewey, presupposes a reintegration of society that can only consist in the development of a common consciousness for the pre-political association of all citizens. “Pre” here, means not evolutionarily, temporally, or developmentally prior, but rather “propaedeutic.” Educational institutions are exactly such politically propaedeutic institutions, which are not themselves the scene of politics proper. Without a consciousness of a projected political life, however, rooted in an ethical relation propaedeutic to the political, democratic procedure cannot be conceived as a collective means addressing breakdowns in collective life.

¹⁸⁷ *Ibid*, 81.

For republicanism, in either an Aristotlean or Hegelian understanding of the *Sittlichkeit* or *ethos*, there is no separation between political and ethical life. The political virtues are necessary for life. For Dewey, this is not possible in a value plural world, and furthermore, in a world in which we do not definitively know what is the model of our becoming. As such, the human thing exists prepolitically in a multitude of forms of association and the interconnection of these prepolitical associations is what makes possible a democratic politics. These associations, however, must simultaneously exist at a second level where individuals who participate in prepolitical associations (families, churches, schools, hospitals) conceive of their actions as being justified through and oriented towards communal goods.

Dewey's starting point is '*voluntary cooperation*' in an activity. Unlike Republicanism, this cooperation in life is not yet the 'political,' but likewise he dismisses the idea that political will can only be found arithmetically, the will of the majority, on the premise that individual ends are so incongruous that there could be no other way to arbitrate political decisions. There is an "antecedent intersubjectivity of social life," as Axel Honneth describes Dewey's philosophy, that conditions what and how political activity can be done.¹⁸⁸ The individual maintains the social whole through activity and this individual activity is an embodiment of the "ends of society." All individuals possess, "the entire sovereignty through which all jointly as a people becomes the sovereign bearer of power."¹⁸⁹ Every citizen is a sovereign and so government is not an autonomous representation of this sovereignty, but its living expression.

Again citing Honneth's reading, for Dewey, as for Hegel, as for Plato and Aristotle, "citizens are said to attain freedom through self-realization in conformity with the ethical ends that together constitute the ethical life (*Sittlichkeit*) of the polity."¹⁹⁰ It is only the means which differ, meaning that for the ancients, this ethical self-realization was confined to a few who would conduct the rest; for Dewey, mass education was the route to mass political and ethical perfectibility.¹⁹¹ What is important about Dewey's formulation is the recognition that division of labor entails prepolitical and ethical work constitutive of politics proper.

¹⁸⁸ Axel Honneth, "Democracy as Reflexive Cooperation: John Dewey and the Theory of Democracy Today," *Political Theory* Vol. 26, No. 6 (Dec., 1998): 763-783, 767.

¹⁸⁹ *Ibid.*, 768.

¹⁹⁰ *Ibid.*

¹⁹¹ Cf. Alexis de Tocqueville, *Democracy in America* (London: Signet Classics, Penguin), Chapter 18 "Equality Suggests to the Americans the Idea of the Indefinite Perfectibility of Man."

As many scholars have pointed out, the defect of Dewey's early Hegelian theory of democracy is that there is a teleological account of human activity embedded the understanding that self-realization in a cooperative division of labor *will* lead to the living expression of a collective political and ethical life. So what then is the mechanism for the compatibility of human self-realization, if not inter-subjective speech in the political sphere?

Dewey's understanding of this mechanism is laid out in his theories of socialization in *Human Nature and Conduct* and *Democracy and Education*. Reference groups esteem and cultivate certain capabilities as habits. The fact that an individual will belong to multiple reference groups is the mechanism that ensures 'socially useful' habits of action are formed. Hence, Dewey's example of a thief in a gang is that the thief cultivates her habit at a high social cost, since she narrows herself as a social being. Self-realization as a political and ethical end is therefore in a mutually constitutive relation with democratic and plural forms of life. These forms of life can mutually enhance each other if they can be brought into a good relation;

“the intelligence of the solution to emerging problems increases to the degree to which all those involved could, without constraint and with equal rights, exchange information and introduce reflections [...] democracy represents the political form of organization in which human intelligence achieves complete development.”

Dewey in the *Public and its Problems* identified the institutional form and mechanism for the activity of intelligence in thinking through problems of human life. This form and mechanism was modeled on the rational solution of technical problems in laboratories, but now transposed to other public institutions. How can the procedure of science (method) be reconciled with the idea that a self exists as an ethical and political end only in community of cooperation capable of reflection on those ethical and political ends? Of this Axel Honneth writes the following:

“Social action unfolds in forms of interaction whose consequences in the simple case affect only those immediately involved; but as soon as those not involved see themselves affected by the consequences of such interaction, there emerges from their perspective the need for joint control of the corresponding actions either by their cessation or by their promotion.”¹⁹²

Like the notion of a *milieu*, a public comes into existence to recognize and either to regulate or to impede an action experienced at a certain distance, or thought to

¹⁹² Axel Honneth, “Democracy as Reflexive Cooperation: John Dewey and the Theory of Democracy Today,” *Political Theory*, Vol. 26, No. 6 (Dec., 1998): 763-783.

have certain problematic spatio-temporal effects. The relations between the organizations funded to do science, on the basis of their work being useful, and thus producing public effects, and those environments in which such work exists became increasingly challenging to manage.

Utility: Science & Industry (1950s–1980s).

The US National Science Foundation was created in 1950 with a clear statement of purpose: “to promote the progress of science; to advance the national health, prosperity, and welfare; to secure the national defense ...” It was formed as part of the post-second world war and cold-war response to the need for reconstruction, both material and ideological. Milton Lomask, the author of *A Minor Miracle: An Informal History of the National Science Foundation* points out an editorial from *Industrial and Engineering Chemistry* entitled “How firm a Foundation?” The editorial posed three questions for interrogating the successful functioning of the new National Science Foundation: the scope of the subject matter, the evaluation of projects, and the financial basis. Lomask wrote that

“What most worried editor Walter J Murphy was that the same clause in the law that directed the Foundation to ‘initiate and support’ basic science also obliged it to appraise the impact of such research” upon industrial development and upon public welfare.”¹⁹³

The concern was the well-worn commonplace of the ‘golden goose’ and how much golden grain the Federal government would be willing to give relative to what kind of assessment. The concern for this particular editor was the following:

“Many of the very real links between basic research and the general welfare are too subtle and interconnected to be traced by human perception. We know they are there, but our knowledge contains a strong element of faith that goes beyond what can be measured and thereby proved.”¹⁹⁴

One might disagree with Murphy’s faith position, but the question as to what the relation between knowledge and welfare is, what a general welfare is and the problem of naming criteria of evaluation was germane in 1950, likewise when Lomask was writing in 1975 as much as it is relevant to the first decade of 21st Century.

In the early 1980s there were major concerns in government and industry about U.S competitiveness. A National Academy of Science panel was convened to recommend how government, the academy and industry could cooperate better to

¹⁹³ Milton Lomask, *A Minor Miracle: An Informal History of the National Science Foundation*, National Science Foundation: Docs., U.S. Govt. Print. Off., 1976, 40.

¹⁹⁴ *Ibid*, 41.

remedy this situation. The recommendation was not novel; interdisciplinary centers for engineering research. However, the scope, scale and level of funding of the recommendation were new. The scope had three primary goals: to focus on “interdisciplinary problems,” to build closer ties between industry and academy and to provide a “different” education for, what they termed, a *cadre* of engineers. This recommendation would find a form in National Science Foundation funded *Engineering Research Centers*. Bozeman and Boardman remind us of the following fact:

“in 1983, the academic department and its laboratories was the place where university research was performed. Today, there are hundreds of university research centers and about one-third of academic scientists and engineers are affiliated with a multi-disciplinary, and often multi-university, research center.”¹⁹⁵

ERCs.

The origins of the Engineering Research Centers, as a form for work, were in a 1983 presentation made by the Committee on Science, Engineering and Public Policy to the Office of Science and Technology Policy. The presentation was about new capabilities in processing chips. The latter body was established in 1976 with a broad advisory mandate to the Executive Office of the President. The former was a joint committee of the National Academies of Science and Engineering and the Institute of Medicine. According to participants, the presentation gave urgency to a perceived need to calibrate the training of engineers to developments in technology, especially in information processing technologies. Training and technology development were seen as in need of being brought into a mutually formative relation for the purpose of “future industrial success.”¹⁹⁶ This mutually formative relation was encouraged by establishing spaces of “cross-disciplinary” training, work and research for graduate students, post-docs and Principal Investigators. “Cross-disciplinarity” was not the aim per se, but rather multiple skill sets working together for the purpose of “better system design, optimization and integration” in the context of a political economic demand for global competitiveness. As Lewis Mayfield wrote in 1987:

“The goal of the centers is to improve engineering research so that U.S. engineers will be better prepared to assist U.S. industry in becoming

¹⁹⁵ Bozeman and Boardman, “Managing the New Multipurpose, Multidiscipline University Research Centers: Institutional Innovation in the Academic Community,” *Report of the IBM Center for The Business of Government*, (2003).

¹⁹⁶ George A Keyworth II, quoted in Bozeman and Boardman, 2003, 366.

more competitive in world markets. Thus, engineering research and education must be judged by their success in achieving this linkage.”¹⁹⁷

Despite divergent research areas, all ERCs were to have the same characteristics: they would facilitate the flow of knowledge and technology between students, post-docs, PIs and their counterparts in industry, they would be problem based and they would educate engineers as to the “needs of industry.”¹⁹⁸ On their near future vision for the aims of the ERC the NSF wrote the following:

“Recognizing that optimizing efficiency and product quality are no longer sufficient for U.S. industry to remain competitive, these ERCs will optimize academic engineering research and education to stimulate increased U.S. innovation in a global context.”¹⁹⁹

The NSF, from this self-narrative about the aims of these ERCs, today recognizes that optimizing efficiency is *not* sufficient as a means for the end of remaining competitive. It is left as an unknown what, then, can be optimized, if efficiency is not sufficient. What is clear now, after five years of participant-observation in one of these ERCs, is that the core end, industrial competitiveness, is not in question relative to the ramifications of “increased US innovation in a global context.” Whilst this may not be surprising, it is significant given that the NSF also recognizes the necessity of incorporating ‘ethics,’ or ‘social consequences,’ terms which are basically indistinguishable for the NSF, into such centers. What such a relation between ‘ethics’ or ‘social consequences’ and an academic-industrial institution can consist in institutionally, the perspectives which people have of this relation and the problem of forming a working practice is precisely a challenge given the indetermination that Lomask indicated.

¹⁹⁷ Lewis G Mayfield, “NSF’s Engineering Research Center Program: How It Developed,” *Engineering Education* 78(1987): 130–132, quoted in Bozeman and Boardman, 2003, 367.

¹⁹⁸ *Ibid.*

¹⁹⁹ National Academy of Engineering, “Engineering Research and America’s Future,” National Academies Press, (2005).

Chapter Three

Venues: SynBERC and ASU-CNS

“the idea of an achieved Utopia is a paradoxical one ... the paradoxical humor of an achieved materiality, of an ever renewed self-evidence, of a bright new faith in the legality of the fait accompli which we always find amazing ...”

–Jean Baudrillard ²⁰⁰

The last chapter aimed to give an historically based feeling for the relation between the problem I began with in Chapter One and the settings in which I worked; research centers and a technical university. The chapter gave additional dimensions to the interconnection of the problems of the subject of knowledge and the relation between the sciences.

The venues in which I worked were specific ones insofar as they were both oriented, to one degree or another, to the interconnection of these problems. Or minimally, they were both oriented to the latter problem of producing a collaborative relation between the bio and human sciences and I took up this problem as one which needed to be interconnected with the problem of the subject of knowledge capable of engaging this problematic relation.

In this chapter the venues which I describe are the Synthetic Biology Engineering Research Center (SynBERC) and the Arizona State University Center for Nanotechnology in Society (CNS). I will not describe the Swiss laboratory in this chapter since although it was a *site* of research, it was not a *venue*. What I mean by this is that a venue is characterized in its design through its capacity to respond to set of demands or problems. The Department of Biosystems Science and Engineering was a response to the need for Basel to profit from its pharmaceutical base by bringing in a new department from the knowledge production center at Zurich. I will describe the lab in Basel in Chapter Six as part of an account my efforts to implement the methodology devised in the venue at CNS. At both SynBERC and ASU-CNS, the problem of the relation between the human and bio sciences was central to the design of the centers. In that sense, they were venues for this project.

²⁰⁰ Jean Baudrillard, *America* (London: Verso, 1989), 75.

In each of these venues, work was taking place on forms of possible knowledge about *bios* and *anthropos*, in each of these venues there were jurisdictional and subjectivational demands which I wish to render visible with respect to the participant-observation I conducted.

Synthetic Biology Engineering Research Center (SynBERC).

Synthetic biology is the latest among a set of sub-domains in the molecular sciences to emerge after the genome sequencing projects of the 1990s. Those sequencing projects produced a large amount of data, however, that information needed to be put to further use. Publicly and privately funded science, it is reasonable to suppose, should produce public and private goods. Aside from the inherent intellectual interest of inquiry into biology, much of the resources devoted to biological sciences today, are oriented to the utility of such knowledge for the amelioration of a range of health and energy problems. Such support is also oriented to the production of commercial value related to the amelioration of such problems.

Such biological work required new forms of collaboration between a range of specializations; computational biologists, molecular biologists, bio-engineers, chemical engineers and others. Collaboration between these research areas required an organizational form in which to bring these specialized forms of knowledge together. It also requires a way to frame the problems in order to do this collaborative work. Proponents of synthetic biology provided the framing and the NSF provided funds, \$16 million over five years (plus industrial and university matching funds) and an institutional form, the Engineering Research Center. I outlined previously some of the origins of the ERC form, and the broader transformations which accompanied the development of such institutions, and in the next chapter, I will describe the engineering activity of synthetic biology.

The initial plan, in the original SynBERC proposal, was for the organization to have three scientific “thrusts,” functionally distinct and yet integrated. These would be integrated for the purpose of designing and assembling biological functions from standardized biological parts.²⁰¹ All NSF Engineering Research Centers have to be divided into “thrusts,” a metaphor of long duration within the NSF. The mechanical metaphor of force is tied to the role that these centers are supposed to have in guiding the direction and magnitude of scientific activity.

²⁰¹ Barry Canton, Anna Labno, & Drew Endy, “Refinement and Standardization of Synthetic Biological Parts and Devices,” *Nature Biotechnology* 26 (2008): 787–793.

From the ERC self-narrative, it is clear that the object being ‘thrust’ forward is US industry and metonymically, the nation.²⁰²

The NSF’s vision of SynBERC, however, was exciting; that the Center be not only a dynamic form for solving technical scientific questions, but that it should have the resources and capabilities to be self-observant of its own practice, relative to the wider, mutually formative, relations that constitute it. I hoped that this Center would be an ‘assemblage.’ That is to say, that the claims from the NSF and the synthetic biologists of inventing a ‘novel’ approach to biological research and to concomitantly engage the relation of this research to complex ‘real world’ situations, was true.

It seemed plausible, in 2006, that the way these novel relations had been conceptualized in advance by these biologists, the NSF and the human scientists involved, could be actualized. It was hoped that these heterogeneous elements—a formal structure provided by the NSF, older and newer molecular techniques, and novel uses of engineering schemas in biology along with a novel approach to ethics in an anthropological mode—could be combined to produce capacities not formerly present for working on the relation of biology and the ethical and political difficulties of its constitutive environments.

These concerns are usually externalized onto various publics and the labor of ‘interfacing’ with these concerns outsourced to social scientists, often of the opinion polling kind.²⁰³ It is precisely this way of relating science and ethics that Human Practices hoped to change. It was precisely a different mode of engagement that Rabinow and the NSF agreed to. Let me be explicit; without this agreement to try something new, we would not have been involved.

SynBERC is a second generation Engineering Research Center, comprised initially of five universities, and by 2010 of six. It is administered from Berkeley and is geographically clustered in the two biotech hubs in the US; the Bay Area

²⁰² “The only question in this journey is: how far can we go in the extermination of meaning, how far can we go in the non-referential desert form without cracking up and, of course, still keep alive the esoteric charm of disappearance? A theoretical question here materialized in the objective conditions of a journey which is no longer a journey and therefore carries with it a fundamental rule: aim for the point of no return. This is the key. And the crucial moment is that brutal instant which reveals that the journey has no end, that there is no longer any reason for it to come to an end. Beyond a certain point, it is movement itself that changes. Movement which moves through space of its own volition changes into an absorption by space itself - end of resistance, end of the scene of the journey as such (exactly as the jet engine is no longer an energy of space penetration, but propels itself by creating a vacuum in front of it that sucks it forward, instead of supporting itself, as in the traditional model, upon the air’s resistance). In this way, the centrifugal, eccentric point is reached where movement produces the vacuum that sucks you in. This moment of vertigo is also the moment of potential collapse. Not so much from the tiredness generated by the distance and the heat, as from the irreversible advance into the desert of time. *Tomorrow is the first day of the rest of your life.*” Baudrillard, *America*.

²⁰³ Dave Rejeski from the Woodrow Wilson Center was added to Scientific Advisory Board in 2011

(Berkeley, UCSF, and as of 2009 Stanford) and the Boston area (MIT and Harvard). Between 2006 and 2009 Prairie View Texas A&M (PVAMU), a Historically Black University, was a partner institution, but since 2009 was demoted in status to one of 13 Affiliated Outreach Institutions, including High Schools, Colleges and other educational programs.

As far as the timing of the SynBERC institutional form is concerned, it was in November 2004 that Jay Keasling applied for a National Science Foundation (NSF) grant to setup a new Engineering Research Centre (ERC). Between November 2004 and the start date of the ERC in August 2006 a number of changes occurred to the proposal. The initial idea for the ERC was to have three integrated yet functionally distinct scientific thrusts to achieve the stated goal of SynBERC; to “develop foundational understanding and technologies to build biological components and assemble them into an integrated system to accomplish a particular task.”²⁰⁴ These scientific thrusts—parts, devices and chassis—were to be integrated towards the goal of standardization and characterization of biological components, which would be useful for building biological systems. The ambition was, and is, to list these components on an open source searchable registry. The goal, of standardizing biological engineering akin to mechanical engineering, was to be driven forward by collaborative work on two ‘testbeds’; one testbed was to re-design *e.coli* bacteria so as to target tumors, the second was to re-design bacteria as microbial factories, for the production of drugs and other “molecules of interest,” such as fuel, through hosting re-worked metabolic pathways. These projects were designed to test the feasibility of such a standardized approach to engineering biology, as a significant improvement in how biology addresses what the biologists referred to as “real world problems.”

SynBERC is comprised of multiple scientific sub-disciplines, diverse forms of funding, complex institutional cooperation and intensive work with governmental and non-governmental agencies. Although most immediately it is a National Science Foundation Engineering Research Center, it is more broadly embedded as part of the system of Federally Funded Research and Development Centers. “Center” here means “operated, managed, and/or administered by a university or consortium of universities, other not-for-profit or nonprofit organization, or an industrial firm, such as an autonomous organization, or as an identifiable separate operating unit of a parent organization.”²⁰⁵ SynBERC is administered from the California Institute for Quantitative Biosciences (QB3) which is one of four Governor Gray Davis Institutes for Science and Innovation. These Institutes were designed as a catalyst for the “California Bioeconomy” and

²⁰⁴ SynBERC, “Strategic plan,” October 2006.

²⁰⁵ U.S Code of Federal Regulations, Title 48, Part 35, Section 35.017 by universities and corporations.

represent what the funders see as an unprecedented partnership between the State, UC, and California Industry.

The precedent for work in SynBERC was among other things, the technical success of the Keasling laboratory's work on the anti-malaria drug artemisinin. The artemisinin project is the exemplar on which the claim that synthetic biology can "solve real world problems" is based. Keasling had research administrative credentials that gave the NSF confidence in his ability to direct such an enterprise. From April 2005 to June 2009 Keasling served as Director of Lawrence Berkeley Lab's Physical Biosciences Division, he had joined that division in 1992 and in 2002 became the first head of its Synthetic Biology group. He became Berkeley Lab's Acting Deputy Director in March 2009, and was also a founder of Amyris Biotechnologies. The Bill & Melinda Gates Foundation awarded the NGO One World Health a five-year grant of \$42.6 million in December 2004 to manage a research and development collaboration with Amyris and Keasling's lab to develop the technology platform for producing artemisinin.

It seemed as though SynBERC deserved the name 'assemblage' insofar as it was composed of heterogeneous elements (individual labs, projects, careers, technologies) that retain their original properties even as they are combined and re-combined into new inter-relations (e.g. a series of labs and personnel are connected through a new technology and the fact that they operate in the "parts" thrust of this virtual center). In addition, it was clear that those individual parts, whilst put into one configuration were able to be re-configured into another. With the success of the work in producing artemisinin, the Keasling laboratory were able to leverage this technology, connections and personnel in order to take a core role in one of three Department of Energy Bioenergy Research Centers, the Joint BioEnergy Institute, as well enter into relations with start-up companies which share personnel with these labs, such as the biofuels company LS9.

In response to the initial proposal from 2004, and in verbal communication between the NSF and Keasling, a fourth thrust, in addition to the three extant scientific thrusts, was added and integrated into the design of the ERC, in order to approach the wider foundational and applied questions that this biological engineering practice raises, ranging from ethics to legal questions. It was crucial to the NSF that SynBERC be not only a dynamic form for solving technical scientific questions, but that it have the resources and capabilities to be reflexive about its own practice relative to the wider mutually formative relations that constitute it. The support for this kind of reflexive work, we will see, is exemplified in other NSF funded projects, such as the ASU-CNS.

Under the original plan, dated November 2004, a bioethics director, in a position not integrated with the scientific practice, was to ensure oversight of so-

called “ethical issues” pertaining to SynBERC. Michael Nacht Dean of Public policy at UC Berkeley was named in the position. I was unable to get a clear idea of what exactly changed, however, by 2006 the position occupied by Nacht had been replaced by his colleague public policy professor Steven Mauer. The mandate and invitation offered to Rabinow was the unexpected outcome of a disagreement between the management of SynBERC, including Director Jay Keasling, and Maurer. This turning point, in 2006, and his subsequent invitation into the Center, was an opportunity for Rabinow to find a different medium for and practice of anthropological inquiry.

Such ERCs are not known as venues for experimentation in ethics and anthropology; nor will they be. This situation was a deviation from a standard practice, for the NSF, the engineers directing and working in the center, and the anthropologists. Our engagement with the biological engineers was on the basis of the engagement being a to-be-determined working out of ways to reflect and act on to-be-determined ethical problems which are integrally connected to the innovations developed in the labs. The NSF had mandated such work as a condition of funding the Center. This mandate was consistent with their concern to engage the so-called “broader impacts” of science.

At the end of 2006, Rabinow and Bennett worked assiduously on thinking through how to frame and design an anthropological and ethical engagement with this ERC. The, until then, existing work on the ethical and social consequences of synthetic biology consisted either of intensive, short term meetings, such as the now annual “*SB*” conferences, global events building and showcasing the brand of synthetic biology often with a small contingent of human scientists, of which Rabinow was one of the early engagers (SB1 & SB2) prior to joining SynBERC; or, standing committees whose purpose was limited to protocol review or rule enforcement.

Such work had proven valuable in identifying the ways in which synthetic biology was intensifying already-known challenges in recombinant DNA technologies. These forms were not suited, however, to identifying new challenges as they emerged. Our collective engagement was staked on a need for regular, on-going collaboration in which human and bio scientists reflect together on the significance of work being done in synthetic biology, the environments within which that work is being done, and what problems might be on the horizon.

We did not know how to do this; that was the indeterminate ethical challenge of collaboration on the ethics of synthetic biology. The language used to frame such collaborative engagement, in 2007, turned on getting our colleagues to take seriously the aim to identify challenges as they emerged, and to redirect scientific, political, ethical, and economic practice in ways that could mitigate

future problems and actualize hoped for benefits. This was the initial aim for on-going collaboration as conceptualized by the Berkeley side of the Human Practices Thrust of SynBERC.

The Thrust, however, was divided across two teams; ours at Berkeley, and one at MIT, which nominally was supposed to enhance collaboration across this behemoth institutional set up. The MIT group, directed by PI Kenneth Oye, framed their work as “applied” research, which was supposed to focus on Intellectual Property and on “risks,” including security, environmental and health risks.²⁰⁶ The Berkeley lab’s work was framed, by contrast, as “fundamental” research, first of all framed as work on “Ethics and Ontology,” and with time specified as a series of questions around the organizational form of SynBERC and conceptual work on the different strategies through which the organization was attempting to activate a novel approach to engineering biology.

Furthermore, after some time, a division of labor was agreed upon where the “security” topic, would be shared, with a focus at MIT on ‘risk’ and at Berkeley on ‘preparedness,’ this division appropriately emphasizing safety and ‘risk assessments’ in the former, and concept work, organizational form and ethics in the latter. I worked on this topic and take it up in Chapter Five. What I wish to develop now is the initial design and engagement of Human Practices in SynBERC and to contrast it with ASU-CNS.

“Welcome to SynBERC”: 2007

“Why are you trying to give us *more* problems?!” exclaimed a then graduate student from the front row. “Shouldn’t you be trying to *solve* the problem of communication with the public?” It is late in February 2007 and we are at the first SynBERC “retreat and site visit,” a get-together in Berkeley of all five universities that make up the Center, before the annual NSF audit. The questions were posed by a confident young molecular biologist, now working for LS9, a SynBERC spin-off company competing to bring third generation biofuels into the world. She posed the order, veiled as a question, to Bennett, who had taken the opportunity of the principle investigators being in a closed door meeting, to outline our project and mandate, to the fifty or so students and post-docs who were sitting or standing under halogen strip lights in the foyer of the Berkeley West Biocenter.

Many were working contently on their laptops, some clutched San Francisco souvenirs, some clutched data, and others talked quietly about their projects. The students had been allotted time to give informal introductions of their work, but no

²⁰⁶ For more information see Paul Rabinow and Gaymon Bennett, *Designing Human Practices: An Experiment with Synthetic Biology* (Chicago: University of Chicago Press, 2012.)

one seemed prepared or interested in doing so. A projector had been set up in the make-shift presentation space and Bennett took the opportunity to describe the initial rationale and design for Human Practices in the Center. We had been preparing for this event for months. It was, however, a doubly surprising event for the other students: Many I spoke with were not aware until shortly before the site visit that their work was being funded through this organization. Furthermore, none expected an ambitious vision of scientific and ethical collaboration, which the organization was mandated to realize. The vision articulated in the promotional materials, of educating a new cadre of engineers in a new collaborative discipline, was far removed from the stakes and concerns of the majority of researchers and lab directors. In this moment, the pressing concern was funding whatever it was these labs and students were doing anyway. The premise of collaboration was that there was a need to try a different way of relating science and ethics.

Listening and Speaking.

What would it mean to take the young biologist's enunciation seriously? Minimally it would have to start with her question, why *we* were trying to give *them* more problems. The hope had been that the collective subject position we were forging between the three of us could be enlarged to include others from within the Center. This did not have to be "all" of SynBERC, but the hope was that minimally this experiment in trying to think about science and ethics would include 'some' self-selecting bio-scientists, in the active making of a reflective stance towards the creations in the labs and the relation of the creations to the complex set of relations which could give significance to and render problematic, such creation.

The young biologist's—I assume—unmediated, perhaps quick, reaction could not hear the aim and endeavor. One could argue that the aim was articulated in language that the young biologist did not already know; a language of anthropological ethics oriented by quite specific concepts which were supposed to orient listeners and speakers toward the invention of a different kind of ethical practice. With respect to the point I made in my Introduction, if the vocabulary of ethics can be taken as given one might be able to 'get on' with the task of facilitating communication, if one were so inclined. Regardless of such inclination, the fact that there is common agreement that ethics after ELSI is not already determined, indicates that that the vocabulary of ethics cannot be taken as given and determinate. The projects I participated in, broadly speaking, were responses to this indetermination.

Collaboration, in our development of the term, had a specific meaning. It would come to mean something for those of us who occupied the Human Practices

subject position, which was quite different to the meaning it would often have in SynBERC more widely, or even in the other human science project I participated in STIR. Collaboration could mean; any form of working together.

Collaboration, however, was used in a specific sense within the work internal to our group and in distinction with cooperation. Cooperation is characterized by objects and problems defined in advance of inquiry with a demarcated division of labor. Where problems are known and somewhat stable, a cooperative mode of work functions well and allows existing expertise to then be mobilized. Under such conditions, the question of significance and ends, as well as the capacities needed to realize those ends, are also assumed to be stable. Collaboration is characterized by sustained labor on the common definition of problems and this labor poses the question of which capacities are required to work on these problems, toward which ends?

To listen to the student's enunciation from another side, what can be heard is the claim that the right problems are known, one of which is communication with the public, such that the goods assured by this activity, synthetic biology, will not be impeded. I imagine it is clear by now, but just for the sake of clarity let me re-iterate, I did not seek to produce an *ethnography* of the young biologist's "us" – "why are you trying to cause *us* more problems," but rather to reflect on the breakdowns which enervated the possibility of making a different "we." I reflected on this by looking at how two different projects sought to produce different kind of such a 'we' and to think about their ethical orientation in this aim.²⁰⁷

The demand to which we collectively in Human Practices refused to accept and thus could not fulfill was to give a 'solution,' such as a policy proposal or IP strategy, to the problems we would name. As will become apparent in the chronology of the episodes of this engagement, the Human Practices 'position' initially aimed at participation oriented to intervention. This commitment was based on an agreement that we would find some researchers with whom to identify problems and work together on finding the intellectual means of "remediating" them and arguably, with time, practical means as well.

It is important to be clear here, unlike STIR, and I will return often to the importance of this distinction, there was not a method to be implemented in Human Practices. There was genuinely open question as to whether anthropological inquiry, which fundamentally turns on two terms "participation" and "observation," whether such participant-observation could be in some serious sense part of a scene or setting observed.

²⁰⁷ Marilyn Strathern, "Re-making Knowledge: 'Relations' and Relations," "Tribute to La Pensée Sauvage. Nature, relationships and contributions to indigenous knowledge," *Collège de France*, 14 – 15 May 2012.

To a degree it was clear from the beginning that the question of power relations, institutional position, and resources would be parameters of our capacity to engage in such remediation. Even with such position, resources and power, the aim was ambitious; namely to give a different form to a mode of collaboration between different forms of knowledge and scientists, one which would reformulate the ethical stakes of such work. What this meant in actuality was a simple but troubling question: It is clear that the dominant justifications for knowing and making things today are in terms of monetary value and health. Rather than start from the assumption that synthetic biology was assured to bring about these goods—or to denounce the contrary—the question Rabinow and Bennett posed, and which I learned to take on as my own, and which we developed together, was whether there is a reformulation of the human good—living well and doing well—of which ambitious projects in sciences of life are a part?

Otherwise said, can the question of living well and doing well, in a scientific research center, dedicated to knowing well and making well, only be answered in terms of the consequences of such knowing and making for health and monetary value? This was an experiment in trying to answer that question differently.

Human Practices.

An initial marker, the ancient Greek ‘flourishing’ (*εὐδαιμονία*) served us as an outside to these dominant justifications. I will return again to what I took to be the stakes and difficulties of such an orientation. Nevertheless, initially what we can say is that without collaboration and active engagement such a project was a lost cause. It struck me early on that the challenge to work together, between forms and practices of knowledge, on problems which should be able to be taken as ‘*common*’ is symptomatic of a difficulty of a much broader collective kind.

The day after our informal encounter with the students at the February 2007 site visit, Rabinow laid out our mandate and his vision at the official presentation to the NSF audit team: “We’re not doing social implications!” His tempo was upbeat and filled with urgency. “Almost everybody assumes that’s what we are doing. We are trying to think of a different way of organizing things so that we can actually begin to collaborate with you as things unfold, rather than wait for a catastrophe and complain about it afterwards.”

It seemed possible that his vision and articulation of work across domains of thought could move those who had been so hostile the day before. He asked us to underline a sentence from the SynBERC Strategic Plan: “imagine ways to invent and sustain new forms of collaboration.” He was re-iterating to the students, the Center’s management, the other PIs, and me as I listened, what we had agreed to engage in. At this point he asked a question: “What kind of form could we invent

in a research organization such that anthropologists and ethicists could take part and do this better and contribute to a better form of science?” Our possible contribution was outlined and the stakes of the engagement rested on it being taken seriously: “Our contribution is what we call ‘problematizing’ critical domains of energy, health and security environments. We want to ask what’s happening in these domains and how they may fit together. Just because a molecule moves, or because you can engineer a protein within these domains, what is it that is defining these domains, is it a new conception of life, is it political– economy, is it a new security environment?”

The metonymy of a relation between protein domains and political and ethical domains required both an imagination and seriousness as to what is involved in having the capacities to bring new biological objects into the world.²⁰⁸ “We are going to argue that the new kinds of things that you are bringing into the world and ethics are closely related: What is it you are making? How are you making it? Why are you making it? What form and shape does it take and how does it travel? This raises the challenge then of how to design a form of collaboration such that we would be able to work in some regular fashion with you.”

The intention to change the engagement between science and ethics needed to be signaled with a name that was both true to our purpose and disruptive of received expectations. I say disruptive because the collaborative mandate from the NSF was a chance to re–think ‘consequences’ as the only way of inquiring into the question of the goods of science. Bioethics has traditionally attended to this relation in two ways. It is concerned with the governance of science relative to the problem of a research practice that is capable of violating proper limits.²⁰⁹ As such, limits, which cannot be crossed, need to be named in advance as well as the ‘consequences’ of doing so. Secondly, there is concern within this mode of engagement with questions of distributive justice, of a consequence for a polity.²¹⁰ This form of jurisdiction, where appropriate, is necessary, and the form it takes is a set of rules and power to enforce them. But it was clear that this was not the right form of jurisdiction for the work we wanted to do in this research center. Since the claim of synthetic biology was that it was bringing a new way of engineering

²⁰⁸ A protein domain is part of a protein sequence that is functionally distinct from the overall structure, with the capacity to fold in its specific way, but which is nevertheless a constitutive part of the whole sequence.

²⁰⁹ Albert Jonsen, *The Birth of Bioethics* (Oxford: Oxford University Press, 1998).

²¹⁰ Karen Labacqz, *Six Theories of Justice: Perspectives from Philosophical and Theological Ethics* (Augsburg Publishing Minneapolis, 1986).

biology into the world, it seemed appropriate to us to start from the assumption that the ‘rules’ could not be decided in advance.²¹¹ We needed inquiry.

Living well & doing well: a difficulty.

The term “flourishing,” was important as an orienting outside to ‘biopower,’ although appropriately undefined, marker for the work in Human Practices .²¹² It was also the source of confusion about who were and why we were a part of the organization. It was a sign for a different mode of engagement and was significant for how I was able to transform the difficulty experienced in that first year (not only at the first Site visit, but subsequently, see Chapter Four) into an anthropological problem.

Flourishing was a marker designed to ask whether there is an outside to the two dominant justifications for doing bioscience; amelioration of health and the production of wealth. Flourishing, to my knowledge, was first published as a translation of the Greek *eudaemonia* by Anscombe, in her 1958 classic text, “Modern Moral Philosophy.”²¹³ All ancient Greek reflection on human goods were ‘*eudaemonistic*,’ however it was Aristotle who gave the most thorough account and whose basic distinctions we used as our orientation.

In Aristotle’s conception, *eudaemonia* is an objective state, i.e. a state of being a human in a proper way. This state is not only good for the particular person but is also reflective of what is good about humans understood as rational animals. In order for Aristotle to have such an account of ethics, he had to have an anthropology; if one takes up both of these points as a cultural-anthropological problem requiring inquiry from a pragmatic point of view, then there is a simultaneous inquiry into both what kind of thing humans are and can be, and what the goods for this creature are and can be.²¹⁴

²¹¹ Cf. Faubion on the relation of the non-contradictory relation of the ethical and the *themitical*. One nevertheless may see in my account how an initial enthusiasm for a reconstructive relation of the ethical and *themitical* was dashed. As such this indicates to this author at least, that whilst (pace Zigon and others) the ethical and *themitical* are not contradictory, the affective field in which this dynamic operates serves as an indication of why system maintenance and reproduction is hardy against those wishing to disrupt system practice with ‘other’ concerns. Faubion, *An Anthropology of Ethics*.

²¹² Cf. Our collaborative reply to several critics regarding how ‘flourishing’ can provide a different orientation to the question of ethics in spaces which may otherwise be taken up with a diagnostic of biopower; Paul Rabinow, Gaymon Bennett, and Anthony Stavrianakis, “Reply to the Respondents,” *The Journal of Law and Literature* Vol. 21, No. 3 (Nov 2009): 471-479.

²¹³ G. Elizabeth Anscombe, “Modern Moral Philosophy,” *Philosophy* Vol. 33, No. 124 (Jan., 1958): 1-19.

²¹⁴ i.e. the question of what humans are is inquired into from the point of view of what they do, the nexus of their practices: Immanuel Kant, *Anthropology from a Pragmatic Point of View* (Cambridge University Press, [1798], 2006); Foucault, *Kant’s Anthropology*; Paul Rabinow, “Beyond Ethnography: Anthropology as Nominalism,” *Cultural Anthropology* Vol. 3, No. 4 (November 1988): 355–364.

In Aristotle's working out of the problem he has two related assumptions, which we could not take with us regardless of how powerful the rest of his account of the human good is: The first is that there is a human nature and the second is that this nature is realized to its fullest extent in and through the Ancient Greek *polis*. The *polis* is the teleological culmination of what is good for and about the human, as he suggests in the *Politics*.²¹⁵ Put another way, the Aristotelian tradition is a general moral scheme the structure of which is laid out in the *Nicomachean Ethics*. The scheme involves beginning with human nature as it happens to be, reflecting on human nature as it could be if it realized its *telos* (dependent on the form of communal life provided by the *polis*) and then offers the precepts—but not the directives—of a rational ethics as the means for the transition from the one to the other.

Since we do not live in the ancient Greek *polis*, and with agnosticism with regards human nature, in what way can Aristotle's question—what is the human good and how can we live it?—be re-posed today? I offer not an analytic exercise in definition, but rather how this schema was a resource and orientation for thinking about the problem of science and ethics in a case such as our work in SynBERC; it was, to return to one of our commonplaces, part of an anthropological-ethical *paraskeuē*, or “statements with material existence.”

Meditation & Method.

It worth remarking, briefly, that this kind of statement with material existence – in my case, these dual questions of living well and doing well, which I returned to time and time again over the five years in both this institutional setting and CNS—is in actual fact a meditative form of thinking.

Meditation can be opposed to other forms of thinking or reflection which constitute a particular kind of relation between a subject, truth and the truth the subject claims to know; one is memory, in which a truth, which a subject already knew, is recognized. Psychoanalysis and Platonic philosophy are perhaps the exemplary forms of memory, or “anamnesis” as a way of thinking which constitutes a relation between a subject, knowledge and truth.²¹⁶ This form of thinking is of interest to us, because it is fundamental to the STIR project, which I

²¹⁵ “Every state is as we see a sort of partnership (*κοινωνίαν*) and every partnership is formed with a view to some good (since all the actions of all mankind are done with a view to what they think to be good. It is therefore evident that, while all partnerships aim at some good the partnership that is the most supreme of all and includes all the others does so most of all, and aims at the most supreme of all goods; and this is the partnership entitled the state, the political association.”

²¹⁶ See for example, other than *the locus classicus* in Plato's *Meno*, see *The Seminar of Jacques Lacan: The Formations of the Unconscious, 1957-1958*, 4th June 1958, 399. Here Lacan develops the relation of the subject's anamnesis to the Big Other.

will narrate shortly. The STIR project, in part, is oriented to “latent concerns” when researchers are working. The method offers a conduit to bring out these concerns. STIR is not however oriented by a full “memorial” mode of thinking, since this could only really take place in the philosophic or psychoanalytic dynamic of a spiritual guide or master. Rather, STIR sought to operationalize this form of reflexivity through method.

Method is a form of thinking which states that there are steps through a subject can move in order to know a particular object. As indicated in my reflection on Foucault’s distinction between spirituality and philosophy in Chapter Two, method, which became foundational for philosophy, theology and science, was institutionalized in the work called “research.”

In a meditative manner of thinking, access to truth is known in the form of the examination of reason. This is exemplified in the Stoic tests of thought, such as in Marcus Aurelius’ *Meditations*. If one is trying to re-activate a meditative mode, The *Nichomachean Ethics* also offer resources, insofar as the examination of reason could not be separated from the cultivation of the hexis, of state of character. For students of Aristotle (literally, his students), it was propaedeutic to the master science and the master good, the successful running of the *polis*. Inquiry into ethics was not only an intellectual question of what the good is, but an exercise in trying to be good.

If we accept that we, as readers of Aristotle have no *polis*, nevertheless, the task of ethical inquiry is still propaedeutic to a way of living in common, that some might call a politics.²¹⁷ If, for Aristotle, the ethical was a pedagogical work of becoming those people we need to be in order to realize the virtues of living in the *polis*, today one might say, it is an open question as to what kind of pedagogies we need to live in common, and in what kind of way. The difference is between a human becoming *what she is when she actualizes what she should be* and becoming *that which she has the capacity to be*. The latter has no pre-ordained formal limits. If one were to disagree with the unquestionable good of the intensification of all capacities regardless of what they are and of what we become when we intensify them, then the “form”-question, of what ‘form’ or manner of life we are inventing for ourselves, and that shape what we are becoming, enters back in. The question to then ask is how to *give form* and make a practice, for a collective life, relative to what kind of difficulties in thought and action? The Aristotelian tradition is a resource but not an answer to this challenge.

Examples Bennett and I extended when the demand for clarification came from the graduate students in SynBERC and others during our first encounters,

²¹⁷ Cf. Faubion, *An Anthropology of Ethics*, 86.

regarding what they thought was a strange term, flourishing, were the multiple questions that can be posed even about a project as seemingly worthwhile as the production of the synthetic variant of the malaria drug artemisinin.

Artemisinin is the Keasling laboratory work funded by the Gates Foundation, which served as one precedent and justification for work in SynBERC (as well as the millions of dollars in other funding Keasling secured on the basis of this success). The technical–biological problem of producing the synthetic malaria drug was a major achievement. The details to still be arranged at the time were the distribution plans and the provisions to protect local farmers who grew the plants from which the drug was typically derived.

Aside from hopefully saving lives, we asked if work such as this posed any difficulties that required reflection; We asked questions to the students such as how the biological ‘parts’ that went into making the drug could be shared without restriction within the research community—a core claim of the synthetic biologists—given the intellectual property constraints of the Gates funding. What about resistance to artemisinin which is currently the last line of defense against malaria?²¹⁸ There were additional conceptual questions, taken up further by Bennett about what these biological ‘parts’ are and what contribution this conceptual and material practice whose rationale is “to make biology easier to engineer” will have with regards a broader field of molecular life sciences. I take up this concern in the chapter that follows.

Furthermore, we asked, how this scientific activity—which is justified in practical terms—relates to the kind of lives being formed in part by such technologies; certainly a ‘broad’ question, but one seemingly ‘anthropologically’ appropriate, and, given our mandate, we thought would be one that we could, appropriately, think through collaboratively. In order to give one set of concrete dimensions to this question and as a way of clarifying the stakes of thinking this biology in relation to a ‘life well lived,’ we articulated the serious problem that such biological work, and the techniques developed for it, could also exponentially transform problems in political and environmental ecologies. One such example is the danger of the design of novel harmful pathogens, a known difficulty in the biological community and in the US government.

Testing, Testing.

Whilst I deal with the details of what we called the ‘preparedness problem’ in Chapter Five, I wish now to give a description of how initial efforts to ‘co-labor’ foundered: twenty scientists were sitting around a rectangle of tables, waiting for

²¹⁸ This problem has been shown to have been exacerbated in the last three years.

technical problems with the “*holodeck*” to be solved, taking the opportunity to catch-up with lab mates and indulge in a second slice of “Extreme Pizza.” The Star Trek reference to a *holodeck* is how the administrative director, Kevin Costa, would refer to the videoconferencing link between MIT and Berkeley, which in the first years of the Center’s life was a tool alternately invested with the capacity to produce cross-Center collaboration, or inveighed for its incapacity.

This meeting was, in effect, the second formal Berkeley initiated Human Practices “lunch” to propose a topic for collective work, the first having tabled the topic of the “Registry of Standard biological Parts.” The registry is an online repository in which DNA sequences, which correspond to a design standard and which should be well-enough characterized so that if someone were looking for a particular function, for a particular kind of biological system, they could find it there. Interestingly, this first Human Practices lunch was commandeered by members of the Keasling lab in order to have what they termed an overdue conversation about the lab’s move toward design standards for biological parts, and how the development of such a standard could fit in relative to the work pioneered at MIT. This was fine with us and the event was rather illuminating about the differences in conceptualization as to what a ‘standard’ ‘parts-based’ approach to engineering biology is (I take this up in detail in Chapter Four).

Briefly however, one effect of our participation in the conversation was that when I pursued the topic in the next weeks after the meeting, following up on the conceptual distinctions that seemed to be emerging as to certain differences in design approach between MIT and Berkeley, my questions were rebuffed with the claim that I was “making too much of the difference” and that I was “looking for a conflict where there really isn’t one.” This was of course not actually my aim—to look for conflict—rather I was asking naïve questions about why the MIT and Berkeley approaches seemed to be different.

At the second formal meeting in October 2007, a more direct mode of engagement was tried with around twenty or so members of SynBERC who had chosen to participate. The first portion of the meeting was dedicated to Rabinow presenting on distinguishing types of problems in “security,” broadly conceived. As part of his presentation, he analyzed a short section of a then recently published piece in *Nature*, on security frameworks, a piece co-authored by a SynBERC bioengineer Drew Endy, whose image was being beamed to us from MIT through the *holodeck*.

Rabinow began by putting the piece in a brief historical perspective and proceeded to break it down into components that could serve as better orientation for inquiry;

“If the article had been written 30 years earlier it would have concentrated on safety concerns and countered public anxiety with technical assurances coated with promises of innovation and looming breakthroughs in human health and medicine; ten years ago, as the sequencing of the human genome moved along, it would no doubt have highlighted the commercial and health benefits sure to follow. Assurances would have been promised that these breakthroughs would be handled responsibly; issues of privacy and/or what to do with the new ultra healthy humans about to emerge. The tropes of commercial prosperity and of amelioration of the human condition are invoked in the article’s second sentence:

Improvements in synthesis technology are accelerating innovation across many areas of research, from the development of renewable energy to the production of fine chemicals, from information processing to environmental monitoring, and from agricultural production to breakthroughs in human health and medicine.

The following sentences, however, signal the recognition of security as a significant new factor:

“Like any powerful technology, DNA synthesis has the potential to be purposefully misapplied. Misuse of DNA-synthesis technology could give rise to both known and unforeseeable threats to our biological safety and security. Current government oversight of the DNA synthesis industry falls short of addressing this unfortunate reality.

There are several quite diverse claims juxtaposed in these two sentences. The seemingly innocuous and even tautological assertion of analogy “*like any powerful technology*” is laden with assumptions; the complex irrigation systems of Bali, uranium enrichment, Linux, the printing press and the flush toilet are all powerful technologies that have been directly involved in major shaping of human existence: are they really all alike? However one decides to answer that question, the question is worth posing. Furthermore, the term “potential” has a long philosophic history. Whether DNA synthesis is the kind of thing that has a potential is not clear. Thirdly, depending on how one answers that question will influence how this thing can be purposively misapplied. This claim assumes that there are norms in place that establish such criteria. The claim that it ‘*could give rise to*’ both known and unforeseeable threats is basically meaningless since of course anything could give rise to other things and unless the previous claims were fleshed out this latter claim means little if anything. The claim also rhetorically glides past the hidden or unexamined claim that it is not only the misuse of the technology

that could be dangerous. The phrase “known and unforeseeable,” however, is rich and this is something we should work further on.”

As Bennett put it as we discussed this afterwards, a key point of the exercise was to show how a lack of precision in claims about the problem of security covers over the kinds of fine grained distinctions that would allow the problem to be worked on well.

The room had already been tense because of the technical difficulties and furthermore this was the first time we were attempting to include the MIT group in our way of doing Human Practices work. Several of the researchers, including Endy himself, responded defensively to the analysis, Endy claiming that it was a form of “meta-level jujitsu” and asked Rabinow as to how such careful distinction making can be made “actionable.”

The MIT Human Practices group, in accord with Endy, made the curious claim that precision in the use of terms was “unhelpful in getting things done,” and that “we need to be willing to blur things a bit.” Citing Markus Schmidt’s intervention at SB 3 in Zurich, he told the room that in German there are not two separate words for ‘security’ and ‘safety’ (*Sicherheit*). Another of the Berkeley post-docs, after the lunch said to me, in a fairly patronizing tone, that “in science ‘precise’ has a technical meaning that is different from ‘accurate.’ It means using the same method of measurement in all your experiments,” suggesting that we need to be *accurate* and not *precise* per se. I replied to him that the point of the presentation, had been that statements about the world may turn out not to be accurate, but if your “measurement methods,” to use his terms, or in our terms, the conceptual distinctions you are using, are precise, then you can refine your statements about the world. If you start from imprecise distinctions, and do not work on them, then you have no hope of progressing in saying true things about the world.

The general sentiment from our nominal collaborators was well taken. Wittgenstein’s intervention into 20th Century philosophy showed effectively that *meaning comes from use*, and that concepts are not static. In order to have a use, however, we need to know what we are working on; to know what one is working on, in a self-descriptively indeterminate zone of invention, this requires better, more precise specification of the problem. The Human Practices contribution was the conceptual clarification of some aspects of this problematization of science and ethics.

ASU-CNS.

During the initial orienting phase of our work, in April 2007, Rabinow had been invited to visit the ASU-CNS annual “all-hands” meeting. It was clear from his report that the scale and scope of CNS was beyond that of our three person team on the Berkeley side of SynBERC’s Human Practices Thrust. The Center was established in 2005 with \$6.2 million in federal funds and was the institutionalization of a number of developments in approaches to technology assessment relative to the problematic aspects of what is understood in political science approaches to “Science and Society” as the ‘social contract’ of science.

The CNS, funded by NSF is one of eighteen nano-scale science and engineering centers doing the social science of nano technology, inspired by a piece of legislation passed by the US congress in 2003. This legislation, the 21st Century Nano Technology R&D Act authorized the conduct of social science research integrated with nano scale science and engineering work, and also called for public input on the emerging nano technology agenda. The initial award was renewed for a further five years of work at a slightly higher rate.

Regardless of the difference in scale, however, the activities developed at ASU under the names “Real Time Technology Assessment” (RTTA) and “anticipatory governance” seemed sufficiently close to our idea of what we wanted to do in SynBERC so as to be able provide mutual exchange and support.²¹⁹ It seemed that taken together, our activities could be said to be part of a *common problematization*, meaning that older practices and discourses of the governance of science were being re-constituted so as to produce a different kind of object and practice of intervention, and that our different efforts to constitute such a relation between governance and science could be reflected on as simultaneous responses to older social scientific engagements with the natural sciences.²²⁰ One exemplary older model of engagement, which was being reconfigured at both ASU-CNS and in our Human Practices work, was the Ethical, Legal, Social Implications (ELSI) model of the Human Genome Project.

ELSI.

Remediations of ELSI included efforts to move from the downstream further “upstream,” as in Human Practices, and within the particular ASU project in which I participated, explicitly operating in the “midstream,” meaning working with

²¹⁹ David H. Guston & Daniel Sarawitz, “Real-Time Technology Assessment,” *Technology in Society* Vol. 24, Nos. 1–2 (2002): 93–109.

²²⁰ “It’s the set of discursive or nondiscursive practices that makes something enter into the play of the true and false, and constitutes it as an object for thought (whether under the form of moral reflection, scientific knowledge, political analysis, etc.)” Michel Foucault, *Dits et écrits*: Vol.4. (Paris: Éditions Gallimard, 2001), 456-7.

researchers in on-going projects which have already been approved but which have not yet produced determined outcomes.²²¹

Briefly put, and as Kathi Hanna has written in her review of ELSI in 1995, the medics, scientists, ethicists and legal scholars who were active in the ELSI programs did not have either the moral authority, qua researchers, nor the political authority relative to their position within ELSI programs, to make statements or decisions which would have practical effects. In a situation where human scientists and ethicists are given small research grants to produce speech acts, with little possibility of practical effect, it became clear that in such a situation the engrained habits and practices of the sciences relative to ‘social issues’ would not change. Furthermore, ELSI research was conceptualized as “downstream,” meaning that it took as its object and objective the evaluation of, or deliberation over, the products of scientific research after research has taken place. This then led to a veridictional challenge for those human scientists and ethicists: given these limitations, how can ethicists, social and human scientists make (serious) speech acts that count not only as true but that can be taken on as significant by those scientists with whom such ELSI scholars were supposedly engaging? How, that is, to produce a place, or a position, from which the human sciences can speak?

Welcome to the New American University.

We were a few miles outside of metropolitan Phoenix at the “New American University,” to seek aide in our Human Practices experiment. Today, ASU is a site where the “old” standards, the standards and measures of the Old World have been replaced; the replication of a German ideal has been exchanged for the design of a new organizational form for the pursuit of knowledge and value. Michael Crow, on assuming presidency of the University set out a series of design principles, under the banner of the “New American University,” to overhaul the model of Research in the US. The claim made by Crow, and those making these changes, is that the environment in which the German Research University model was imported to the US has changed, and as such, the model should change. The focus today is on use, invention and access oriented to “social needs.” The criterion of judgment for excellence in the production of knowledge, in this model, is the increase of ‘economic and social development.’

Only a few years out of his Ph.D. Crow was already Vice Provost of Columbia University as well as a Professor of Science and Technology Policy. It was during this period that he established the Center for Science Policy and

²²¹ George Khushf, “Upstream Ethics in Nanomedicine: A Call for Research,” *Nanomedicine* Vol. 2, No. 4 (August 2007): 511-521.

Outcomes (CSPO) in Washington DC, before setting up its central office at ASU. CSPO was an organizational intervention which represents changes to the human, social and political sciences taking place under this new model of research; it is interventionist, use-driven social scientific research into the multiple layers at which science and technology is developed, regulated and used. Three years after Crow moved to ASU, David Guston, a political scientist involved in the founding of CSPO, along with colleague Dan Sarewitz, responded to a major NSF funding initiative for proposals on the societal aspects of nanotechnology. Their proposal, and resulting Center, the Center for Nanotechnology in Society, at ASU, (ASU-CNS), which falls under the organizational umbrella of CSPO, was a combination of European technology assessment, incrementalist US science policy and constructive science and technology studies.

CNS was of interest relative to what we were doing for a number of reasons; whilst they took up their work in the discursively dominant terms of ‘Science and Society,’ they were however developing research and educational activities on a model quite different to ELSI, but on a similarly impressive scale. As such, we were not competing with them in that discursive space, rather we envisaged a relation of exteriority in which the things each project was *not* doing, could act as an object of reflection for the other.

There were two interesting aspects of what characterized the work at ASU; the development of an approach to technology assessment that eschewed ‘consequences’ in favor of developing ‘real-time’ assessment as the mechanism, and developing a *political* imperative and justification for work on the problem of science and society; this imperative was often, although not always, named ‘democracy.’ The political imperative was explicated by Guston at numerous events in term of “maximizing the benefits of science based innovation, minimizing its risks, and ensuring responsiveness to public interest and concerns.” Importantly, this type of statement is equally compatible with technocracy as with democracy.

A Broken Social Contract.

In Robert Cook-Deegan’s terms, the ELSI project consisted of a “new social contract,” a prominent metaphor within the discursive boundaries of the academic field of Science and Society, where ELSI struck an “anharmonic in the cacophonous din of democracy.”²²² This metaphor is pervasive and tends to structure the discourse of those wishing to engage in work on the politics or ethics

²²² Robert Cook-Deegan, *The Gene Wars: Science, Politics, and the Human Genome* (W. W. Norton & Company Inc, 1994), 255.

of science. Those working at ASU-CNS were attempting to remediate what was understood as a breakdown in the ‘contract’ and formulate a different kind of relation, one not modeled on the idea of the democratic *contract*, but rather a something more akin to a democratic ‘dynamic’ between ‘science and society.’ The etymology of ‘dynamic’ (*dynamis*) is important insofar as it means a capacity. The Arizona Center was attempting to transform a broken contract into a set of capacities oriented maximization of benefits calibrated to the “public interest.”

The problem that is figured within the metaphor of the contract is that the 80s and 90s saw a breakdown of what was thought to be a stable relation. David Guston, director of ASU-CNS, with co-author social psychologist Kenneth Keniston, characterized the relation in the following way a decade before Guston began directing the Center;

“government promises to fund the basic science that peer reviewers find most worthy of support, and scientists promise that the research will be performed well, and honestly and will provide a steady stream of discoveries that can be translated into new products, medicines, or weapons.”²²³

The breakdown stemmed from disillusionment, and an accusation; research scientists were, in their representation of a collective sentiment,

“arrogant and self-indulgent, rejecting legitimate oversight of the use of public money, claiming ‘entitlement’ to ever-escalating funding, and unwilling to share responsibility for dealing with the growing deficits, trade imbalances, and other economic ills of their country.”²²⁴

The ground of such entitlement was based on the ‘meritocratic peer review process’ of grant applications to national and international funding bodies, which in the post-war period were to a large extent oriented to the support of collective life, whether imagined in terms of national projects of defense, or ‘internationalist humanitarian’ conceptions of political life, i.e. the contract, from the point of view of funders is science is by *some* people for *the* people. The counter-claim from those requesting funds to “do science,” again in their presentation of the an-harmonic dynamic, is that governance is performed in a manner of ignorant micro-management anathema to the expertise and capacities required to perform the best research. This management comes in the forms of government specification of

²²³ D. Guston and K. Keniston, *The Fragile Contract: University Science and the Federal Government* (Cambridge: MIT Press, 1994), 2.

²²⁴ *Ibid.*

which areas of work should be funded, and onerous reporting mechanisms that waste vast intellectual resources.²²⁵

Boundary Work for a Boundary Organization.

It is with respect to the problematic relation between executive and executor that science policy practitioners and scholars were able to create an organizational niche to facilitate and improve this relation; the boundary organization. Whereas in Science & Technology Studies boundary work was a concept that functioned to lay out how a distinction (like science and non-science) was produced, some science policy scholars took the distinction of ‘science’ and ‘politics’ to be a distinction that exists organizationally and then worked to show how a different organizational form can rework this boundary.²²⁶ There are boundary objects within organizations that perform this work and an organization can itself be a boundary which performs this work. These objects and organizations stabilize a relation through consent to a ‘productive cooperation.’

Boundary organizations, in the conceptualization by Guston and others, try to create conditions to use boundary objects. They encourage the participation of actors on both side of the relation. The boundary organization sits on a ‘frontier’ between two domains, such as “science” and “politics,” and has different lines of accountability to each.²²⁷

It is important that the boundary organization, qua agent, is accountable to principals who authorize this agent to act to create relationships with a third party, such as the other side of the boundary. As such, the boundary is both stable, insofar as the boundary organization serves to establish this ongoing relation between principals, and simultaneously works in its activities to negotiate this relation. The boundary organization rather than using boundary work to isolate itself from claims on its authority, uses boundary work to open itself up to two separate and opposing authorities. Boundary organizations, in the words of Guston, “facilitate collaboration between scientists and nonscientists, and they create the combined political and social order through the generation of boundary objects ...”²²⁸ This, then, is the manner in which it generates its own authority by co-opting agents in this frontier in order to stabilize this relation between the principals to whom it is accountable. The image used is a classic one in STS, that of the Roman god Janus:

²²⁵ *Ibid.*, 3.

²²⁶ Thomas F. Gieryn, “Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists,” *American Sociological Review* Vol. 48, No. 6 (Dec., 1983).

²²⁷ David Guston, “Boundary Organizations in Environmental Policy and Science: An Introduction,” *Science, Technology, & Human Values* Vol. 26, No. 4 Special Issue: Boundary Organizations in Environmental Policy and Science (Autumn, 2001): 399-408

²²⁸ Guston, “Boundary Organizations,” 401.

“The boundary organization is able to project authority by showing its responsive face to either audience. To science ‘I will tell politics you are contributing to their goals and I will facilitate your research goals insofar as I can ensure funds and good will.’ To the consuming principal, who funds this relation, the boundary organization says, ‘I will give you assurance that this research is productive.’”²²⁹

Politics and Truth: Democracy & Technocracy.

For this boundary organization, the design challenge is rendered as a problem of ‘*integration*,’ one of ELSI’s shortcomings and one of the shortcoming addressed in the Socio-Technical Integration Research project. The problematic dimension of integration is the organizational relations predicated on the view that, “science proposes, society disposes.” This could be thought of as a principle of technocracy, to which thoughtful policy makers need to respond, i.e. given the tendency towards technocracy how can science and technology policy be improved? In Guston’s terms,

“Without a robust capacity to conduct Real Time Technology Assessment, society will be unable to maximize the benefits of science based innovation, minimize its risks, and ensure responsiveness to public interest and concerns.”²³⁰

This design challenge is responding to the breakdown in the contract model where it is no longer sufficient to consider the production of knowledge as politically neutral until it is put to use by actors in society. A model, which circulates under the banner ‘co-production’ attempts to render the production of knowledge as a *political* space and as a space amenable to intervention.²³¹ Quoting Robert Dahl, Guston suggests that democratic forms of science would involve “fair input into decisions about their interests.” Democratic forms mean, in Guston’s account, “integrating mechanisms for participation and accountability into science in those places where authoritative decisions affecting interests are at stake.”²³²

Regarding the former model of the ‘social contract,’ it failed with respect to the ordering term, democracy, due to what Guston called in his chapter with Keniston in their 1994 edited volume, *The Fragile Contract*, three principal

²²⁹ *Ibid*, 405.

²³⁰ David Guston and Daniel Sarewitz, “Real-Time Technology Assessment,” *Technology in Society* Vol.24, Nos. 1–2 (2002): 93–109

²³¹ Sheila Jasanoff, *States of Knowledge: The Co-Production of Science and the Social Order*, (Routledge, 2006). Helga Nowotny et al, *The New Production of Knowledge: The Dynamics of Science and Research in Contemporary Societies* (Sage, 1994).

²³² David H. Guston, “Integrity, Responsibility, and Democracy in Science,” *Sci Policy* Vol 1, No.2 (2001): 168-189, 181.

‘categories of tension’ within the democratic governance of science: the *populist*, *plutocratic* and *exclusionary* tensions.

The populist tension refers to that between democracy and truth, where “truth sought by scientists” is figured as one among multiple “value-spheres.” With respect to the conceptualization of truth vis-à-vis plural value spheres, the democratic context of truth sought by scientists, requires that the pursuit of knowledge be brought into relation with other values.²³³ As such democracy becomes the arbitration of the relation of these values spheres, oriented to economic and social development. This highlights the two remaining tensions; the “exclusionary” problem of participation in the game of the governance of the production of truth in the context of democracy: What knowledge is necessary to participate in that political space? Furthermore, this relation of inclusion and exclusion to the ordering of value highlights the third, *plutocratic*, problem. The authors write of the third term in their trio, that rapid development of scientific work may not be the best economic organization to produce overall health, wealth and security. These three parts of the democratic design challenge, with regards to science, have spurred a number of attempts to re-think and re-calibrate technology assessment relative to these relations and this ordering term.

Implicit, although unarticulated in their account, is the inherent problematic relation between truth seeking and democracy resulting from the latter two tensions, populism and exclusivity. If under conditions of ‘democratic’ governance the search for truth is tied to both health and welfare and is bracketed as one among multiple value spheres how, if it at all, does it matter what truth or untruth circulates in a community? Does it only matter with respect to justification by ‘products’? What would it mean to make research ‘more’ democratic in relation to and with effects on the making and pursuit of truths, rather than merely scientific goods that benefit bodies and populations? When we look at the activities of CNS and the particular project I participated in, the Socio-Technical Integration Research (STIR) project, I will highlight questions of the veridictional and ethical orientations ordering STIR and the CNS broadly.

RTTA & Anticipatory Governance.

Guston and Sarewitz proposed in 2001 “Real Time Technology Assessment” which has the goal of developing two capabilities, ‘anticipatory-governance’ and ‘reflexivity.’ ASU-CNS is now an organizational form appropriate to trying to develop these capabilities and I participated in one of its RTTA programs, “reflexivity, assessment, evaluation,” of which the Socio-Technical Integration

²³³ *Ibid.*

Research project is a core element. RTTA is distinguished by the authors from older forms of constructive technology assessment by being “embedded in the knowledge creation process itself.” This embeddedness they suggest allows RTTA to use “more reflexive measures” such as “public opinion polling, focus groups, and scenario development to elicit values and explore alternative potential outcomes.” What exactly reflexivity means I will deal in the next section, as it became a design principle of STIR.

Anticipatory governance attempts to manage emerging knowledge-based technologies while such management is still possible. “Anticipatory governance” seems to have two dimensions to it, ‘value’ and ‘temporality’; the former refers to the conditions of pluralism and the latter to the relation of precedent to prospect. Precedent is taken up through analogical case studies, prospect is taken up by mapping resources and capabilities of the relevant research enterprise. They also elicit knowledge and perceptions of stakeholders in order to pose questions about how ‘values’ are changing and produce analytical and participatory assessment of potential impacts.

Methodologically ASU-CNS sees its work as continuous with the Office for Technology Assessment and the strains of technology assessment developed mainly in the Netherlands under the term “Constructive Technology Assessment” (CTA). After the fall of the Office of Technology Assessment, a space opened up for academics such as Guston to work in new institutional spaces, to create organizations and tools de-linked from formal channels of governance such as congressional panels or legislation.

What we see in an organization like ASU-CNS is the exercise of a role formerly attributed to government, but now considered to be able to be done better by para-governmental, para-academic institutions. The Office of Technology Assessment played a very important role in the development of both the formal governmental channels of technology review and what would then become the much wider field of technology assessment. This is important to remember as we pursue the question of what kind of judgments are formed when doing this kind of assessment.

Socio-Technical Integration Research.

I met Erik Fisher, designer and governor of the STIR project, with my Human Practices collaborators on our first visit to CNS in 2007. At this meeting he detailed his intention to co-ordinate a group of graduate student studies to test a “midstream” approach, which he developed during his PhD; this method engages the limitations of both upstream and downstream ethics. There were some affinities between what we wanted to do in Human Practices and the STIR project insofar as

the PhD project out which it came had similar design parameters: the question of how to take the past practices of ‘upstream’ and ‘downstream’ engagement to the level of ongoing research, how to learn lessons from the limitations of ELSI and how to navigate the question of proximity to the research object. The first and last of these parameters spoke to a shared concern regarding the limitations of what has come to be known as the ‘lab study.’²³⁴ Furthermore, I was interested in investigating work being done under the name “synthetic biology” in Europe as well as the US and the STIR project offered a modal comparison: a comparison of two modes of human science engagement with synthetic biology. Whilst this chapter is not an explicit comparison of the two modes, my work in Human Practices is a crucial context for understanding how I engaged with the STIR project.

Relative to my work in Human Practices I was initially hesitant to get involved in STIR, unsure what to make of key orienting terms such as “socio-technical systems.” I was pursuing an anthropology of ethics vis-à-vis practices of science. I am neither a student of science policy nor a practitioner of technology assessment. Furthermore, the *methodological* aspect to ‘social-technical integration’ seemed adjacent to our anthropological mode of inquiry. Our mode took as its object (and objective) a practice through which a reciprocal relationship between anthropology and biology might be forged. Our objective was collaboration in thinking about the scientific form of life (anthropological and

²³⁴ The ethnographic lab study was one way of bringing observation and inquiry ‘down’ to the level of on-going research. Within the social study of science one can make a broad heuristic distinction; on the one side there have since the late 60s been laboratory studies which focused on the process of knowledge construction in situ. On the other side we have the much older *Wissensoziologie* literature and some of the new directions in science studies. These new directions have taken up the problem of scientific knowledge production in relation to a plethora of ethical and political problems and contexts not delimited by the laboratory. The traditional ‘lab study,’ from a look at publications in science studies in the last twenty years, seems no longer adequate to the contemporary problem of science. Why does this seem to be the case and what then is the problem of science today? ‘Lab studies’ arose from two concerns which could be located at a specific site, the laboratory: Firstly, there was a reluctance to take scientists’ claims about science at face value insofar as they obscured the practice of science, the craft dimensions of how knowledge is produced. Secondly, there was an insight that social studies of science had relied too heavily on objects and problems already known as “social” or “ideological.” The question ‘lab studies’ were interested in was what kind of process involving what kind of practices of thought allow scientists to make sense of their observations and to produce knowledge. There are now, after forty years, powerful and wide-ranging studies which show the process and inter-relations of making scientific knowledge, people and things. One of the effects of these studies as a whole was the emergence of a narrative about a shift from “Mode 1” to “Mode 2” science. This was an effort to put to use the insight into how knowledge is constructed. This shift described and called for the form and purpose of knowledge production to ‘take more into account,’ of context and relations between people and things in the practice and production of science. This is calibrated to a broader discursive, and arguably socio-historical, shift of which Beck’s ‘risk society’ has been one of the most easily recognizable marker. For those narrating a shift from Mode 1 to Mode 2 science, no longer is a description of science considered sufficient, but rather a working of these insights into either scientific practice or policy is considered an aim. For many scholars the social study of science having shown that science is a process, ‘always already’ social, needed to take the next step which was to find a form to engage with the relation of science and political stakes, environments and problems.

biological), the biological objects being produced through such a form of life and the ramifications of such inventions.²³⁵

The attraction of being part of the STIR project was that it was a different response to the same core difficulty that we in the Human Practices thought ought to be posed relative to new sciences and technologies: namely, if external mandates are of limited efficacy for posing political, cultural, social and ethical questions, and furthermore since these technologies are supposed to bring up novel problems for thought and action, there is a ‘need’ for a mode of reflection and engagement appropriate to that indeterminacy. It did not seem a priori impossible that such a methodological mode could attend to the interrelation of the scientific form of life, both anthropological and biological, the medium of their inter-relation and the ramifications of the scientific inventions. Nevertheless, the methodology foregrounded observation of the bio-scientist’s reasoning and not the reasoning of the anthropologist.

Regardless of my hesitations over terminology, the fact that the STIR project was oriented to inquiring into the possibility of changing practices, and hence was logically oriented by (at least) a semantic differential of ‘better’ and ‘worse,’ was significant as an indicator that my work in the anthropology of ethics and the work in STIR might be constitutive of a shared problematization. What I mean by this is that whilst the philosophy and sociology of science has often wrestled with the question of how it is that the code of true/false gets constituted in science, STIR like our work in Human Practices in SynBERC was willing to consider, what one could call following John Dewey, “judgments of practice” and to consider the mode in which such judgments could be made.²³⁶

Midstream Modulation.

Explaining the framework for the STIR studies to the ten participating graduate students, Fisher sought to outline for us a basis through which we could engage collectively with the project. Two elements were highlighted, the conceptualization of *modulation* and the use of a *decision protocol* within the “midstream,” which is to say, within on-going research (FIGURE 2).

the Midstream Modulation framework seeks to contextualize laboratory research within broader distributed processes and we can play a lot with

²³⁵ “Life” (*le vivant*) as both object and practice of knowledge not delimited by meaning of a lived experience for a subject of experience (*le vécu*): C.f. Michel Foucault, “Life: Experience and Science,” in *Essential Works of Foucault 1954-1984, Volume II: Aesthetics, Method, and Epistemology*, ed., James D. Faubion (London: Penguin, 1998).

²³⁶ John Dewey, “The Logic of Judgments of Practice,” in *Essays in Experimental Logic* (Chicago: University of Chicago Press, 1916), 214-281.

the metaphor here, for reasons of brevity we won't open up that can of worms ... generally how we've pitched this is as an alternative to internally viewing the scientific method as non-problematically applied and learned through pedagogical practices and material practices. These practices are very successful and essentially have the effect of closing down curiosity about the social, political and ethical dimensions of an individual's practices. This is not so new, but it's one re-framing. It's also an alternative to top down attempts to control scientific research, not that those aren't good, or are bad, guided or misguided, it's just that they were ineffective. What seems to be called for in the US nanotechnology legislation [2003 Nanotechnology R&D Act] is something that is an alternative. We've hemmed ourselves into this highly constrained space, by highly constrained actors, and the question is: what is possible? What are we directly bringing about? Or, are we just simply understanding and scoping out and mapping the conditions for the possibility of midstream modulation? Such modulation is really change from within. That's where this notion of governance from within comes from, it's the notion that just as modulation happens already, governance from within happens already. And so, if there are going to be midstream modulations, the primary function, or medium through which it would occur would be self-governance and self-changing. So what does it take for self-governance to change on some practical or structurally significant level? This is a realistic recognition that scientific actors are highly autonomous and that's what we're dealing with."

A *decision protocol*, a sheet of paper divided into quadrants, was the device through which conversations and observations between social and natural scientific researchers could be mapped and 'fed back.' As such, and in its architect's own framing, the STIR project was a methodological response to the challenge to develop research mechanisms to integrate '*values*' and '*social considerations*' into science and engineering research practice. The protocol is in effect a model made of four categories: opportunities, considerations, alternatives and outcomes. The act of such modeling begins with a *decision or 'opportunity'* the researcher is facing and then poses the question of what *alternative technical means and broader considerations* could go into the addressing this opportunity or problem (Figure 1). It is temporally flexible and can be operated in indicative and subjunctive moods as well as all three tenses. Things taken into account under considerations are more (although not categorically) 'social' and things taken into account under 'alternatives' are often more (although again, not categorically),

‘technical.’ So what of the manner in which these observations are made? Is the work of ‘operating’ the protocol social, technical, socio-technical? Observations of the ‘social’ and the ‘technical’ with regards the natural scientist’s self-observation includes both social and technical elements, but on the human scientific side, what constitutes an observation as social rather than technical? The challenge for the anthropologist is to render technical observations into anthropological observations, which will necessarily require posing the question of significance; a question to which I will return.

A Model.

In Fisher’s pilot study, a researcher in the nanotech lab in which he was based and with whom he had worked on developing the protocol, had made a comment about a material he worked with being ‘messy.’ This observation became the starting point for questions about the problems with the material, one called ferrocene, and this generated further questions as to whether there are any alternatives to it. Through dialogical probing over a matter of weeks the status of the material was re-thought by reflecting on the use of that material. The material’s problematic role in the setup, the environmental problems with it and the existence of possible alternative means to achieve the desired experimental situation, were named, such that after a time a better alternative, ferrofluid was named. This observation led to a change in research setup and opened a new line of inquiry for the nanoscientist. This was important as an exemplar. It was presented to the graduate student social scientists in the STIR network as proof of concept.

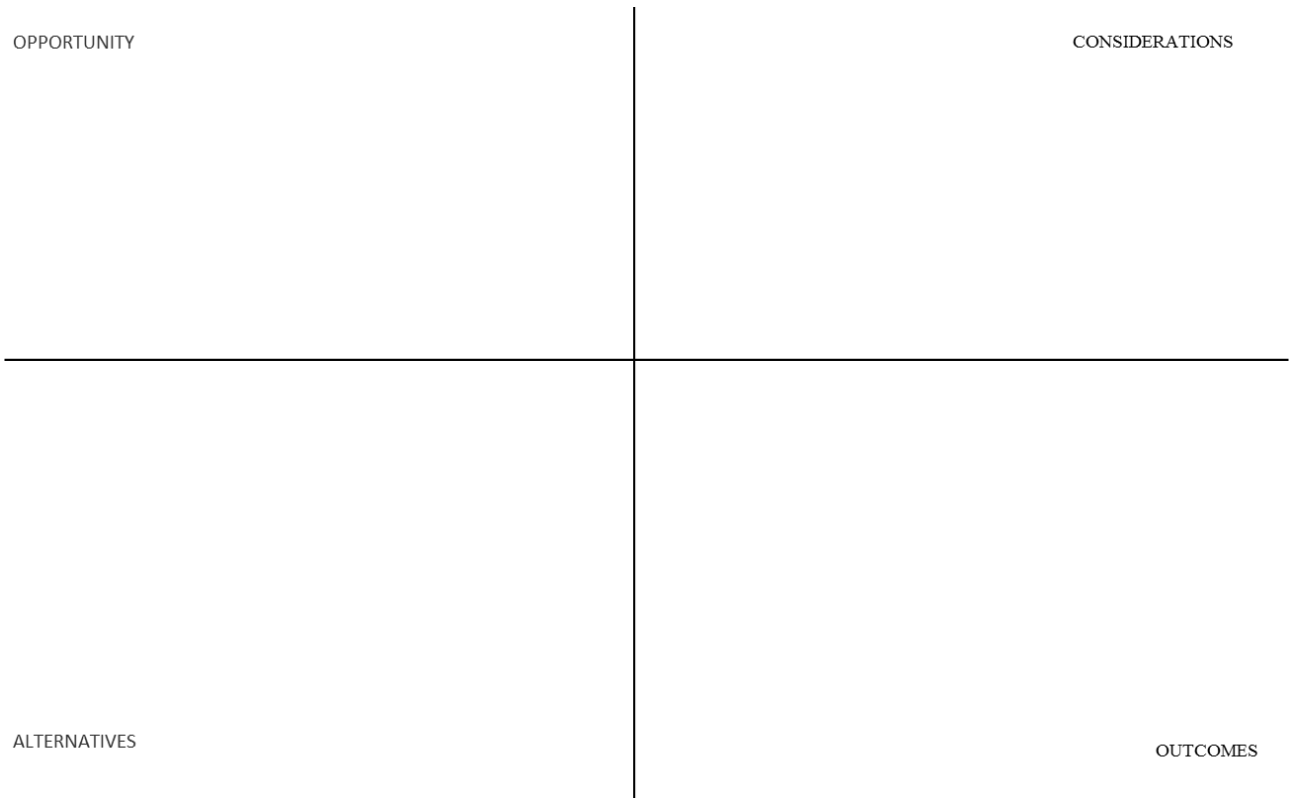
This aim, to find a way of getting natural scientists and human scientists to work together on seeing what is not being taken into account in and about the practice of developing new and emergent sciences and technologies, could be described as “second-order observation” in systems theoretic language, or how someone observes what they observe.²³⁷ Second-order observation, theoretically, can be put to work towards the goal of expanding a system’s capacity for self-observation, by understanding how the system observes what the system observes.

The ‘*midstream*’ is a heuristic contrastive with extant modalities of human science engagement with the natural sciences, however, it is itself problematic qua metaphor. For instance, when I would talk with a lab director in SynBERC, in his office, about his lab’s work on the development of technologies for the synthesis of ever longer and more complex DNA sequences (at falling cost and with increasing access) and when I asked him to consider this development relative to the political

²³⁷ The term is of course Luhmann’s and the practice originated in his attempts to understand and intervene on the relation of information and reform in post-war German administration.

environments in which this technology exists; is this asking an ‘upstream’ question of a ‘midstream’ actor? Such an interpretation of this episode was given during work in one of the STIR workshops. The danger of the stream metaphor, and of such an interpretation, is compartmentalization. There is the danger that one will contribute to reifying the autonomy of, and insulating the scientific system from, the environments in which it exists.

As I had understood it, projects addressing the limits of extant approaches to the social, political and ethical dynamics of science, such as STIR, were aimed at asking scientists, that is people whose vocation and work is inquiry, not to insulate themselves from the relation between their work and their various ethical, political and social milieus in which it exists. The challenge was to identify the right questions in these relations and to pose them as problems for collaborative work.



“STIR PROTOCOL”

FIGURE 1.

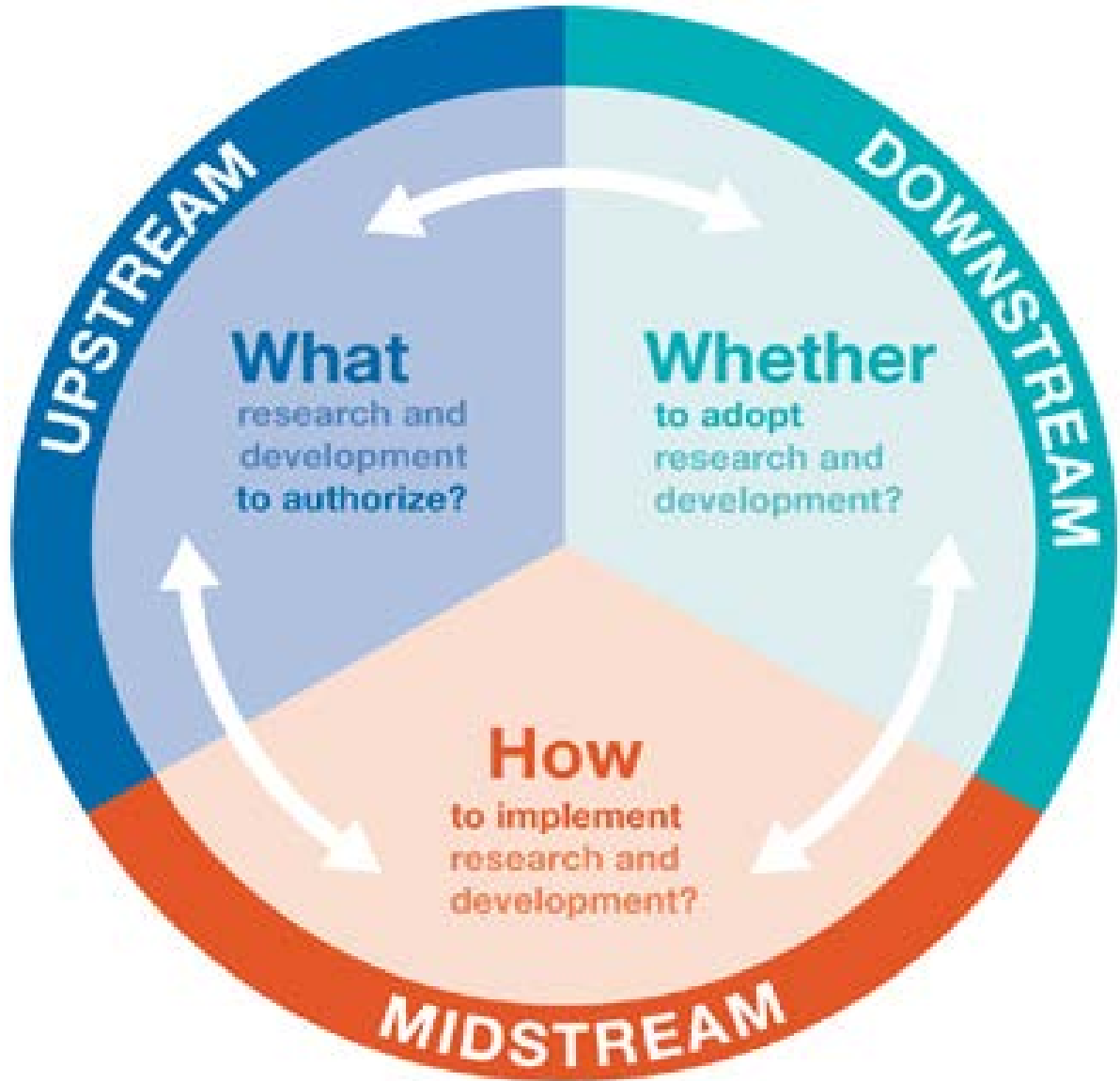


FIGURE 2.

Daan Schuurbiers & Erik Fisher, *EMBO reports* **10**, 5, 424–427 (2009)

Chapter Four

Function & Significance: Synthetic Biology as an Ethical Domain

“This nature has nothing more in common with the ancient concept of nature to which the mimesis idea referred: the unmakeable model of all that is made. That all phenomena can be manufactured is instead the universal presupposition of experimental investigations of nature, and hypotheses are outlines of instructions for the manufacture of phenomena. Nature then becomes the embodiment of the possible results of technology ... Only through the reduction of nature to its raw potential as matter and energy is a sphere of pure construction and synthesis possible. This results in a state of affairs that seems paradoxical at first glance:

An era of the highest regard for science is at the same time an age of the decreasing significance of the object of scientific study.”

–Hans Blumenberg.²³⁸

The epigram from Blumenberg brings together two key points on which I wish to reflect; if experimental investigations into biology presuppose the capacity to manufacture biology, then our knowledge of biology is dependent on knowing what we wish to do with biology. This capacity to know biology, which presupposes being able to make biology, requires a “reduction” of biology to functions of matter and energy. The significance of the objects of biology is only “decreased” with respect to biology having a status independent of its construction and synthesis, a ‘nature’; hence the decreased significance of the object which accompanies the process of its creation is only paradoxical at first glance. At second glance, the paradox becomes a question; if one recognizes that biology, our knowledge of the molecular evolution of biological phenomena and the production of molecular systems, is made through experimentation, then what is the significance of our experimental production of biological functions?

It is with respect to this orientation provided by Blumenberg that I ask: how are the bioscientists and engineers, with whom I worked, subjects of an ethical

²³⁸ Hans Blumenberg, “‘Imitation of Nature’: Toward a Prehistory of the Idea of the Creative Being,” *Qui Parle* Vol. 12, No. 1 (Spring/Summer 2000): 17-54.

domain? To re-pose the question in still other terms, what is the relation between the techniques used to make biological functions, along with the plurality of ends that such functions can be used for and the ‘ethical life’ of those doing this making?

This chapter, and the one that follows, revisits the questions introduced in a general way in Chapter One and they attest to the problem of working collaboratively on this question. My argument in this chapter is, following Blumenberg, that inquiry into ‘how biology works’ is also a normative question of the human being’s capacities of production; that inquiry into biological function requires us to ask a question of the significance of such function for an understanding of the life in which such functions exist. I will attempt to show the need for the question, rather than beg the question.

With specific reference to bioengineering, knowledge of living processes, and their production, re-design and re-production, always involves questions of their use and the living environments in which they are used. The question of their use then has to be considered in the light of the specific problem which needs to be solved and the environments in which the solutions and problems exist. There are biological and ethical sides to these problems and environments; human problems (“real world problems”), proponents of synthetic biology claim, can be solved through biological technologies and these biological technologies, as we will see, require attention to biological environments.

The inclusion of a “Human Practices” thrust within SynBERC was predicated on the assumption that the problems of (human) living, which can be transformed into biological problems, in addition to requiring attention to biological environments, require attention to human environments, the milieu of work and world. This point holds for STIR as well; given the focus in laboratories on the means of experimentation, means which are considered within a fairly strict set of parameters dependent on the skills, know-how and orientation of the lab, the STIR project sought to add an element into this setting to bring to the surface latent unarticulated thoughts about the process of experimentation.

In this chapter, I am making an argument about why one should pose the question of the ethical significance of making in biology and I show it through some experiences in my first venue, with biologists from SynBERC. I take up the question and contrast of *how* the ethical significance of making in biology can be posed by way of STIR in Chapters Six and Seven.

The first claim, that engineering biological systems requires attention to “properly biological environments,” I take from the biologists own discourse and practice. This claim arose in relation to what has been called “the first wave” of

synthetic biology.²³⁹ The first wave was the effort to introduce a purely engineering principle into the construction of biological objects. This principle attempted to factor out the complexities of the biological environments in which the objects exist. Bioscientists in this domain of practice understood the elegance of such a purely engineering approach to biology, but quickly recognized the biological limitations of such an approach, since it was not focused enough on well-characterized biological problems and did not take into account the critical limitations of factoring out appropriate biological environments.

Description and analysis of this first claim then opens up the reciprocal question and then argument; how is it possible to frame our nominal bioscientific collaborators' scientific practice, their relation to their own work, and the products themselves, as an ethical practice and its objects as admissible of ethical judgment? The inquiry turns on the manner in which this set of practices of bioengineering, reveals something ethical; what is made with biological substance, why it is made and how.

Briefly put, synthetic biology is dependent on techniques for producing biological objects on which experiments can be conducted. What can be known about these objects is (logically and experimentally) dependent on which biological objects can be made and sustained in experimental conditions. With respect to Blumenberg's epigram, we are a long way past the activity of 'merely' recording nature, if that were ever the case.²⁴⁰ Interventions into nature, even when at first they were a case of isolating something from nature, something which had not been isolated before, implied the question of what we could or should do with it.²⁴¹ To give an exemplary instance, this manifested itself clearly in the case of Jonathan Beckwith's initial isolation of the *lac operon* from the bacteria *e. coli*, in 1969; the *lac operon* is a series of three genes which express enzymes which allow the bacteria to breakdown the sugar lactose. Beckwith was alarmed by what he had been capable of doing, prefiguring the debates to come in the 1970s over the politics of recombinant DNA.²⁴² The point I wish to make at this juncture is simply that techniques for knowing, in molecular biology, always include a capacity for doing. This poses the question of the ends and ramifications of that which we produce and the aims of such experiments.²⁴³

²³⁹ PE Purnick and R Weiss, "The Second Wave of Synthetic Biology: From Modules to Systems," *Nat Rev Mol Cell Biol* 10, 6 (June 2009): 410-22.

²⁴⁰ Lorraine Daston and Peter Galison, *Objectivity* (New York: Zone, 2007).

²⁴¹ Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge: Cambridge University Press, 1983).

²⁴² Susan S Wright, *Molecular Politics: Developing American and British Regulatory Policy for Genetic Engineering 1972-1982* (Chicago: University of Chicago Press, 1994.)

²⁴³ Beckwith cited in Carl Zimmer; *Microcosm: E. coli and the New Science of Life* (Pantheon Press, 2008), 161.

Mimesis and Invention.

Blumenberg's essay "Imitation of Nature': Toward a Prehistory of the Idea of the Creative Being," charts the transformation in the concept of nature with respect to two manners of production, imitation and invention. As a conceptual history, it traces the emergence of invention as a historically specific moment in modernity, and the effect that invention has had on the normativity of the concept of nature. His broad question relative to which he tracks this change is what the human being, using her power and skill, can do in the world and with the world?

Aristotle's answer was that the human being can imitate nature by means of *technē*. Aristotle distinguishes between *technē*, understood as productive knowledge, 'know-how' oriented towards making, from theoretical and practical knowledge aimed at 'knowing that' (knowing for its own sake) and 'doing well' respectively. As I have tried to indicate initially, and as I will develop, I think it is characteristic of human beings' technical capacities to know and make, with respect to biology today, that such knowing and making are intimately tied one to the other as well as to the question of 'doing well.'

For Aristotle, through *technē*, the human thing either completes nature (according to nature's own end), or imitates nature.²⁴⁴ "Nature" here means both a self-causing activity and a produced form.²⁴⁵ Blumenberg's investigation, in keeping with his metaphorology, takes up the trope "art imitates nature." This trope is of interest because within it one can see the self-recognition of creative being, as a creative being, one who opposes nature to that which he or she makes.

Art, or the product of craft, is not nature. For Aristotle, however, that which can be made is either made according to a natural form or in accord with a natural principle. Is there not a gulf, Blumenberg asks, between the Aristotelian formulation and the rise of an ethos oriented to the deliberate transformation (and outperformance) of the given?

"The vehement passion with which the attribute of creativity was gained for the subject was marshaled *in the face of* the overwhelming importance of the axiom of the 'imitation of nature.'" ²⁴⁶

For Blumenberg, the artisan is a key transformative figure in the historical emergence of modernity characterized by what he elsewhere called "self-

²⁴⁴ Aristotle, *Physics*, II, 8.

²⁴⁵ C.f. Proposition 29 of Spinoza's *Ethics*; "*Natura naturans we must understand what is in itself and is conceived through itself, or such attributes of substance as express an eternal and infinite essence, that is ... God, insofar as he is considered as a free cause. But by Natura naturata I understand whatever follows from the necessity of God's nature, or from God's attributes, that is, all the modes of God's attributes insofar as they are considered as things which are in God, and can neither be nor be conceived without God.*"

²⁴⁶ Blumenberg, "Imitation of Nature," 19.

assertion.”²⁴⁷ Citing the second chapter of Cusa’s well-known *De idiota*, he highlights the importance of the activity of the artisan in breaking the normative hold of a “naturalizing nature” and its “naturalized” productions. Citing the example of Cusa’s spoon maker, the point he makes is that “spoons, dishes and jars are perfected by human artistry alone” and not the imitation of created visible forms.²⁴⁸ The importance of this point for our biologists will become evident. At this point I would like to introduce a second orienting trope, which concretizes Blumenberg’s gulf, with respect to the practices under observation. It became a widely circulating trope in synthetic biology to use a quotation from physicist Richard Feynman, as a way of justifying this particular approach to engineering biology;

“what I cannot create, I do not understand.”²⁴⁹

Synthetic Biology.

Synthetic biology, as I came to understand its goals in 2006, aimed at introducing a properly engineering principle toward the construction of novel biological systems that exhibit specified behaviors. The ‘artisanal’ aspect of biology is precisely what was considered problematic such that a range of (engineering) concepts and tools needed to be worked with so as to transform the practice of inquiry and production in biology. It was “in the face of” the complexity of biological systems as they are, which required laborious and inefficient artisanal skill for their representation and intervention, that a better set of techniques, as well as concepts, for intervention into biological substance were required.

²⁴⁷ “The Middle Ages came to an end when within their spiritual system creation as ‘providence’ ceased to be credible to man and the burden of self-assertion was therefore laid upon him.” Hans Blumenberg, *The Legitimacy of the Modern Age* (Cambridge, MIT Press, 1985), 138.

²⁴⁸ “A spoon has no other exemplar except our mind’s idea [of the spoon]. For although a sculptor or a painter borrows exemplars from the things that he is attempting to depict, nevertheless I (who bring forth spoons from wood and bring forth dishes and jars from clay) do not [do so]. For in my [work] I do not imitate the visible form of any natural object, for such forms of spoons, dishes, and jars are perfected by human artistry alone. So my artistry involves the perfecting, rather than the imitating, of created visible forms, and in this respect it is more similar to the Infinite Art.” – Cusa, *De Idiota*.

²⁴⁹ On the use of the quotation by synthetic biologists, I disagree vehemently with Evelyn Fox Keller’s assessment that “its appropriation by synthetic biology is at the very least ironic.” The irony, in her view is that Feynman is as an icon, along with Einstein, “of the ultimately pure scientist, of the inquirer who seeks to build the world not by construction but by deduction, reasoning always from first principles.” I would argue that even models of physical reality are not built merely by recording and then deducing from ‘nature.’ To posit such a claim, as has recently been re-affirmed by my colleague Arpita Roy, I would suggest, is to minimize and misunderstand the role of technē in the possibility and purpose of knowing. See, Evelyn Fox-Keller, “What Does Synthetic Biology Have to Do with Biology?” *BioSocieties* 4 (2009): 291–302. Arpita Roy, “Dualism and Non-Dualism: Elementary Forms of Physics at CERN,” PhD Thesis, (University of California, Berkeley, 2012).

Synthetic biology refers to the design and construction of biological functions and mechanisms. I use the term mechanism as a stand in term for a range of other words that we will come across; ‘parts,’ ‘composite parts,’ ‘devices,’ ‘networks,’ ‘modules.’ The design and production of such biological functions are either new functions, or functions which already exist and whose biological basis has been redesigned. There are, broadly speaking three main characteristics of work in synthetic biology: First, the use of engineering techniques for designing and building organisms; second, the ultimate aim to make new functions so as to be able to “solve real world problems”; third, an ethos which brings together the activity to the end. To give an example of the first characteristic, the Voigt lab, based at UCSF in 2007, re-designed *e.coli* by introducing genes which produce a dark pigment and genes which could respond to light. The light responding genes when activated would cause the pigment producing genes to shut off. Whilst the ‘novelty’ is that through this technique they were able to produce light induced bacterial photographs, (Figure 3), the technological interest is the spatial and temporal control of gene expression; i.e. to get bacteria to do something that you want them to do, where you want them to do it, when you want them to do it.

One of the reasons synthetic biology seemed so interesting in 2006 was that as a possibly transformative set of techniques, and mode of production, we in the Human Practices thrust hoped that human scientists could participate in the formation of the ethical domain in which this practice existed, which is to say, to work with the synthetic biologists on connecting the question of what they are doing to the kind of life being made possible, both scientific, ethical and political (not only for those doing the practice but those affected by the practice).

This is important to underscore, and marks the point of orientation for the chapter. Synthetic biology, we thought, was an exciting re-stylization of older and newer elements of molecular biology, remediating its artisanal mode with respect to emerging bioinformatics and engineering technologies. This remediation, interestingly for the anthropologists, was being conceptualized and articulated, prior to the formation of anything that could (nor perhaps should) be called a paradigm. The concepts and the activity were only partially determined. This made it scientifically and ethically interesting; I specify ethically, because it seemed as though anthropologists could take part in thinking through, with the biologists, the goods of this activity, understood as both the goods internal to the practice and the products of such a practice.

The older biotechnical elements included relying on well-established methods in molecular biology, such as molecular cloning, polymerase chain reaction (pcr) and in vivo recombination. Although the newer element was the attempt to introduce engineering principles (which I will describe shortly), the object through

which such principles were to be worked, in its initial formulation, was well-known to the field of molecular biology, the genetic circuit.

Biological Machines.

Francois Jacob and Jacques Monod, in 1958, in order to explain how *e. coli* regulates its production of the enzyme which breaks down lactose, figured out that *e. coli* uses several genes controlled by a protein, which they called a “repressor,” to regulate its production. The analogy they used in order to concretize how this process works was that the “repressor” works ‘like a switch in a circuit.’ The image was logical; three genes controlled by a switch could easily be represented as a circuit diagram in electronics. Highlighting one of the problems with analogies, in a recent account of the history of molecular biology written with respect to the history of *e. coli*, Carl Zimmer wrote, regarding Jacob and Monod’s discovery, “*e. coli*’s circuitry *mimics* circuitry you might find in digital cameras or satellite radios.”²⁵⁰ His claim is clearly not quite true; *e. coli*’s lac operon and repressor system could be understood as though it were circuitry in a human invention, like a radio. This is an analogy and not a metaphor. In isolating and describing *e. coli*’s “circuitry” Monod and Jacob used a proportional model to understand it. The production of biological ‘circuitry’ was made to *mimic* electronic circuitry only once the tools for isolating and recombining these biological “components” were available. The analogy is extended to the *mimesis* of electronics only once an electronic principle was introduced into the making of biological functions.

It was precisely this aim to take a biological phenomenon understood on the model of electronics, and re-design it along ‘simplified’ lines, which was behind the synthetic biology idea circa 2000. A paper published in 2000 by Elowitz and Leibler showed that they had been able to construct a synthetic gene expression networks. Such networks are of interest because, in their terms, ‘design principles’ underlying intracellular networks are poorly understood. The authors introduced a very basic principle; for a complex phenomenon of interest, which is poorly understood, build a simplified version of it to act as a model on which to understand the “network behavior” of gene expression.²⁵¹ The interest in the problem of the design principles underlying the “functions” produced by “networks of interacting biomolecules” is that with more knowledge about such principles, one may be able to engineer new cellular functions, or understand networks which already exist; “natural” ones, in the authors’ terms.

²⁵⁰ Carl Zimmer; *Microcosm: E. coli and the New Science of Life* (Pantheon Press, 2008), 36. Italics mine.

²⁵¹ Michael B. Elowitz & Stanislas Leibler, “A Synthetic Oscillatory Network of Transcriptional Regulators,” *Nature* 403, 20 (January, 2000): 335-338.

A general obstacle, the authors suggest, is that biologists, engineers and others, do not know enough about the parameters that characterize “interactions between different components” in the network. In order to inquire into such parameters they designed (“conceived in advance,” especially with through modeling tools) and constructed (in a wet lab), an oscillating network of transcriptional repressors (“repressors” are proteins that bind to specific sites on DNA and prevent transcription of nearby genes), i.e. a circuit of genes which could turn one another on and off repetitively with one of the genes reporting a fluorescent protein such that the network rhythmically ‘flashes.’

Technology and Equipment.

Whilst I began the chapter by indicating that I will include both the study of, and making of, biological objects under a broader relation of science and ethics, the particularity of synthetic biology is the attempt to introduce an engineering ethos into the study of and making of biological objects. This specificity needs to be isolated, so as to understand the object on which we in Human Practices hoped to intervene.

A manifesto aimed at delineating principles and practices to transform biotechnology from a “specialized craft into a mature industry,” was published in June 2006 in the journal *Scientific American*. The manifesto was published by the ‘Bio Fab Group,’ a group consisting of protein, chemical and electrical engineers, as well as a physicist and molecular biologist.²⁵² It began with a historical analogy sketched in heroic tones of a lone entrepreneur way out west;

“Electronic engineering ... was transformed beginning in 1957, when Jean Hoerni of Fairchild Semiconductor, a small company in what would later be known as Silicon Valley, invented planar technology... This new approach allowed engineers to produce integrated circuits cleanly and consistently and to create a wide variety of circuit types.... Soon engineers could draw from libraries of simple circuits made by others and combine them in increasingly complex designs with a widening range of applications.”²⁵³

The analogy with integrated circuits was of interest to the “BioFab group” relative to their aim, to transform how biological systems are engineered.²⁵⁴ The

²⁵² David Baker, George Church, Jim Collins, Drew Endy, Joseph Jacobson, Jay Keasling, Paul Modrich, Christina Smolke and Ron Weiss.

²⁵³ Baker et al, “Engineering Life: Building a FAB for Biology,” *Scientific American* (June 2006): 44–52, 46.

²⁵⁴ The use of the analogy I have been able to find is “Circuit Simulation of Genetic Networks,” *Science* Vol. 269, No. 5224 (1995): 650-656. HH McAdams and L Shapiro “Genetic networks with tens to hundreds of genes are difficult to analyze with currently available techniques. Because of the many parallels in the function of these biochemically based genetic circuits and electrical circuits, a hybrid modeling approach is proposed that integrates conventional biochemical kinetic modeling within the framework of a circuit simulation. The circuit diagram of the

reasoning that followed was that since electrical engineering, as well as examples in mechanical engineering such as the screw, developed on the basis of producing interoperable parts regulated by standards, as well as new techniques and technologies for producing such parts, it followed that those seeking to build ever more complex biological systems will need similarly standardized methods and parts. A discontinuity within biotechnology, a change “from craft to mature industry,” was predicated on a historical constant, “like other successful engineering fields” genetic and biological engineers must do as they have done. As Rabinow and Bennett have observed,

“Synthetic biology aims at nothing less than the (eventual) regulation of living organisms in a precise and standardized fashion according to instrumental norms. There is a feeling of palpable excitement that biological engineering has the capacity to make better living things, although what that would mean beyond efficiency and specification opens up new horizons of inquiry and deliberation.”²⁵⁵

What the authors identified was the fact that the effort to introduce a change in biological *technologies* was sustained by the mobilization of particular ethical orientation; the regularization and standardization of how to make living things is normatively oriented to instrumental outcomes and the practice is underpinned by a constitutive affect with respect to the development of these capacities.

The intention in the human science projects in which I participated was to work with synthetic biologists on the relation of synthetic biology and ethical questions; i.e. how ethical problems relevant to synthetic biology can be formed such that something significant could be said about them? How could or should one be oriented ethically to such problems? What are the relations between inquirers that would contribute to having the appropriate capacities to listen and speak so as to affect and be affected appropriately relative to these questions?

It is important for me to indicate that there was, operative within this domain of practice, relative to their biological projects, extant ethical parameters, in addition to technologies, that would have to be engaged with and that would parameterize the capacity to collaborate. This point is taken up further in the comparison between STIR and Human Practices.

bacteriophage lambda lysis/lysogeny decision circuit represents connectivity in signal paths of the biochemical components. A key feature of the lambda genetic circuit is that operons function as active integrated logic components and introduce signal time delays essential for the in vivo behavior of phage lambda.” McAdams then wrote a paper with Arkin, one of the SynBERC PIS and new head of the SBI in Berkeley in 1998. “Stochastic Kinetic Analysis of Developmental Pathway Bifurcation in Phage λ -Infected *Escherichia coli* Cells,” *Genetics* Vol. 149 (August 1998): 1633-1648.

²⁵⁵ Rabinow and Bennett, *Designing Human Practices*, 2012, 36.

Truth Claim.

The kind of speech act that I would frequently hear, indicative of the kind of truth claim and affect, as well as ethical orientation, of this domain of practice, were ones such as, “you don’t make 1000 computers and then pick the best one.” The analogical reasoning is that since one does not go about producing complex electronic devices in this way, biology as an engineering practice should not be done this way. The BioFab group quotes Tom Knight of the Massachusetts Institute of Technology artificial intelligence laboratory on his diagnosis of the difficulty in finding ways to assemble sequences of DNA,

“The lack of standardization in assembly techniques for DNA sequences forces each DNA assembly reaction to be both an experimental tool for addressing the current research topic, and an experiment in and of itself.”²⁵⁶

Previous to his work in synthetic biology Tom Knight worked on computer design at MIT in the seventies and eighties. On his own account he was a computer builder and electrical engineering master within the field of integrated circuit design. In the late eighties two developments happened that moved him to start considering biology and biochemistry. The first was the realization that the future of computer chip design would soon reach its physical-material limits. With respect to standard chip design, Knight was envisioning the end of Moore's law, a plateau with respect to possible speed and efficiency of future chip design. The second was what he saw as a dismal outlook for the prospect of developing Artificial Intelligence methods for solving complicated biological problems, specifically the design of computers and algorithms for solving and modeling questions around protein folding. In Knight's view, solving these sorts of problems with the status quo of computer design was not feasible. This inspired him to look for new ways to do what he knew how to do, that is, manufacture circuits. Specifically he was intrigued by the question of how biological substance would be used to manufacture such circuits and whether biological substance could overcome the physical-material limits he encountered in silicon engineering. The hope was that by learning to engineer and control biological functions, they could use that functionality to create integrated circuits on scales that would surpass the capability of current circuit design techniques.

Towards this end Knight became a self-designated graduate student in molecular biology in 1991 at MIT, and over four years completed the graduate course in molecular biology. In 1996 he proposed a DARPA study, which was accepted and involved a small number engineers and biologists also working on the problem of mobilizing biology towards computing ends. This study led to a

²⁵⁶ Tom Knight, “Idempotent Vector Design for Standard Assembly of Biobricks.”

proposal in 1996 for a DARPA funded program which directly led to him setting up a wet lab in the MIT computer science department in 1997.

At that time the focus of his lab was on the study of controlling metabolic pathways for constructive purposes. He described this experience as both frustrating and rewarding: frustrating because they did not know what they were doing and were not making rapid progress towards the original proposed goal, and rewarding because they were slowly learning something new. One of the frustrations that Knight encountered in the change in culture from electrical engineering to the biology lab was the different ways in which techniques were taught. In the biology lab technique was taught as a craft; passed from one person to another. The ability to get experiments to work relies as much on “good hands” and “magic” (sic) as it does following well scripted methods. For Knight this was exemplified by the fact that every experiment automatically became two experiments, one doing the experiment and the other just to make the DNA for the experiment as there were no standards for DNA synthesis. Practices were ad hoc and varied from lab to lab, person to person and even instance to instance. Another consequence was that one “never got good at it” and DNA synthesis never became routine. As a self-described engineer, this craft-like aspect of biology “drove [him] bonkers.”

The aim for those seeking to emulate other engineering disciplines is to uncouple design and production, which as Knight suggests, is currently entangled within the experiment. As he continued, in the 2003 article from which the BioFab Group quote,

“One of our goals is to replace ad hoc experimental design with a set of standard and reliable engineering mechanisms to remove much of the tedium and surprise during assembly of genetic components into larger systems... The key notion in the design of our strategy is that the transformations performed on component parts during the assembly reactions are *idempotent* in a structural sense. That is, each reaction leaves the key structural elements of the component the same. The output of any such transformation, therefore, is a component which can be used as the input to any subsequent manipulation. It need never be constructed again, it can be added to the permanent library of previously assembled components, and used as a compound structure in more complex assemblies.”²⁵⁷

The solution to this vexing problem, how to assemble sequences of DNA so as to produce intended functions, was found in what Knight called the “BioBrick” part. A BioBrick part is a sequence of DNA which has been ‘cut’ using an

²⁵⁷ *Ibid.*

assembly method that makes sure that whatever the core of the sequence codes for, a standard prefix and suffix will always be at either end. This ensures that the end of one part, like LEGO, a favorite analogy of Knight, can be affixed to the beginning of another part.

A BioBrick part is thus a sequence of DNA flanked by specific “restriction sites,” a series of base pairs, which are recognized by restriction enzymes (these cut double-stranded or single stranded DNA). The ‘actual part’—sequence of DNA which codes for a function – does not include the BioBrick sequence of restriction sites. The flanking sequences allow someone to cut and ‘ligate’ parts (rejoin two ends of DNA molecules using a DNA ligase). The standardization of the sequences and their non-inclusion in the sequence that codes for the function one is interested in is crucial. This is what allows the same cutting and pasting tools to be re-used in order to join together parts with their associated function in order to build biological mechanisms.

This frustration directly led to his development of the BioBricks standard. A set of rules for packaging segments of functional DNA in order to standardize DNA composition and synthesis. The hope was to invent a system in which combining different parts of previously constructed DNA became routine and standard. Results could be recreated with ease and the act of running experiments could be black-boxed and the role of designing experiments could be emphasized.

This idea for set of “standard biological parts” was presented at a DARPA meeting in 2000 to little enthusiasm. For the other computer scientists working on biological problems, at this point mostly creating computer models of biological systems, there was little use for the “standard biological part.” The one notable exception to this general lack of interest was Drew Endy, a civil engineer-turned-biological engineer, who at that time was working using computers to model the inner workings of the T7 bacteriophage. Endy was to become an ambassador, spokesperson, and manifesto provider for this approach to standard biological parts based engineering.

A single sequence (a ‘part’ encoded on a plasmid) is joined to flanked parts, made into composite parts in order to give intended functions to a system, either a series of ‘composite parts’ made into devices, such as genetic circuits, or the incorporation of parts and devices into cells, so as to confer a different function to the cell. This is the attempt to introduce an ‘abstraction hierarchy,’ an engineering principle, into the organization of designed biological systems, moving “up” in terms of size and complexity from basic parts, to composite parts, to devices and finishing by putting them into a “chassis,” for example, an *e. coli*, which itself has been modified relative to the purpose it is supposed to fulfill. This was the division

through which SynBERC as an institution was organized: Parts, Devices, Chassis, in addition to Human Practices.

It is difficult, as many authors have noted, to disaggregate a part as a thing (i.e. a sequence of DNA), from a part as a function (i.e. the function that a regulatory sequence codes for), and from a part as a relationship between components in a cell. It is clearly all three as the BioBrick Foundation, a non-profit organization established to promote this vision of bio-engineering suggests:

“each distinct BioBrick™ standard biological part is a nucleic acid-encoded molecular biological function (e.g., turn on/off gene expression), along with the *associated information* defining and describing the part.”²⁵⁸

One of the assumptions of this solution to the problem of assembling sequences of DNAs which perform new biological functions and housing them in cells, is that biological systems are the kind of things that are made up of such “parts,” which can be treated in such an inter-operable, and linear fashion; i.e. that the output of one reaction could be used as the input for another. This assumption thus poses the ‘ontological’ question of how they exist, as well as methodological questions of how one could standardize their production and the extent to which their production and use could be generalizable.

Recruitment and Subjectivation Through iGEM: ‘Open and Fun!’

“*You are now Synthetic Biologists.*” And it was so. Randy Rettberg, former executive at Sun Microsystems and the president of the international Genetically Engineered Machines (iGEM) competition had the authority to make this kind of claim. With outstretched arms, standing on-stage under dimmed lighting, he made the pronouncement to the 800 or so young molecular biologists and engineers who comprised the fifty four teams that had entered the competition in 2007, as well as a range of older scientists and observers who comprised the judging panels. Packed into an amphitheater in the Bill Gates funded “Stata Center” at MIT, Rettberg’s youthful, excited stammer rallied the assembled scientists and engineers, with a trope of invention; “When we started this, they said it couldn’t be done; three years later with fifty four teams, we’ve shown them that we can!” The “it” in question is this approach to making biological circuits, and operating them in living cells. Whilst, as we will see, the question of what is made through synthetic biology varies, its practices, ends and achievements depend on different conceptualizations of biological problems, specific techniques and technologies, the question of *who* a synthetic biologist is, was at this time, to a large degree, controlled by passing

²⁵⁸ Quotation from BioBricksFoundation website.

through the pedagogical experience of iGEM. This experience and the self-designation of a subject position, whilst not determinative of a “field” was constitutive of an ethos toward a practice of science and engineering.

In 2003, the first Genetically Engineered Machines competition took place at the Massachusetts Institute of Technology. It was an in-house affair and included four teams under the eye of civil engineering-turned-genetic engineer, Drew Endy and the renowned computer scientist-turned-genetic engineer Tom Knight. Endy, on arriving at MIT as a young faculty member only a few years after the opening of their bioengineering department in 1998, began working with Knight and his BioBrick assembly method, in order to try and make more complex and interesting circuits than the one they studied in the aforementioned paper by Elowitz and Leibler. Together with Rettburg, they sought to develop a student competition modeled on robotics competitions, as a vector of recruitment as well as a testing ground for the mode of work. By 2004, the competition included teams outside of the Institute and since then has grown exponentially and in 2010 had 130 participating teams. In the first years of the competition, the majority of projects were in the vein of things like circuits which functioned as biological counters, or which demonstrated the ability to ‘switch’ states, “toy systems” as they are called.

The challenge at iGEM, broadly conceived, is for students, over the course of a summer, to design and build biological circuits out of ‘standard biological parts’ using the BioBrick assembly method. ‘BioBricks’ are documented and collected in a searchable open source registry centered at MIT, the *Registry of Standard Biological Parts*. The BioBrick method for construction and linking of biological parts is foundational to the iGEM competition. Students must use BioBricks to assemble their systems and then log new parts and discovered characteristics of these parts on the open source registry. The characterization of parts is dependent on shared measurement standards which have begun to be developed but are still far from stable six years on.

iGEM is organized around the concept of creation and assembly of biological parts into biological devices and as such teams work on problems which can be broken down in terms of such parts. This is quite important to note, the “parts” concept determines the problems that can be worked on. One reason for this is that transcriptional and translational regulation elements, such as “ribosome binding sites” (a sequence which regulates the translation of protein) and “promoters” (which regulates transcription) are isolatable elements from which to start putting together functional relationships, such as genetic circuits with simple outputs. Pedagogically it shows proof of principle to young biological engineers that you can apply an engineering ethos to the design and physical composition of biological circuits.

Several things should be noted about iGEM as a venue which facilitates pedagogy in biological engineering: iGEM is a major driver for the creation of a community of researchers that self-identify as synthetic biologists. The young scientists who work for a summer designing and trying to build biological systems from these parts, leave MIT after the November competition with a reinforced conception that bioengineering can be done with attention to a form of ‘black boxing.’ This black boxing involves work predicated on a linear, modular, and fully abstractable model of biology. As such, nonlinear dynamics of biological phenomena are designed out of the biological problem. This encourages the recruitment of ‘non-biologists’ to bioengineering on the supposition that one need not deal with the ‘complexity’ of biology. Many teams do not get far beyond the design stage in three months and so do not have to tackle the limitations of this “black boxing” approach. Many senior biologists who are interested in synthetic biology are aware that pedagogically such ‘black boxing’ both facilitates a certain work (especially of recruitment) and has limitations.

Furthermore, the ‘open source’ format of the Registry was a foundational principle for work in synthetic biology more broadly, including within SynBERC. There is a commitment, promoted through iGEM, the Registry and the non-profit organization which supports them, the BioBricks Foundation, to provide ‘parts’ in an open source manner. Continuing the analogy between developments in silicon and biology, some proponents of synthetic biology wished to model the developments in their activity on the open source movement in software engineering. It was unclear (and is still unclear) what the operational relations could be between such a principle, and a commitment to solving “real world problems,” with which the BIOFAB group’s manifesto explained the rationale for pursuing such an engineering approach to biology. We will return to this problem.

Jamboree 2007.

In November 2007 I was part of a contingent of Berkeley students visiting MIT for the annual iGEM “jamboree,” four days of presentation in which panels of judges review and decide which teams’ work over the summer months merits a prize. I was at iGEM as both observer and quasi-participant; quasi-participant because, as I will explain, I was responsible for mentoring a young undergraduate anthropologist, Kristin Fuller, who had joined the Berkeley iGEM team in June, as part of our efforts at collaboration within SynBERC.

Our experiment consisted in the dual tasks of my thinking through with her the role and work of Human Practices within the context of an iGEM project and her ‘fieldwork challenge’ of developing a mode of co-labor with her colleagues on the team. Before describing this experience, it is worth giving a sense of iGEM

such that its complexities can be ascertained. If I were to use a catchphrase to encapsulate how participants in iGEM characterize the affect of the competition, I would use the term “serious play.” The four days at MIT are referred to as a “jamboree,” a nineteenth century slang term in the US to refer to a “noisy revel.” The students are young, the t-shirts are in primary colors and there are simultaneously four or five presentations being conducted at any one time; it’s dynamic and indeed, “playful.” As we will see however in our experience of Human Practices at iGEM, we will have reason to qualify this self-conception of the activity.

A recent account of work in synthetic biology, written by journalist Marcus Wohlsen, focused on the relation between synthetic biology and Do-It-Yourself Biology. The main figures in DIY-Bio all passed through MIT in one fashion or other, either directly as students of Knight and Endy, or by way of iGEM from other colleges. In his account, Wohlsen typifies, and exalts, the self-stylization of the community:

“The DIYbio crew seeks to make science more playful, and bringing down the price is part of their effort to make that happen. Another part is place. They believe science can happen in a garage. Or in a bar. Important work gets done in buttoned-up fashion in buttoned-up labs. But why not imagine science another way, they say, an approach that retains the personal spirit of the scientists involved? Why not see what happens when science is done in style? This approach is not merely an attempt to project a little hacker chic, although fashion does play a part in the appeal. Playfulness, fashion and a direct appeal to fun all play a part in an attempt to use style to draw people toward the substance of science.”²⁵⁹

At the jamboree, I met James Brown, a graduate student, who first went to iGEM in 2005 with the University of Cambridge team, one of thirteen, and one of two European teams participating for the first time; the ETH-Zurich being the other one. Cambridge had been introduced to synthetic biology through an iGEM Ambassador Envoy excursion made by Drew Endy and Randy Rettberg. The idea of a parts based approach to engineering biology, Brown said, was resisted by the Cambridge “old guard,” but embraced by “forward thinking types” such as his advisor, Jim Haseloff. Their 2005 team was made up of 3 biologists and 3 engineers, and they “did the whole media pack thing by bringing brochures and t-shirts.” Such stylization is now highly characteristic of iGEM. Teams design a logo, pick a color scheme, and “pitch” their biological design, often with humanitarian justifications, such as simple infection detection tools for the

²⁵⁹ Marcus Wohlsen, *Biopunks: DIY Scientists Hack the Software of Life* (New York: Penguin, 2011), 23-24.

developing world, or justifications by way of significant health problems worldwide, such as AIDS. The justifications are grandiose relative what is currently able to be produced within the context of the competition.

After Brown finished his MSc at Cambridge, Rettberg offered Brown a position as an iGEM ambassador, travelling around the North East US in order to recruit new teams and cultivate established relations with old teams, including dealing with submissions to the Registry. Some of the issues he had to deal with were familiar ones for those running the Registry and the competition more broadly: How to describe a “composite part,” i.e. a “part” which is made up of several parts such that it codes for a specified function? Is this different from a “device”? How to deal with new assembly standards, which have been invented in order to overcome different kinds of biological problem? One standard, it became clear in the competition’s short history, has not been enough.

These problems are recognized within the community, and the ‘ontic’ question of what a part is, constitutes an organizational, as much as scientific and philosophical question. Techno-scientifically, whilst it may be possible to *physically* compose sequences of DNA using BioBrick standardized prefixes and suffixes, in practice it is not clear that such composition would have the *functional* outcome envisaged. This discrepancy between physical and functional composition threatens to put the principle of an abstraction hierarchy into jeopardy. This abstraction hierarchy is an important organizational concept for synthetic biology. As a conceptual frame it is generative, but seems in operation to have a number of critical limits. The abstraction hierarchy presupposes that you have basic units out of which more complex entities are built. The first published analysis of available collections of BioBrick parts in the Registry, suggests that empirically and as it currently stands there are interrelationships between “basic parts” and more “complex designs,” which are inconsistent with the proposed abstraction hierarchy.²⁶⁰ As the authors point out, because “parts” are the bottom of the abstraction hierarchy there should be no inclusion relationships between them. In fact the study discovered relationships of inclusion between basic parts, as well as more complex part sequences present in the sequence of supposedly “basic” parts.²⁶¹

Indeed, even the “Parts” Thrust Leader of SynBERC, Wendell Lim, at one point exasperated publicly “let’s just get rid of the distinction between parts and devices!” The exasperated request, to focus on problems and methods, rather than reifying what in effect was a heuristic distinction, was not heeded on scientific and political grounds; SynBERC as an organization is funded on their being able to

²⁶⁰ Jean Peccoud et al, “Targeted Development of Registries of Biological Parts” *PLoS ONE* 3, 7 (2008): e2671.

²⁶¹ *Ibid.*

operationalize such a distinction, and furthermore, the organization takes it as a challenge, and not necessarily a pre-given fact, to be capable of treating biological systems in terms of such an abstraction hierarchy.

Disaggregation: From Parts to Problems.

Sitting with Brown in the office of the Registry of Standard Biological Parts, in the Stata Center of MIT, there was a programmer, a web designer, and two bioengineers, entering specifications into the database and re-designing the Registry interface. As he narrated it, it used to be the case, that iGEM, Synthetic Biology, the Registry of Standard Biological Parts and BioBricks were inseparable. In 2007, he lamented, they were becoming disaggregated; “People are rebranding their work in relation to synthetic biology, but they are not changing their practices, they are just re-describing it. They are not really doing parts.”

His attitude was paradigmatic of a confidence (as opposed to assurance) in the parts based engineering, modernist, ethos. He claimed that biologists struggle with abstraction and suggested that “it’s easier for an engineer to learn biology than for a biologist to learn how to think in engineering terms.” Brown is, needless to say, an engineer by training, and this speech act is highly debatable, although not rare. What is clear is that iGEM is a very powerful image for thinking with biological substance in engineering terms.

Currently, the conceptual apparatus of iGEM defines the kind of problems people work on in iGEM. In many other work situations, however, it is the kind of problem you work on that will define the kind of conceptual apparatus you need in order to work on your problem. For many of those people in the community who dedicate their summers to making iGEM happen, the rest of the year they work on different kinds of problem, through different although related concepts. The point is that you are limited in what you work on if you only think in terms of “standardized parts,” although standardized biological parts, whether BioBricks or another specification, may be one element of a future biological engineering.

The great benefit, however, as narrated to me by the director of the ETH team, Sven Panke, in whose laboratory I would later conduct fieldwork, is that it highlights a difference in the temporality of pedagogy between engineering and biotechnology. Biotechnology is usually characterized as being a very long apprenticeship, whereas mechanical engineering is characterized as faster and more hands on. The fact that iGEM is modeled on the engineering ideal, and specifically robotics competitions, is important for this re-stylization of biotechnology. Panke suggests that iGEM changes the image of work in the making and study of biology away from an image of ‘it takes too long and nothing works’ to one of ‘it’s cool and fun.’

Attention to the actuality of work in synthetic biology showed that “parts, devices and chassis” was useful as an orienting idea, but in practice work proceeded as “a diverse ensemble of projects concerned with parts, pathways, genomes, and systems.”²⁶² This four-fold distinction was an observation of the actuality, and not only the manifesto organization of work in synthetic biology.

The “parts” approach popularized by iGEM, was for the purpose of designing and building biological circuits. An important part of the use and re-use of such standard parts was that they could function regardless of the context in which circuits would be used. This was supposed to be the modularity of parts and devices within host organisms (“chassis”). When we look to the activities being done by some labs, both within the iGEM competition and without under the banner of synthetic biology, it shows that for the successful design and building of novel biological functions for application, context will need to be factored in, and the use of engineering abstraction calibrated to the particularity of the biological phenomenon being inquired into.

In a diagnostically useful review of the state of synthetic biology, Purnick and Weiss explain succinctly the achievement of the initial phase in synthetic biology;

“The tremendous increase in the availability and characterization of devices and modules provides an important foundation for the field. These efforts have improved our quantitative understanding of natural biological processes and have helped us to establish design principles that work for small modules.”

‘Modules’ in their vocabulary include things such as genetic ‘switches,’ ‘cascades’ and ‘oscillators.’ These modules can be used to regulate gene expression, protein function, metabolism and cell-cell communication. These are usually aimed at controlling isolated cellular functions. Even without the full abstraction hierarchy these modules, made of better characterized and reusable parts, were of use across a range of approaches to the engineering of biology. Insofar as details of most biological environments are poorly understood, making and testing modules from ‘better known’ elements allows more determinate knowledge of biological systems to be produced. As Purnick and Weiss point out, however, there was a large increase in the number of people building and characterizing genetic modules, but the complexity of what was being made was not increasing. As they wrote,

²⁶² Rabinow and Bennett, *Designing Human Practices*.

“To be effective, we need to learn more about the systems that we are manipulating and to dynamically incorporate this information into our design strategies.”²⁶³

Standardization of modules per se was, in other words, not the right problem. Whereas traditional engineering practices typically rely on the standardization of parts, the complex and intricate nature of biology makes standardization in the synthetic biology field not only difficult, but possibly not the best way forward. So, what then is an “effective” design principle for biology if not standardization?

Nobel prize winning molecular biologist, Sydney Brenner, put the point succinctly in his testimony before US Congress during their deliberations on the prospects for this form of engineering:

“I think that personally that the most important thing we can understand is the discovery of how devices work at the molecular level. We have three classes of devices in the world -- or gadgets if you like. There are the devices that come out of synthetic organic chemistry. There are the devices that come out of what is called nanotechnology, and there are the devices that can come out of biological systems. Now, if you look at the character of all of these, they have different modes of specification. They have different modes of fabrication. And the area that is completely missing is how we put this all together to make things that are of use and that function in the real world.”

Brenner developed this point in conversation with members of SynBERC including, Leonard Katz, whose role it was to develop Industry Relations. With respect to the manifesto of synthetic biology, Katz reflected,

“I think standardization comes about when you’re considering parts and devices as standalone pieces.”

Brenner: Well, I’ve been looking at all these mechanisms, which—it’s all mechanisms based on how electrons are moved through membranes in bacteria. And in which each of the components does not make sense by itself. You couldn’t draw them all—you have to construct the whole thing. You can say what the parts are, and you can probably get them as pieces of DNA, but in order to make the device, you have to integrate it.

So if you want to do engineering, it’s no use getting all the parts—you have to have the framework. You have to have what I call the integrated design, which is always implicit. So you have these assembled things in higher

²⁶³ Purnick and Weiss, “The Second Wave.”

organisms, like ourselves, which I think these are the gadgets that we really need to study—you know, these things that open and close. I think all these BioBricks can just be—you know, it belongs to an outmoded form of engineering. It's Detroit engineering, rather than Silicone Valley. It's an outmoded form of engineering: that you build chassis and this, and you assemble the parts.

I think they're thinking of this assembly—it's Detroit again. See, everybody starts to think of everything as an assembly line.

LK: With off-the-shelf parts ...

SB: You get off-the-shelf parts, and you start with the chassis, and you move it down, and you stick on the seats, and you go down and do all of these things. This is the wrong model. And a lot of people think of metabolic pathways like this as well. It's the wrong model. It's not like that—it's not the assembly line. The only place where we often directly hand things from one step to another is with electrons, because you can't dissolve electrons in water, basically—you lose them. So, electrons are handed from one component to another, but that's interesting.

So the assembly line is wrong, and if you have the wrong concept in terms of how the cell works—not in terms of mechanism, *but in terms of organization*—you will build the wrong things.

This whole idea that we're going to have components—that there are going to be all these guys in overalls and spanners in their back pockets—you know, and "I'll have a dozen of them, of that lot there. No, no, not that lot, I like *that* lot better. And then take it home and assemble it. It's the wrong concept, actually, to teach. If you want to do synthetic biology, it's the wrong concept to teach students, because that was *not* the way it was made. See, it's your imposition of that structure on it. But I'm just saying, there are alternatives.

Design principles needs to be oriented to the combination of "modules" into complex synthetic pathways such that complex cell behavior can be engineered: As Rabinow and Bennett reflected on the change in strategy indicated by Purnick and Weiss, the aim is "to identify domains within the cell or cellular populations in which biological complexity holds the promise of being manageable and potentially open to strategy leveraging."²⁶⁴ Which domains? Work in the first wave on "modules," will contribute to the design and proper functioning of synthetic metabolic, pathways, systems and re-designed genomes.

²⁶⁴ Rabinow and Bennett, *Designing Human Practices*, 114.

Biobricks as a means of combining components is valuable as an easy cloning mechanism along with the catalogue of components. The characterization of components, however, is more complex than the elegance of the cloning mechanism. For example, the information needed to characterize gene regulatory parts will be different from that of post-translational regulatory elements. How can intra, inter and extra cellular environments be factored in to such characterization? Purnick and Weiss make the important observation that,

“Parts and modules need to be characterized in systems and contexts of interest. These components will be most useful if they account for and adapt to the dynamics of the system.”²⁶⁵

Such accounting will also have to include the distinction Brenner drew between mechanism and organization. What the authors have indicated is that if standardization of method is not per se the issue, then the mode of composition of biological objects needs to be aligned to the kind of biological problem, environment and organizational requirements of that which is being made. The important point is that in addition to a shift in technological means, for example describing better the plurality of contexts in which the ‘same’ part may function, this shift indicates a change in the equipment underlying such a technological endeavor; synthetic biology can still be ‘cool and fun,’ but now needs to get more serious about the complexity of the biological problems being addressed.

This, in fact, was the same point of entry for our work in Human Practices broadly, albeit in a different register. The complexity we were indicating was ethical and political, and the techniques we were interested in developing was the manner in which such complex environments could be taken into account when trying to build such biological objects. Just as the biological system-biological environment dynamics need to be taken into account, according to a mature view of synthetic biology, so too the ethico-political environments need to be attended to with respect to the scientific-ethical activity of building biological systems. In the chapter that follows, I further specify this problem of building biological functions with respect to biological, political and ethical milieus, with respect to the topic of security.²⁶⁶

²⁶⁵ Purnick and Weiss, “The Second Wave.”

²⁶⁶ The similarity to the general point made by the “sociology of translation” is not lost on me. i.e. I do not begin with an a priori distinction between things ‘natural’ and things ‘cultural’ or ‘social,’ and explain the former by way of the latter. Nevertheless, anthropologically I insist on the Weberian–Nietzschean requirement, named in the Introduction, of requiring a “point of view” from which to give significance to the relations analyzed.

A Claim and a Problem: Function and Significance.

A Claim: Synthetic biologists, or molecular biological engineers broadly, recognize that an engineering principle alone is insufficient, with respect to a properly biological and technological aim; to make living things better and better living things you need to take into account the specificity of the problem which the engineered mechanism or organism is supposed to solve and the environments it is supposed to exist within.

*A Problem: And yet, even though it is recognized in the ‘community’ that the technologies and functions are problem oriented and milieu specific, they rejected the milieu specific problems of their **practice**:*

The 2007 Berkeley iGEM project was distinctive insofar the project was aimed at building a whole system, not only the construction of a circuit or device. In that respect it pre-figured the later critiques of ‘first wave’ synthetic biology. This is perhaps not surprising since Weiss and Purnick identified the Berkeley team leader, and SynBERC PI Chris Anderson’s work, as characteristic of “second wave” synthetic biology. They included his work in what they called, Application Oriented Systems, whose aim is: “programming cells to produce macro functions,” systems level control of gene expression, creation of novel enzymes. Most such systems, such as the Anderson lab’s Tumor Killing Bacteria, a “living computational therapeutic” which I discuss in the next chapter, need to be operated within cells and cell populations, and so require attention to context.

The 2007 Berkeley iGEM project was to design and build an Application Oriented System. They began with a ‘real world problem’ the need for “inexpensive, disease-free, and universally compatible blood substitutes,” for short term use in conditions where whole blood is unavailable. A justificatory reference to the developing world was used in the project rationale. The system was designed to perform the function of red blood cells, which transport oxygen and remove CO₂ through the protein hemoglobin and a necessary small molecule called heme. The system consisted of a heme biosynthesis module, a hemoglobin generation module, a chaperone module (which helps the protein fold), and a detoxifying module. The system was housed in the *e. coli* chassis developed by the Anderson lab, a chassis that is designed to be safe to introduce into the blood stream.

To John Dueber and Chris Anderson, the project’s senior coordinators, who were at this time post-docs at Berkeley, I proposed that we try and experiment with Human Practices within the iGEM project. This was six months into our SynBERC experience, and so was characterized by the hope that collaboration could be forthcoming, even if the first months in Center were indicating the contrary. I drew up a list of points to help orient the collaboration and to have a shared

understanding with the project leaders of what the aim would be. I wrote to them a list of points, thinking that a bulleted charter would be more persuasive than an essay:

1. Integrating human practices is not a challenge of making biologists do something they would not normally do.
2. The division of labor is strategic.

Human Practices frames a division of labor in a manner in which those non-technical questions of how to think more broadly about the technical project are not “outsourced” to law schools (for example). Questions generated within the scientific milieu will be explored in wider domains to be brought back into a discussion of the technical science.

3. As such the task would be to integrate an undergraduate Human Practices member of the iGEM team.

The idea would be to select a highly motivated, curious and invested undergraduate interested in synthetic biology itself, new forms of collaboration in the human and natural sciences and willing to commit to the project.

5. The task for this undergraduate:

A sophisticated articulation of the question why is this chosen iGEM project important, what are the questions the project is attendant to, what are the stakes of this project and how one might go about answering those questions

6. The Human Practices element of the iGEM project would have two parts;

a) an investigation of the project “in principle,” i.e. how can we account for the principles on which the project is based and the goods toward which it works and

b) to investigate how we could evaluate this question “in practice” both as the project unfolds and in the imagined stakes of a final product e.g. safety, IP, end-user concerns, externalities etc.

Ultimately this would entail a translation of the “in principle” significance to “in practice” significance of this project.

7. As such, this project specific work would have two costs associated

a) the Human Practices undergrad would have to be allowed in as part of the team

b) allow the member to participate in the presentation at iGEM with one slide and two minutes [of a ten to fifteen minute presentation] to report what they found out.

In terms of outcomes, one might characterize this experiment as a success; whilst the team did not win the grand prize, they were finalists and were furthermore awarded the “best poster” prize, in part, as judge George Church said, because of the inclusion of Human Practices reflection as part of the scientific endeavor.

Discordance.

The manner in which the undergraduate anthropologist was included within the team, however, was discordant, both during the summer and then during the presentation at the iGEM competition. This was a troubling experience, for myself and my colleagues in Human Practice, and became emblematic of many of our difficulties in pursuing further collaboration.

Participants in iGEM are judged not only relative to their presentations at the event, but also with respect to a virtual laboratory notebook (in wiki format). All team members are responsible for keeping an informative and tidy lab journal, which connects their individual activity to the larger project, the anthropologist included. The typical contents of a wiki would include daily descriptions of procedures and tests along with a list of newly designed parts, made specifically for a team’s project. As the young anthropologist narrated it, “about three weeks into field work in the lab, I posted my first notebook page onto the wiki about my orientation onto the team and why I was doing conceptual rather than technical research for the team. This first wiki posting would lead to a meeting between my team advisors (Chris Anderson and John Dueber), my human practices advisors (Paul Rabinow, Gaymon Bennett, and Anthony Stavrianakis), and myself that would result in me not only figuring out my research concept but also the power struggle over my research between my teams advisors and Paul, Gaymon, and Anthony, my faculty advisors.”

Fuller’s initial wiki posting had concerned safety and security issues within the lab and had been construed by the supervisors as having framed the team in a negative light. I did not have access to the wiki and took it as a principle of pedagogy that I would not “check” what she was writing unless she asked me for advice. It was, according to her, a misunderstanding. As such, a meeting was called in order to re-set the terms of the engagement, and to figure out together in what anthropological collaboration in iGEM might consist in.

Fuller was of course right; this was, to a certain extent, a question of the connection of truth and power. As she continued in her account to me that, “Chris

and John both made it clear that they found my involvement in the team was very interesting; however, did not understand what I was doing and fear that I was going to cause them to lose their means of funding. Once Paul had explained my presence [in the lab] in the meeting to Chris and John, Chris made suggestions as to what I could focus my research on, such as: what would be the steps after animal testing or how would a product like BactoBlood be patented when its parts are open-sourced.”

In this meeting, the three of us, Rabinow, Bennett and myself, re-iterated to Dueber and Anderson the fact that Human Practices involvement with SynBERC was predicated on not providing technical solutions, e.g. what’s the best way to patent, or what are technical steps in navigating the rules involved in animal testing, but rather our involvement was predicated in problematizing the relation of these emerging practices and a series of domains, which might include property questions, experimental animal models and ethics. If this were to be a Human Practices engagement on IP, then it must be IP as an anthropological problem.

In effect, this is exactly what Fuller did. After spending a month in the lab learning basic methods, although not doing the experiments, and after gaining a good understanding in what the iGEM mode of engineering consisted in and furthermore, what the ethos of open source biology consisted in, she posed a question: given these practices (such as BioBricks), and given this ethos of open source, is there not a tension between this ethos and practice, and an ethos and practice that justifies itself relative to the solution of real world problems, such as health. I.e. if work is justified relative to its capacity to circulate in the world as products, and if products under contemporary conditions are facilitated by an industrial landscape which includes patents, is this not at odds with the mode and practice of iGEM?

The Veto.

As agreed with Anderson and Dueber, Fuller would have one slide and two minutes to present her summer research (the total presentation time was 15 minutes, and so this division seemed reasonable.) A week before the iGEM competition Anderson asked to review Fuller’s part of the presentation. Her presentation introduced Human Practices, herself as an anthropologist, and framed her summer research as the question of open source organization of biological practice relative to patents, given that there is a commitment within the community to open source but that to become a “real world solution,” any product coming to market must navigate a very tough IP terrain.

The presentation was vetoed and then censored in two respects: Fuller was told not to introduce herself as an anthropologist, on the grounds that “people

won't understand and it will be a distraction"; secondly, she was told to focus only on the part of her research which states, in technical steps, how in practice one could patent an invention such as BactoBlood, which eliminated the anthropological problem relative to which these technical steps could take on significance as problematic.

This was, of course, a very distressing situation for those of us trying to collaborate. This turn of events was discordant, with no clear means for remediation or rectification. In consultation with Fuller, and my Human Practices colleagues, we agreed that we did not have the authority or position from which to do anything about it. This feeling of futility had been further strengthened by the fact that in late August we had begun a Townsend Center for the Humanities sponsored Synthetic Biology course, which was supposed to be co-taught by Keasling, director of SynBERC and Rabinow. After I had sent several emails to Keasling in August regarding possible syllabi, to which he did not respond, it became apparent, via his secretary, that he would not be playing a major role in the course, if at all. Our participation in the Center, it became clear to me, was entirely superficial the biologists.

The Q& A.

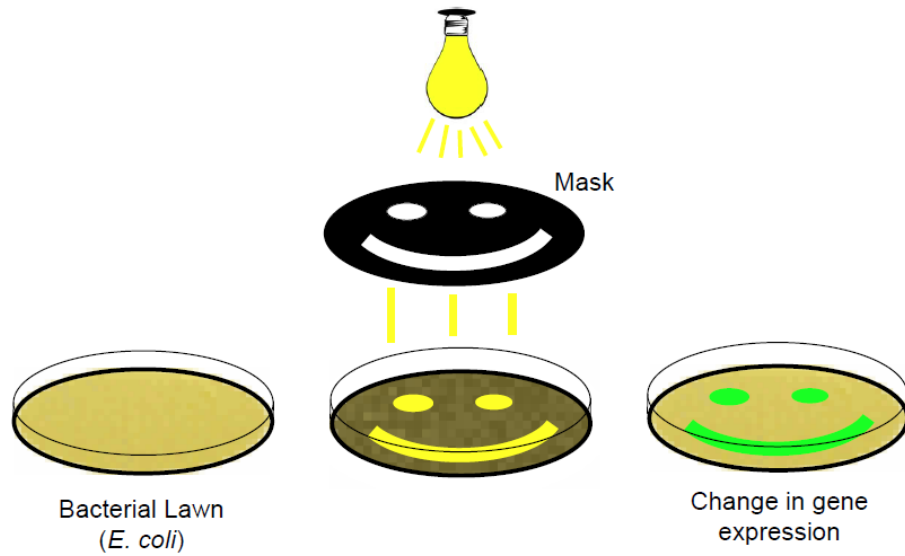
During the iGEM presentation itself, two episodes transpired which were remarkable, both occurring during the questions period. The first was that an audience member, a senior biologist from Imperial College London, who now runs their synthetic biology center, raised his hand and made the following comment: "Why are you talking about patents? iGEM is supposed to be about fun. It's meant to be a fun summer thing; I don't think this gives the right impression, all this talk about patents, that shouldn't be your concern." Apparently there are times and places for modes of subjectivation. The biotech guru who is as comfortable with Venture Capital at Sandhill road, patent lawyers at the Boalt School of Law, and wet lab work in Mission Bay, is not the figure some want to see at iGEM; this is good clean fun, so don't bring up anything serious. Remarkably the presentation which followed was on a re-engineered lentivirus for attacking AIDS, i.e. to use a virus similar to AIDS to attack AIDS: "Fun," indeed.

Fuller responded to the professor from Imperial that the reason she was working on the topic is that she was an anthropologist, part of the Human Practices effort within SynBERC, and treating iGEM as a test-case for the possibility of collaboration between the human and natural sciences. The professor from Imperial followed up, "oh well, you should have said so, right yes, I understand." What he understood then, on one interpretation, is that if she had been a biologist it would have been inappropriate to waste (presentation) time on such issues, but that

since she is an anthropologist, the outsourcing of these kinds of question to others is “understandable.” His comment gets precisely to the heart of why there is a blockage in forming common problems on which practitioners of different forms of knowledge could co-labor.

The follow up question was from Drew Endy. He posed the question, exactly in the form that Fuller had originally wanted to present the problem; He disingenuously asked, “how can you have patents and open source?” Fuller was nervous and clearly froze, saying she didn’t understand the question. In fact, there was not a question, it was a description of a contradiction present in the situation of iGEM relative to wider milieu of bioengineering activity. Endy himself is exemplary of the contradiction; he flies around the world presenting a vision for open source biology, while at the same time founding companies, such as the now defunct Codon Devices, which was not operating an open source platform.

Bacterial Photography



Voigt Lab Presentation, 2007



Image from Ellington Lab homepage,

“Synthetic biology: Engineering *Escherichia coli* to see light,” *Nature* 438, 441-442 (24 November 2005) | doi:10.1038/nature04405; Published online 23 November 2005

“BACTERIAL PHOTOS”

FIGURE 3.

Chapter Five

Meditation: Preparedness

“Insofar as we are geometricians, then, we reject the unforeseeable.”

–Henri Bergson ²⁶⁷

The unexceptional episodes described in this chapter are narrated for analytic and diagnostic purposes; how, after an expectant phase of design, were efforts to get biological researchers to care for the problems named by Human Practices buffered and neutralized (rather than rejected *tout court*)? I analyze this through one instance of an ethical-political problem associated with the work of the Center, namely security. It was in our second year in the Center that it became clear to us that this problem was imperative precisely because it was emergent from the biologists’ own practices. We thought this problem arising from their activity was precisely the kind of problem on which collaboration would be not only possible but necessary.

The ethical stakes of this problem was not taken on by the management of the organization. Our incapacity to affect the organization in the manner which we intended, and our eventual recognition of this situation, is what I seek to diagnose as the second purpose of the chapter. ²⁶⁸ The lack of collaboration will be diagnosed as an institutionally fostered and trained incapacity, which is to say the flip-side of the powerful technical capacities fostered in the organization. ²⁶⁹ This incapacity is, I think, symptomatic of a wider cultural difficulty in communities that produce technological change and the social environments affected by such change. This difficulty is the incapacity to pose ‘other’ ethical questions, which is to say, questions outside of the dominant justifications for the pursuit of utility by (bio)technical means. These sets of powerful technical, scientific capacities function as parameters of how ethical questions can be formulated, heard and worked on. Both the technical capacities and the parameterization of the ethical

²⁶⁷ Henri Bergson, *Creative Evolution* (Dover Publication, 1998), 45.

²⁶⁸ Favret-Saada, Jeanne *Désorcèler* (édition de l’Olivier, 2009), 157 Favret-Saada’s account is of an efficacious relation of affection and much of the descriptive effort is in showing how she was caught in the language and practice of witchcraft and unwitching. She shows how being caught involved taking up a position in a set of relations. What I am describing is a failure to establish such relations of affection.

²⁶⁹ Thorstein Veblen “The Instinct of Workmanship and the Irsomeness of Labor” *The American Journal of Sociology* Vol. 4, No. 2 (Sep., 1898): 187-201, 195. Kenneth Burke, *Permanence and change: an anatomy of purpose* (University of California Press, Berkeley and Los Angeles, 1954), 7-9.

field are fostered by a particular affect.²⁷⁰ The situation I describe has characteristics which assist me in trying to understand how the ability to affect and be affected in a certain manner is constituted by a field of relations with a certain quality, density and intensity. To attempt to show the affect of this field through these actors speech and actions, is preferable to writing of Raymond Williams' well-known "structure of feeling" since it focuses less on the manifestation of a structured existence in the individual as 'experience' and more on the formation of a 'state' that produces a manner of thinking about ramifications of science, or in this case, the security problem.²⁷¹

Problem: No Problem

In 2007, to the clarification of what we had meant by the question of flourishing with regards the security problem, the reaction was vehement; if we were going to raise the problem, then we had better propose a solution. Both at the first site visit and consistently through all the weekly telephone conferences, pizza lunches and the regular conversations in the labs, we had left our colleagues unmoved. After these experiences, our intra Human Practices collaborative work was to continue to think through the problem of ethical and political relations of governance. We continued to develop our analysis that whilst government regulations and laws are necessary, 'self-regulation' under these conditions and with new technologies, will require a different formation in order to engage with the complexity of these circumstances. One of the dynamics at play, on reflection, was a pedagogical dynamic and the unresolved question of who the ethical pedagogue was in the situation.

Our subject position, in the capacity as the Human Practices thrust of the Center, was nominally mandated to articulate ethical concerns. This position, however, was not recognized as such in the way in which we had wanted to engage it. More familiar ethical masters, however, were and are recognized; Institutional Review Boards and industrial and scientific advisory boards, are engaged with and usually submitted to, since these are authorities with actual jurisdiction and recognizable limits.

Part of the ambiguity was that we were not asking the students or PIs to subject themselves to an ethical command, but to engage with an ethical work. If the reader were to think this is a flight of fancy or fantasy on the part of a novice, let me be clear, the NSF asked the students and the senior management to do the

²⁷⁰ One might say following James Faubion, how the dynamic between system maintenance and system change is managed relative to the changing environments in which the system exists. For Faubion's extension and re-working of the problem of the ethical field see, Faubion, *An Anthropology of Ethics*, 20-24, and especially, 104-145.

²⁷¹ Raymond Williams, *Marxism and Literature* (Oxford: Oxford University Press, 1977).

same. The NSF site visit team re-iterated our concern after the first year event in 2007. The NSF, in a written report, was clear that the active initiation and implementation of Human Practices could not be left to the fourth thrust alone.

Reasoned Discourses: Major & Minor Stances.

The changing environments in which synthetic biology operates, we thought, gave pause for thought about the adequacy of the governance technologies that had been developed for the molecular sciences since the 1970s. Those technologies focused on laboratory safety and the limited circulation of viruses, bacteria, and toxins (among other things) that are considered to pose a substantial threat to human health. Our interrogative mood was based on two things: In the short term, it was based on our analysis of several reports on the governance “options” for synthetic biology.²⁷² In the longer term, it was based on several years of prior work by members of the Anthropology of the Contemporary Research collaboratory, on a range of topics including mapping governance rationalities for biosecurity.²⁷³ This work had a focus on “preparedness” rationalities and governance approaches to ‘low-probability–high impact’ events.²⁷⁴ As a general orientation, this work has shown the development of a particular governance rationality which takes as its concern future security events for which there are inadequate probability calculi (the hurricane ‘Katrina’ for example was expected, but not the event “Hurricane Katrina.”)

In the reports that we analyzed, we identified three topics; technological innovation for the production of biological objects, ecological and political environments in which these biological objects and technologies will circulate, and the ‘uncertainty’ of the effects of scientific practice. These were certainly the right topics, but we thought that there were some limitations to the manner in which the topics were approached. These reports were explicit about the non-technological challenges of controlling the ramifications of these emerging technologies. Given this recognition, it was strange that technological approaches to such control were taken as sufficient. For example, the reports advocated confronting the dangers associated with engineering organisms, ranging from environmental problems to bioterror, through the development of technical safeguards built into the organisms

²⁷² The Sloan foundation sponsored report, *Synthetic Genomics: Options for Governance* was produced by ‘policy experts’ from the J. Craig Venter Institute (JCVI), the Center for Strategic & International Studies (CSIS), and the Massachusetts Institute of Technology (MIT), of which the then MIT member was also a member of SynBERC. A second white paper, “*Community options for governance*” was produced by Steven Maurer, the public policy professor who was excluded from SynBERC, with two colleagues from the Goldman School of Public Policy at Berkeley.

²⁷³ Andrew Lakoff and Stephen Collier, eds., *Biosecurity Interventions* (Columbia University Press, 2008).

²⁷⁴ Stephen Collier, “Enacting Catastrophe: Preparedness, Insurance, Budgetary Rationalization” *Economy and Society* Vol. 37, No. 2 (2008): 224 – 250.

and through technologies to regulate who has access to what materials.

The authors of these policy recommendations recognized that the domain of practices calling itself ‘synthetic biology’ will increase the capacities to engineer organisms, and that the political and technological environments make such technologies and capacities ever more widely available. The question for biological engineers, and others who had ethical and political responsibilities relative to this work (ourselves included), was how to think about and act relative to the capacities for engineering biology that are being accelerated. Our analysis of the security problem, relative to current circumstances, was to take as a starting point the claim *that there will be an event, of unknown form and timing*. This preliminary step was interesting to me in part because it required an engagement with how an ‘other’ rationality can be made operative and what form statements about security could take if such an event is predicated of the future. The rationality could be taken seriously only if an inadequacy in current forms of thinking were recognized relative to this disruptive event predicated of the future. This way of thinking needed to engaged collaboratively, before asking what can be ‘done’ on the basis of such thinking. Since this way of thinking is not ‘prediction,’ but rather an ‘ethical’ and ‘temporal’ orientation, the solution cannot be a white paper or a rule.

This “meditative” (rather than methodological) move—predication of a future unknown event relative to which current activity should be thought—was one response to the inadequacy of principles of precaution. Such principles ask that in the absence of scientific determination with regards the danger of an action, the burden of proof is on those taking the action to prove that it is not harmful. With a sufficiently (and realistically) wide definition of danger and harm in our situation, such proof would be hard, if not impossible.

Our approach instead was to suggest that given the engineers’ commitment to inventive activity in biology, there is an obligation to reflect on the range of dangers which are not controllable. There is also a corresponding obligation to reflect on what is currently scientifically under-determined with regards to these biological systems. It is this incapacity to be moved by another way of thinking that I will explore first by looking at how statements about the dangers were able to count as significant for one senior biologist as compared to those who would pose limits to this biologist’s manner of thinking about the problem.

The Veridictional Form of the Affect of Security: Exciting *and* Safe! 2008

George Church is professor of genetics at Harvard Medical School, a SynBERC Principal Investigator and one of the instigators of the Human Genome Project in the late 80s and early 90s. He is a tall, calm and serious man with a deep tone and regular cadence to his voice and speech. He has, throughout his career,

consistently advanced technical capabilities and understanding in molecular biology.²⁷⁵ He, along with several others in SynBERC made efforts to engage the political considerations of their work, especially with regards the political effects of increasing capabilities in DNA synthesis.²⁷⁶ Such efforts consisted in sitting on numerous boards as well as participating in Industry coordinated groups for thinking these questions, the Industry Association Synthetic Biology and the International Consortium for Polynucleotide Synthesis. The chemical synthesis of DNA has been around for roughly 40 years.²⁷⁷ What has changed is the length and complexity of the synthetic constructs. This has moved fabrication from the introduction of single genes into cells towards the fabrication of whole genomes. Technological improvement is having effects on time and cost of operations and complexity of constructions. This technological change is alarming to some environmentalists and along with changes in international politics since September 2001 this work has provoked some concern in government agencies as to the ‘unintended consequences’ of this work.²⁷⁸

In February 2008, there was a large turn-out at Genentech Hall, part of the UCSF Mission Bay campus, for a pre second year site visit workshop on security. Church gave the lead presentation on the unintended consequences of technology. The key themes from this discussion were a critique of the precautionary principle and a proposal for the technical means by which the “unknown” dimensions of

²⁷⁵ To give an indication of his credentials I select three early papers, 1977, , 1984 and 1988. The first is on the structure of DNA – Protein interactions. The second is paper from 1984 on genomic sequencing and the third a paper on a more powerful approach to sequencing. George Church, Sussman JL, and Kim S-H, “Secondary structural complementarity between DNA and proteins.” *Proc. Nat. Acad. Sci. USA* 74 (1977): 1458-1462. Church GM, and Gilbert W, “Genomic Sequencing,” *Proc. Nat. Acad. Sci. USA* 81(1984): 1991-1995. Church GM and Kieffer-Higgins S, “Multiplex DNA sequencing,” *Science* 240: (1988): 185-188.

²⁷⁶ George Church, “Let Us Go Forth and Safely Multiply,” *Nature* 438 (24 November 2005): 423.

²⁷⁷ Michael J. Czar, J. Christopher Anderson, Joel S. Bader and Jean Peccoud “Gene synthesis demystified” *Trends in Biotechnology* Volume 27, Issue 2 (February 2009): 63-72. The last thirty years has seen the invention of the sequencing, pcr, in vivo homologous recombination (and subsequent recombineering techniques).

²⁷⁸ In 2004 the National Research Council produced the report *Biotechnology Research in an Era of Terrorism* (The Fink Report). Whilst this report did not lead directly to the establishment of the National Science Advisory Board for Biosecurity (NSABB) it was instrumental in a systematic review of the current challenges facing scientists, policy makers and government. In addition to older institutions such as the Recombinant Advisory Commission, newer institutions such as the NSABB and more broadly the developments in the Department Homeland Security, there have been initiatives from numerous research groups on control, oversight and regulation of emerging areas in the biosciences. The moves might be categorized as follows: firstly the older safety institutions, such as the RAC wish to update their codes and regulations in line with rapidly expanding synthesis technologies (of which ‘synthetic biologists’ are the key movers of this technology). Secondly, institutions such as but not limited to the NSABB and public policy research endeavors are attempting to frame the problem of biosecurity in a post-911 milieu as a problem of ‘dual use’ technologies, i.e. technology can be used for ‘good or bad purposes’ so let’s figure out who is going to be good and who is going to be bad. . Thirdly, post-911 and post-Katrina (as well as other natural disasters) the US government has much effort in trying to build preparedness capabilities for significant events, through the Department of Homeland Security, among others

Cf. Homeland Security Presidential Directive -8 “National Preparedness “ (HSPD-8, 2008) Homeland Security Presidential Directive -21 National Strategy for Public Health and Medical Preparednes, (HSPD-21, 2007) .

experimentation can be taken up as parameters to allow decisions to be made and for science and technological research to move forward.²⁷⁹ ²⁸⁰ He was the lead on the “chassis” work being done within SynBERC at the time he gave the talk. Chassis, the reader will recall, refers to redesigned cells and genomes capable of housing and acting as ‘power supplies’ for sustaining the proper functioning of designed biological systems. One of the principle safety challenges with regards to this work is the fact that these proposed re-engineered cells will integrate with ecosystems (and bodies). The biotechnological challenge is to design ‘safe’ chassis that will not cause ecological disasters, or in the case of medical therapeutics, death. The governmental challenge, in one framing, is to control who has access to the tools and knowledge for how to engineer such organisms.

Church began the talk indicatively, his assurance warding off any hints of sarcasm that may have been summoned through the coolness with which our collective predicament was narrated; “Many technological developments seemed like a good idea at the time.” To have put this in question, to borrow a phrase, would have been tantamount to moral ill-will against those technophile visionaries who shape the worlds many people live in. “However,” he acknowledged, “they had huge unintended consequences.” Church gave several examples: irrigation led to increases in malaria, settling near rivers resulted in increases in cholera and better pesticides with increased use resulted in a “silent spring.” This, of course, was the bad news. Matter of factly, he stated that the world has simply too many problems, and our responsibilities are too great to do nothing. “Technological paralysis,” he cautioned, “is not an option.” We have seen from the negative cases that solutions to problems carry potential for disaster. Not only that, but often building in safeguards can be very difficult and costly; sometimes so costly that it either makes designers not want to include them, or forgo the development of the technology without them.

For Church there are lessons to be learned from the negative case. Biotechnology, in his assessment has ‘potential’ to find biological ways of solving problems, such as carbon sequestration, which build in necessary and carefully designed safeguards. The point was that in biotechnology, in this sub-field and in his laboratory, they are designing methods that can both make biotechnology safer

²⁷⁹ As Carlo Caduff points out; “Precaution differs from prudence in that it primarily refers to risk management rather than risk assessment,” unpublished manuscript, *The Futures of Risk*, 15 ²⁷⁹ Ewald, F The return of Descartes’ malicious demon: An outline of a philosophy of precaution .In T.Baker and J Simon (Eds.) *Embracing risk: The changing culture of insurance and responsibility* (pp273-301) Chicago, IL: University of Chicago, 2002. Cooney, Rosie Dickson Barney *Biodiversity and the Precautionary Principle: Risk and Uncertainty in Conservation and Sustainable Use* (Earthscan Ltd 2005).

²⁸⁰ Hans-Jorg Rheinberger, *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube* (Stanford University Press, 1997), 24-37 and 133-142.

and more useful in bringing about a future with less global warming and less treatable disease, to give two main problem areas that the biologists refer to as justifications for work.

The first trajectory in designing safer chassis is engineering the dependency of the cell on molecules not found readily in the environment. The second is to intervene by designing a new genetic code—and for instance changing the chirality of certain macromolecules—which would make genetic exchange with other organisms very difficult.²⁸¹ The engineering side of the challenge has been taken up extensively by the Church lab.^{282 283}

Stances to the Future.

The key question for those taking the ‘long view’ is, even if it is possible to engineer such cells, a veritable feat in itself, how do you test such organisms? On this question there has been less news and curiously very little scientific discussion within SynBERC, even though there is quality work being done by microbial ecologists. At the 2008 meeting, in the audience was Dr. Margaret Race, an ecologist whom we had met through an interlocutor of Rabinow’s, Dr. Michael Ascher, who has been involved for many decades in infrastructure development for public health, including positions as the director of the Viral and Rickettsial Disease Laboratory of the California Department of Health Services and the Office of Public Health Preparedness. Dr. Race is an ecologist at the NASA exo-biology program. She suggested that the question of whether one can build a microcosm sufficient to test such organisms is far from clear and constitutes a genuine scientific indetermination.²⁸⁴

²⁸¹ Chirality refers to a situation in which an object or a system differs from its mirror image, and its mirror image cannot be superposed on the original object. The aim is to reduce the ability of phagocytes (cells that can ingest foreign particles) to recognize the cell and also limit the chance of the engineered cell being mutated or dominating the ecosystem.

²⁸² The general strategy is to build a cellular chassis which involves component changes to *e. coli* including pathway removal to free up promoter types and codon changes. Projected safety controls on the chassis include deletion of phage lysogens & receptors, resistance levels and the deletion of surface toxins among others. They want to add complicated or rare auxotrophies to prevent survival outside the lab.

Cf. Markus Schmidt “Xenobiology: A new form of life as the ultimate biosafety tool” *BioEssays* Special Issue: Synthetic Biology Volume 32, Issue 4 (April 2010): 322–331.

²⁸³ They have developed new methods of genome engineering. Old methods only allow for the serial introduction of single DNA constructs into cells at low efficiencies. The new methods allow for simultaneous modification of organisms and selection of desired characteristics in useful timescales. The technology targets many locations in a chromosome, either in a single cell or across a population. These multiple simultaneous changes to cells produce selectable genomic diversity and hence desired phenotypes. Those technologies he was naming in 2008 were finally reported on in 2009 and the exemplar in the paper was the isolation of variants within 3 days which had over 5-fold increase in lysopene production. The general point here is that his lab has invented a technology which will be used to rapidly engineer whole genomes to do efficacious tasks.

²⁸⁴ In the early 80s, while Race was working at Berkeley there was an intense debate and political maneuvering around the first generation of genetically modified bacteria. Researchers at Berkeley had engineered a strain of the

In the early 1980s, while Race was working at Berkeley there was an intense debate and political maneuvering around the first generation of genetically modified bacteria. Researchers at Berkeley had engineered a strain of the bacterium *P. syringae* that could inhibit frost in plant populations. The move from the laboratory to field tests went through the regular channels of oversight but was met with strong resistance from local and international pressure groups and activists concerned over the field trials of genetically modified plants. Field testing did not resolve the indetermination. Rather, the indetermination was displaced into the future. Race highlighted to me in subsequent conversations the similarities of that discussion in the auditorium at UCSF with what was going on in the mid-80s with genetic engineering.

With respect to her exobiological research within a NASA project to bring back samples from Mars, and the ecological implications of doing so, Race brought up the highly visible, and urgent, question of detection and containment. In her assessment knowledge of containment is very good, but the trouble is that over 95% of microbes are unable to be cultured. The dry atmosphere in the windowless conference room was animated by Dr Race's dissonant counterpoint:

“We are only just discovering things about the microbial world. To build the notion of safety around the idea that you can test it in a lab—you can't—you need to do it, but the idea that testing on microcosms for 500 mg of sample from Mars is still the big TBD.”

Her second major concern was that the design of new genetic codes for biological organisms is 'scary' for those outside (and some inside) the scientific community.

“When you're first working with this it makes sense, with regards to biofuels etc. you're talking about a typical ecological problem. But fooling around with chirality, that's one of the signatures we are looking at when we go to Mars and elsewhere. It's scary to the public, it's a reasonable fear. You cannot develop a microcosm sufficient to test it. If you build your system around proving it in a microcosm, then forget it.”

The response from Church highlights one of the interesting characteristics about the desire for experimentation. Whilst the complexity of these biological questions is a concern, the fundamental ethos of experimentation means that one

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should proceed as though functional simplification (and hence *prediction* of effects) of how the organisms and environments interact were possible²⁸⁵ As Church responded, his caution designed to absorb and neutralize the radical cleaved by Race's interjection;

“be careful about saying things are impossible, what are the barriers to making a microcosm that has a rich ecology in it? Take Biosphere 2, it had problems with physical containment and funding issues, but it demonstrated it is not impossible.”

He was referring to a closed ecological system in the Arizona desert used for the study of the interaction of life processes and the possibility of experimentation without harming earth's ecology. Biosphere was also intended as a venue for testing the possibility of such biospheres being used in the colonization of Mars. Race highlights some of the ecological issues that Biosphere 2 ran into, to which Church responds with both a vision of the scientific endeavor and the stakes of what such scientific research might be:

MR: “It had problems with co2 levels, trace levels which then caused massive ecosystem wide problems.”

GC: “Hard is different from impossible. The stakes are high: If the choice is remaining dependent on coal for our energy, or having chiral things escape, we need to invest money in getting biosphere 2s to work.”

In this exchange we can read the situation as the simultaneous uptake of two sides of risk. The first side is linked to the possibility of “getting biosphere 2s to work,” which is a situation in which the goods to follow from such an endeavor are worth the technical challenge and investment. On the other side is the recognition that the elements which fall out of the parameters of risk calculation will be taken up and taken seriously by systems other than the scientific. As Race's comments signaled, if science itself recognizes the limits of what simplifications are possible, for instance the fact that a microcosm might not be sufficient to know the long term ecological effects of these organisms, then the limits will be reflected on by other groups; our Human Practices group included.

Taking together the discursive materials on these topics and the experiences in SynBERC, it is clear to me that these topics existed and could be articulated through a particular field of affective relations. These affective relations involve a combination of assurance regarding what is known about experimentation with biological systems and the greater danger of not proceeding with experimentation, even though it is not yet possible to account for all future ramifications of

²⁸⁵ Luhmann, N Risk: *A sociological theory* Transaction publishers, New Brunswick New Jersey; 2008; 97

experimentation and innovation.

This affective field is in contrast to that which was constitutive of our ‘stance’ towards the issue on the Berkeley side of the Human Practices thrust, which drew on concerns such as those articulated by Dr. Race; this stance took seriously both the biological activities being done and a preparedness rationality towards future biological events. The basic difference I am pointing to is between those that were assured that technical rationality was sufficient, rendering the problem one of demonstration, and those who were oriented to the limitations of this technical rationality relative to the problem of future events.

In Search of a Pedagogy.

For most of the scientists I interacted with, the question of security and regulation were considered either technical matters, or externalized as questions of “fundamentalists,” which was a recurring trope. Such dismissals constituted an exorcism of the problem and indicated one way in which contemporary bioengineering is being limited in its capacities to take seriously a range of dangers through a set of learned incapacities. To treat the milieu relative to synthetic biology as an ethical problem is to ask how “good” science can be a question internal to the practice of that science and structured by an organizational form which fosters appropriate activity. If, for example, as many in the field recognize the Select Agents list—a list of DNA sequences known to be pathogenic and which are tightly regulated—is obviously insufficient once the know-how and tools to synthesize novel organisms proliferates, what is the relation between knowledge and right action in that situation?

The relationship between pedagogy and preparedness and the disconnect between knowledge, the unknown and right conduct, crystallized in an exasperated comment from an MIT graduate student during the questions period of the security event:

“What counts as dangerous? ...I don’t know what to tell my students ... if we’re thinking about an alternative response to security, like educating the next generation of researchers ... that is the kind of question I should be able to answer.”

Whilst it is clear that there are ‘concerned’ individuals, not only in this organization but in others too, social scientific work on security issues in molecular biology in Europe suggests that as a generalized form of activity, life science and the scientists who practice it “do not share the threat perception widespread among biosecurity experts concerning bioterrorism or biological warfare.” This claim was made by a long-term first order policy concerned academic, Malcom Dando, who

has articulated the paradox in the following fashion; “They [bio-scientists and engineers] do not think that their own work might contribute to the threat.”²⁸⁶ Some bioengineers, including Drew Endy, have made the insightful reverse sociology of science claim that the claims of these social scientists are tied up with their interest in re-producing the problem such that they can have a domain of expertise in which to circulate knowledge and gain symbolic capital. At any rate, the move our Human Practices group sought to make was not just to raise awareness of “issues” among scientists working within synthetic biology but rather to attempt to make the political ecology within which this science is being practiced a meaningful set of issues to engage with relative to their daily practice.²⁸⁷

How could we do this? The June 2007 National Science Advisory Board for Biosecurity (NSABB) report made the important point that synthetic biology is one among a number of sub-disciplines within the life sciences which have to be brought within a more comprehensive approach to the challenge of biosecurity. Such a ‘comprehensive approach’ however, following the work by Alexander Kelle and colleagues, cannot be done through a probabilistic mode risk assessment, since there are not enough cases in synthetic biology through which such assessment can be made. In Kelle’s case, their method was to conduct interviews out of which to make judgments. As an approach this was helpful for Human Practices in thinking about which capacities and deficiencies the community of researchers had in a broader perspective. Specifically it helped us see which areas are important to work on pedagogically within our research organization; namely, the integration of capacity building relative to the security challenge into daily practice.

We did this by outlining a program of preparedness exercises which the management of SynBERC would have to organize and pay for as well as

²⁸⁶ This judgment came from a large scale survey and he based the judgment on responses from 1,600 life scientists during 60 seminars in 8 countries. Alexander Kelle’s research, as part of the EU funded SynBIOSAFE project, reiterated Dando’s concerns; he conducted a much smaller set of interviews with 18 leading practitioners within the synthetic biology community on their awareness and knowledge of international governance and regulatory issues regarding “experiments of concern” in biology. Kelle, Alexander, “Synthetic biology and Biosecurity Awareness in Europe,” SynBIOSAFE Working Paper, 2007; 13 http://www.idialog.eu/uploads/file/Synbiosafe-Biosecurity_awareness_in_Europe_Kelle.pdf last accessed 4/14/11

²⁸⁷ . The June 2007 NSABB report makes the important point that synthetic biology is one among a number of sub-disciplines within the life sciences which have to be brought within a more comprehensive approach to the challenge of biosecurity. Reflecting on the work by Kelle and colleagues in Europe we see that there are not enough cases in synthetic biology through which probabilistic assessments can be made. Instead the method was to conduct interviews out of which to make judgments. As an approach this is helpful in thinking about which capacities and deficiencies the community of researchers more broadly has. Specifically it has helped us see which areas are important to work on pedagogically within our research organization; namely, the integration of capacity building relative to the security challenge into daily practice.

encourage laboratory directors to self-select in order to engage with. The purpose here was to change the way in which concerns usually ‘outsourced’ could be made actual and habitual through a different mode of thinking and activity.²⁸⁸ At the level of individuals, it was apparent that our concerns were met with at least some seriousness. For example, at the following year’s site visit, year 3, Julie Norville, a graduate student bio-engineer and representative of the Student Leadership Council of SynBERC made the following claim to an audience made up of SynBERC PIs and NSF auditors. Speaking of the growing Do-It-Yourself Biology movement, a self-styled biohacking scene, she said that it is important not to ignore what’s going on in the Do-it-Yourself biology scene and that outreach to the growing community was crucial, on the principle that “you have to try and be your brother’s keeper.” Whilst I would not want to overindulge what may have been a rhetorical flourish, this is an important distinction relative to the dominant jurisdictional mode of self-governance. One of the major limitations of self-governance as a mode of governing in our experience is that it produces not the flip side of responsibility, i.e. irresponsibility, but rather, negligence (failure to exercise care). Although we are not dealing with the same mode of jurisdiction, negligence is interesting to think with because establishing a claim under negligence law involves establishing that the defendant was under an obligation of care. Out a two-stage duty of care test, the first limb of this test specifies that claimants had to establish ‘a sufficient relationship of proximity or neighborhood’ in order to show that the defendant owed them a duty. As Richard Mullender a recent interlocutor of the Berkeley Human Practices lab wrote in response to a paper by Rabinow on practical judgments oriented to a conception of ‘flourishing’:

“The common law of negligence, as elaborated in England, provides an example of an institution in which three of the substantive concerns central to the Human Practices thrust of the SynBERC project figure prominently. These concerns are ... the logic of practical judgment, mutual flourishing, and capacity-building among those who participate in particular practices. ...bearing in mind that negligence law only requires its addressees to take steps to counter reasonably foreseeable dangers, we might reach the conclusion that this feature of the law presents us with a paradox. For how can a danger be both ‘unanticipatable’ and ‘reasonably foreseeable? There is, however, no paradox for some of negligence law’s addressees will (as they move through life) encounter novel sets of circumstances within which significant sources of danger should if they are reasonably attentive

²⁸⁸ Cf. Stavrianakis, Fearnley, Bennett, Rabinow *Human Practices Green Paper: Safety, Security, Preparedness: An Orientation to Biosecurity Today* Green Paper, 2011.

become apparent.”²⁸⁹

These biologists have an ethical outlook oriented to the goods of science which are ensured by the capacities to know and make biology. My point is to articulate how it was that we hoped to engage in a different ethical work with the biologists so as they could change their relation to the ecology in which this knowing and making operates.

In what follows, I will attempt to diagnose why the analysis of this problem and the exposition of the critical limitations and externalities of the existing governance techniques was unable to properly affect work in SynBERC. This is, if I may be permitted a medical analogy, a second step after attempting to treat the problem *in situ*. Since our effort at collaborative work on the basis of a diagnosis was not engaged with, all I can offer is another diagnosis which may clarify the stakes of a wider problem space in which this work and I think other projects in science and ethics exists.

Diagnosis: Justified.

The “chassis” work in SynBERC by 2009 was headed by Chris Anderson, an assistant professor of bioengineering at Berkeley whom I had first gotten to know when he was a post-doc at Berkeley. By this point, a year or so after the security event at UCSF, I conducted another period of research in Berkeley SynBERC labs, and this time mostly with a graduate student working in the Anderson lab on the “Tumor Killing Bacteria” (TKB) project. The tumor killing bacteria project has been made possible because of advances brought about by the Church and Anderson labs, among other developments in synthetic biology. The question of controlling effects in a milieu, however, is further away. In the case of the TKB application, the environments in which the bacteria is designed to exist are multiple; serum, tissue tumor, and intracellular environments. As Anderson suggested to me, “the immunology is the Achilles heel of this project.” What is important to note is that with a project like TKB in the beginning, as Anderson narrated it, they were not really thinking about the applications. As such environments had to be factored and worked back in to the engineering idea. The manner in which problems that circulate in a milieu can be worked back in to an engineering idea is what seems to be most problematic for synthetic biology and is metonymical for the wider security problem. Whilst the Church and Anderson labs recognize the need to factor technical safety problems back into an engineering idea, for instance to make sure their genomes are environmentally and humanly

²⁸⁹ Richard Mullender, “English Negligence Law as a Human Practice,” *Law and Literature* Vol. 21, No. 3 (Fall 2009): 321-353.

safe, it is unclear how some non-technical problems can be factored in, and then to think about what an appropriate response is.

Which Circle of the Venn Diagram?

We can generally say that the rationale of “safer genomes” is to fabricate genomes whose effects, when circulated, can be accounted for in advance. The security aspects not amenable to biological safeguards have a strange double status; on the one hand they are engaged with as a whole by the community as a serious problem; on the other hand the non-technical dimensions are externalized onto apparatuses and political technologies recognized as weak or insufficient. It is recognized that our current approach to biological security relies on limiting physical access to pathogens. Sequence information and synthesis technologies change this landscape. It is the case that there are political technologies as well as techniques currently in place and being developed, such as individuals who place orders for DNA synthesis identifying themselves, their home organization, and any relevant biosafety level information in addition to software which can screen orders of DNA. However, it is recognized across the community that they are not a total solution. So the question is, if regulation is necessary but insufficient and if technical safeguards are necessary but both hard and not a total solution, then what?

It is worth iterating an account of the synthetic biology community by a figure known to our research group and who advises the government on issues of security and biology. I do so in order to lay out a view of this intra-academic community from a related position within the field of biology. This account is important for two reasons: First this particular biologist can articulate a view of ‘us,’ the SynBERC community, taken as a whole relative to the wider field of the molecular sciences. Secondly, it was in part relative to this particular biologist’s account of the security problem within which this work operates that we took on the stakes of the problem of security.

Roger Brent was Director of the Molecular Sciences Institute in Berkeley and is now at the Fred Hutchison Cancer Research Center where in addition to his work in cell signaling he has set up the Center for Biological Futures. Discussions with Brent have been formative of the stakes of the security problem as a voice other than those, such as Dr. Race, who stress the ecological concerns. Like many in the synthetic biology community, Brent does not think regulation is per se inadequate, given the right conditions. In his view, the regulatory rules would be adequate “if they could be extended over the planet to all people who are ‘hacking’ DNA, and if there could be a more uniform regime covering release of the

engineered organisms in the environment and in people.”²⁹⁰ It is a quality of rules that they cannot determine actions.²⁹¹ Very explicitly, seriously, and honestly, one of Brent’s concerns is that the world is the kind of world in which people use weapons. These technologies make it more rather than less possible to weaponize biology. Having spoken with him several times over our engagement in SynBERC, let me re–describe the situation as he has described it and written about it:

Making pathogens drug resistant, and recovering live viruses from transfected recombinant DNA, are both technically feasible and have been so for a long time. There are people who can perform these constructions. As a descriptive statement, and not a normative judgment, there are people who are motivated to build and release a self-replicating organism. For Brent it is a real threat that some people may have the aim to use such technologies in a warfare capacity. If we take intention off the table then our two circles in a Venn diagram, those who can build things and those who want those things they build to function for the purpose that they built them, will overlap. This would look different, I imagine, to the Venn diagram described by Dr. Brent of “people with bad intentions” and “people who know how to build these biological constructs.”²⁹² Regardless of intentions, some bad things can happen. What is interesting is that he has a third postulate after the fact that we know how to re–build pathogenic viruses and that there is a set of people motivated to use this knowledge for ill: namely that “synthetic biologists have ghettoized themselves.” This third postulate is very important and marks a distinct shift in the vocabulary around self-governance within molecular biology since the 1990s. As he suggests,

“Inside the ghetto everything is good.”²⁹³

Brent, in actual fact, is pointing out two key elements: one, the exorcism of the problem of evil; secondly, he indicates by this, precisely the mistake of searching for intentions, as opposed to preparing for the events that may happen regardless of the motivations or otherwise behind the event. The point is sincere. It is not that he is second–guessing the intentions of those within this self-styled ghetto (as neither was our Human Practices group). As he suggests, applications within the ghetto pose “no risks.” Echoing comments from Dr. Race, he writes,

²⁹⁰ Personal Communication

²⁹¹ Ludwig Wittgenstein, *Philosophical Investigations*, §202, trans., GEM Anscombe, Peter Michael Stephan Hacker, Joachim Schulte, eds., Hacker and Schulte (Oxford: Blackwell, 2009.)

²⁹² On this point, see the very powerful analytic developed by Rabinow and Bennett; “good intentions, or what are claimed to be good intentions, in certain situations and under certain conditions, can operate in a mode that is nefarious ... In situations of danger, good intentions will frequently establish a structural blindness to possible ramifications of actions,” Rabinow and Bennett, 2012, 128.

²⁹³ Roger Brent, “Power and Responsibility,” <http://lifeboat.com/ex/great.power.great.responsibility> last accessed 4/17/11

“there is no reason anyone should fear a minimal Mycoplasma genome, or a bug that makes plastic, or methane, or artemisinin.”²⁹⁴ The point then is to ask what the relation is between things going on in this community, other communities making things and knowing things and the world in which this making and knowing operates.

Even if it is not reasonable to fear a minimal mycoplasma genome, Brent gives the example that it is reasonable to fear drug resistant anthrax; quite reasonable. It is this question of what is reasonable to be concerned about that is the moment that there is a change in the form in which statements are able to count as significant. Increasing capacities have made DNA constructs easier to engineer. The synthetic biology community has been proactive within the boundaries of their community; however, this is basically meaningless when these particular activities are not recognized relative to wider environments. The forms of their statements, “trust us,” are inappropriate for the situation. Recognition of the real situation requires a change in the form of statements and form of thinking about the future, as Brent sarcastically observes:

“Asking for help in screening long double stranded pieces of chemically synthesized DNA to see they don't encode pathogens? Look at how responsible we ghetto members are! The fact that this screen won't apply to shorter, single stranded synthetic DNA, the fact that ligation in vitro, PCR and serial recombination in yeast and E. coli all provide perfectly good alternative ways to make any DNA construction? Not our problem! We synthetic biologists only police our ghetto -- and we reserve the right to move the string that defines the boundary whenever we like. Even though nobody else even understands the string, or insofar as they do understand it, takes the string with any seriousness.”²⁹⁵

In Brent's view, acknowledging the relation of scientific sub-disciplines with wider ecologies has several concomitant obligations. The first is an obligation to invent agile defenses, to harness new technologies, in order to outpace what others will invent. This was basically accepted early on as a legitimate point within SynBERC, but it was not taken up as a part of something they needed to work on, nor something were we committed to within the Human Practices thrust.

The second obligation is truthfulness. This second obligation has been less present in SynBERC and was something we in Human Practices were committed to. There is a scene, one which I have experienced many times while discussing these issues in the Center; the scene consists in asking the question as to whether

²⁹⁴ *Ibid.*

²⁹⁵ *Ibid.*

there are a series of dangers to which one should be attentive. The response, in the recurring scene, is the claim that designing novel pathogens is much more difficult to do in reality than it might seem like to a naïve observer. Such assertions are both not true, since it is already feasible, and dangerous to assert since cost and time are diminishing exponentially, this being one of the fundamental aims of synthetic biology.²⁹⁶ This seems to be the neglected ‘other’ side of the confidence with which the possibility of designing, making, and testing ‘good’ organisms is asserted.

The disconnect between acknowledging a changing technical and political landscape whose horizon is unknown and an assurance that there are the technical means to control the ramifications was later exemplified in conversation with Anderson later in 2009. I had asked to have a meeting, and usual he was very welcoming. We sat and talked about orchids and the prospects for moving on from prokaryotes to organisms such as the *trichoplax*, a very basic eukaryote. We discussed how engineers can develop the morphological control that natural systems exhibit. Gesturing to the orchid Anderson elaborated his point:

“You look at a jellyfish, it has crazy morphology control, or the orchids, the structure control, or even the size control, when that thing pops out (*gesturing to a flower*), they are *exactly* the same; the same size the same shape, having that level of control, there is no multicellular aspect of prokaryotic stuff that’s like that. But prokaryotes are what we’re able to do. I’ve spent a lot of time thinking about what it is, beyond the existing technologies that we can do with prokaryotes. Really, doing DNA and cancer are the only two things I’ve found ... and biosensor kinda things, those are viable too.”

The question of limits then is a question of the technical capability to make morphological and functional changes in biological systems. I asked Anderson at this point, how he sees this work of biological morphology and physiology in relation to the various political ecologies in which these systems function:

“The security thing I think is fairly easy to deal with, you do the whole biosafety thing, we don’t distribute the Bio Safety Level 2 parts (*moderate potential hazard*), that stuff is fairly easy to deal with (*pause*) as long as the world stays where it with respect to synthesis. Of the things we do, the dangerous thing is the synthesis project. Because it

²⁹⁶ Jeffery K. Taubenberger, Ann H. Reid, Amy E. Krafft, Karen E. Bijwaard, Thomas G. Fanning. “Initial Genetic Characterization of the 1918 ‘Spanish’ Influenza Virus,” *Science* Vol. 275, No. 5307 (1997): 1793-1796. “Lessons Learned from Reconstructing the 1918 Influenza Pandemic” *The Journal of Infectious Disease* 1 November 2006: S127-S132.

actually makes it affordable to do your own gene synthesis...”

The discordance is patent and real. It is a problem not only for Anderson but for all those who have obligations relative to the unknown outsiders to invention. What is in these engineers’ control, however, is the manner in which they approach this discordance. This must be taken up not in individual–psychological terms but rather as a sociological or anthropological fact reflecting a manner of approaching the world.

Dominant Values, Residual Norms.

By 2010, several things had changed: The NSF had asked Rabinow to step down as leader of Thrust 4, although we were still included as a formal part of the Center. I received an email a few weeks after the fourth site visit in 2010, indicating that the NSF had delivered a very negative response to the thrust, that Rabinow was to be removed as its head and the name changed. What made the situation especially strange was that the logic for the removal was that Thrust 4 had paid insufficient attention to “biorisks.” There had been a division of labor with the MIT side of Human Practices, led by political scientist Kenneth Oye, in which his group would work with the Church lab on re-presenting the lab’s designs for safer genomes in terms of ‘demonstration,’ which in their language was conceived not just as validation (for which the scientific lab would be responsible) but *assurance* of validation. One may have thought that if risk assessment was what was missing then then onus should have been on the MIT side of the Human Practices work.

However, from early on, we at Berkeley had publicly posed the question as to whether there is utility in such risk assessments. It had been a source of disagreement throughout the engagement. Risk assessments may be worthwhile when the question is properly delimited relative to a known measure of risk. If we recall the conversation between Race and Church and the reflections of Brent, the capability to run meaningful ecological and political risk assessments on technologies which are dramatically opening the parametric space of experimentation is far from clear. Even if they were possible, there would still be a need for the engineers and scientists to engage in active reflection on these political and ecological changes produced by their practice. Our concerted effort to diagnose a genuine urgent and emergent problem, requiring collaboration, was rejected in favor of such exercises in demonstration and assurance, which would re-enforce an old division of labor between technical prowess and public relations. Furthermore, the NSF wanted a new head appointed with a month. Six months later no new head had been appointed. It was simply not a priority for the Center management. Drew Endy, formerly head of the “devices” thrust, and global mascot of the synthetic biology brand, was made head of Thrust 4 along with a recent

bioengineering Ph.D Megan Palmer, as deputy director.

Confidence and Commercialization.

Back in Emeryville for the 5th year site visit, the now re-named “Practices” thrust was ready to give its presentation. Endy, cool in his sober, practical tone, began by outlining the new vision for the thrust: “benefit everyone and the planet, develop responsible practice.” What’s not to like? It is clear that today, governments, industries, universities and a range of other interested parties are entrusting scientists to bring about a hoped for future dedicated to better health and commercial value. Endy made the serious point of locating the demands put on Thrust 4 within a context of increasing capabilities, resource demands (both intellectual and monetary) and expectations: Comparing the funding stream of Thrust 4 relative to the other thrust, he outlined that if Thrust 4 is supposed to do productive work on the biosecurity problem it will take more than \$100,000 without any cross-thrust collaboration; a situation we knew well. It is a genuine question whether the ‘price to be paid’ for entrusting this task of bringing about a hoped for future to these scientists is that they cannot put their assurance relative to this task in question.

The new thrust leader went on to name several problem areas that the thrust would work on; the first was significant. He named the fact that SynBERC is a venue cultivating a new generation of biologists, “many of whom were not alive in 1975.” This was a reference to the Asilomar conference that took place that year, an event dedicated to deliberation among those cutting edge researchers in recombinant DNA on the question of biological safety. “These researchers haven’t inherited the safety framework and some don’t come from a molecular biology background.” The second problem area spoke directly to the concern named in the discussion between Church and Race in 2008; how to take up the concerns regarding safety and security when looking at the use of organisms outside of contained environments. An event had been held in Washington a few months before and several of the MIT Human Practices researchers were present. Endy asked one of the researchers Gautham Mukunda, what he had learned from the event. Mukunda responded candidly; “I learned of a tension involved in a conflict of interest.” Mukunda had gone to the event both as Human Practices researcher on the safety and security dimensions of synthetic biology, and as CEO of a biosensor company, Lumin Sensors, which had been established as a spin-off of the Edinburgh University’s entry into the previous year’s annual iGEM competition.

He informed us that speaking at the event, as CEO, he believed in the inherent safety of the device and that once out of that role, he was more skeptical. Anderson responded from the audience to this observation. He asked Mukunda

what had changed to make him ‘skeptical’; “Was it that you realized it would take a lot to re-assure people, or was there something surprising and new?” Before Mukunda responded, Anderson continued, “I’d say there was no risk, other than the (biosensor) system being in *e. coli* which you have to then show people is safe, which is enough, or else,” a pause indicating his disbelief, “was there some other problem?”

Mukunda mirrored the rhetorical parry in three point fashion: Yes, the system is safe. As he put it in his charismatic style, guffawing as he said it, “I’ve offered to publicly lick the petri dish.” Secondly, what he observed about himself was the following; “I was resistant to questioning that assumption of safety, since we were so invested in it working.” The concern, which seems warranted to me, is that extrapolating from *his* experience to *other* people’s experiences, he wondered whether people with “other” systems, which are “not so surely safe,” may be inclined *not* to put their systems in question. Thirdly, he suggested, “a simple assurance is not enough, you have to convince people.” The first and third concern can be dealt with through public relations exercises and when possible and meaningful, risk assessment. The second point is precisely the kind of ‘other’ way of taking up the problem of activities to which a division of labor is inadequate. Nothing more was said.

Residual and Dominant Elements.

The affective elements which emerged from the Asilomar conference on recombinant DNA may do some work to help explain the affect and resistance encountered in thinking about this second point. Put simply, the outcome of the Asilomar episodes was a generic assurance that things once considered serious are no longer worth taking seriously, even if they need to be occasionally given lip service in order to indicate to a wider set of publics that their (unfounded) concerns are being engaged with. This former seriousness was exemplified by an unnamed conference participant at the conference in 1975 quoted in a Rolling Stone report;

“Here we are, sitting in a chapel, next to the ocean, huddled around a forbidden tree, trying to create some new commandments – and there’s no goddam Moses in sight.”²⁹⁷

The outcome of “Asilomar” is that today it is short-hand in scientific and policy circles for an event which secured authority for those developing molecular biological technologies under conditions of moral equivalence. Steven Shapin has shown how 20th Century America was the core setting for a commonplace which emerged from a late 19th Century question; “why ever should we expect that

²⁹⁷ M Rogers, “The Pandora’s Box Congress,” *Rolling Stone*, 1975, 39.

excellence of mind is necessarily accompanied by excellence of morals?”²⁹⁸ In Shapin’s account of the transformation of science as a vocation to science as job, he focuses on how moral equivalence was generated through an acknowledgement of economic motivation in the practice of science. The fact, he suggests, that today the scientist is “anybody” is “a reminder on the early modern period when the moral superiority of those who spoke Truth about Nature was itself a cultural commonplace.”²⁹⁹

There were three bases for this moral superiority; the referent object, the method securing knowledge of this object and the characterological dimension of the one who pursued this object through this method. In the production of specific technical knowledge the scientist in the 20th Century became a specific intellectual. Specialization and division of labor “caused technical workers to be indoctrinated with an ethical sense of limited responsibilities” and were remunerated at market rates for their specialization.³⁰⁰ The 1975 Asilomar conference on recombinant DNA was an event which exercised these limited responsibilities and assisted in the constitution of an ethical subject position; one from which to exercise powerful methods which may be capable of solving very real human problems. The character capable of enacting these methods on this biological object and problem has become specialized in a range of topics deemed necessary. One significant change in the 1980s was the addition of MBA and JD classes to Ph.D specialization of many biologists to survive in an ecological change perturbed by the Baye-Dohl act of 1980 and the emergence of the biotechnology industry thanks to technologies such as PCR.

What is interesting relative to the transformation that Shapin narrates are the three phases that historian Susan S Wright has identified in the period 1972-1982 in which ‘concern’ over recombinant DNA technologies were expressed, a technical problem named and solution formulated. Briefly stated, a serious problem, requiring a disposition to take it seriously and a mode of engagement not reducible to method was transformed into the opposite of such a problem, disposition and mode.

Wright identifies the year 1974-1975 as the moment in which a shift took place. The move was from a situation in which concerns were real, with no single narrative about the problem and how to engage with it, to one in which a single narrative and problem-solution pair was established. The moratorium on recombinant DNA work, signaled in the 1974 “Berg Letter” to the influential

²⁹⁸ Shapin, *Scientific Life*, 54

²⁹⁹ *Ibid.*, 23.

³⁰⁰ *Ibid.*

journal *Science*, was a preemptive move in order to effect a transformation. In Wright's reading,

“The international conference held at Asilomar, California in 1975 was organized to cement the reductionist discourse that bore within it the seeds of a technical solution, and it largely succeeded in doing so. The few dissenting voices that struggled to express concerns about possible social uses of genetic engineering or the implications of dangerous research were silenced or found those concerns transformed into elements of the technical agenda.”³⁰¹

A second phase involved a public relations offensive. The “epidemic pathogen” argument originated at the 1976 Bethesda meeting as a response to growing public criticism of National Institutes of Health policy. As one senior molecular biologist was quoted as saying, “In terms of Public Relations you have to hit epidemics, because that is what people are afraid of and if we can make a strong argument and make it stick, then a lot of this public thing will go away.” The argument basically held that the ‘worst case scenario’ was something that was so unlikely as to be almost impossible. Indeed as Wright suggests,

“Since the sole risk assessment experiment designed to test the hazards of cloning viral DNA, the Rowe-Martin polyoma experiment, was a year away from yielding results, these conclusions were surprisingly emphatic. The scientific community's response to earlier fears about the cloning of viral DNA had essentially come to be an attempt to tell the public they had nothing to fear.”³⁰²

A third phase of de-regulation emerged in tandem with the Baye-Dohl act, in 1980, as political institutions in the US led the way in accepting the now dogmatic scientific consensus on what the problem was—containment—and the established fact, that the products of such technologies could be controlled. The field was made business friendly by reducing the high safety hurdles and caution over types of experiment that had been set out six years earlier in the, by then dismissed, Berg Letter.

In 1973 there was much that was not known about the production of recombinant DNA, a form of DNA that does not exist naturally, but is created by combining DNA sequences that would not normally occur together. What was clear was that the knowledge produced by molecular biologists Nathans, Arber and Smith on restriction endonucleases had opened a plane of work leading to the work

³⁰¹ Susan S. Wright, “Molecular Politics in a Global Economy,” *Politics and the Life Sciences* Vol. 15, No. 2 (Sep., 1996).

³⁰² Wright, *Molecular Politics*, 242.

of those at the Stanford department of biochemistry between 1969 and 1974. Paul Berg was the department head at the time and the leading voice of those pushing both technical capabilities and voicing concern about the unknown dimensions of such research. Between 1973 and 1978 there were annual meetings which gradually eroded the initial genuine concern and gradually transformed these meetings into questions of self-presentation and public relations. In 1997 Berg reflected both on this shift and the various reasons for it. In 1973, out of the Gordon Conference on Nucleic Acid Research which broached these issues, James Watson had banned the use of feline leukemia virus and cats at Cold Springs. According to Berg he was concerned over human infection. Watson was at the MIT meeting that came out with “the Berg Letter” in 1974, the letter which proposed a general moratorium on two types of experiment until more was known: “He [Watson] was absolutely supportive insisting that we had a responsibility to warn the general public and scientists about the dangers of cloning.” Berg implies in his 1997 account of the events that with the knowledge of how much a P3 containment facility would cost to build, Watson reneged on his initial concerns. Berg says that Watson would never have admitted there was “an economic component,” and so the reason given by Watson was that they had been overly cautious in the Berg letter. Berg reflected further:

“I think if you reflect, as we all did later, on the basis for this concern, it was all hypothetical. There was not strong reason to think what we were doing would be dangerous. It sounded dangerous. We wondered what might be the consequences if you put genes that confer resistance to antibiotics into bacteria that infect man; you’d prevent the use of antibiotics that cured whatever that bacteria caused. That sounds pretty worrisome, and we suggested that such experiments should not be done. Second, putting genes that specify toxins into bacteria that could inhabit man should also not be done. But then, when you come down to the rest of it, it was pure hypothesis. We could imagine you might inadvertently pick up oncogenes from mammalian DNA, incorporate them into plasmids and put them into bacteria. Well, so what? As it later turned out, even if you do, it doesn’t make any difference. It turns out it is safer to work with oncogenes that way than it is to try to work with the viruses which carry them...So, as I look back on the period, even though we were wrong – wrong is probably not the right word; certainly the potential risk was incorrect – by calling attention to it, I think the whole thing was better off in the long term. The science that has come out of it has just been absolutely mind boggling. And so that’s

what in the end will justify it.”³⁰³

Indeed, these justifications are often used and with it an affective confidence that means putting such confidence in question, is both un-thinkable, and that this confidence blocks thinking.

The difficulty of a collective institutional engagement with the kind of unforeseen danger we were postulating as a basis of collaborative work, was, I think, for two reasons: first because such reflection and work creates a more, rather than less, complicated set of relations over which the engineers and we would have had to interact. Second, because there would have had to be changes to how the scientific practice was conceived in its ethical and temporal orientation; what life is this contributing to? To make this a genuine question is no doubt de-stabilizing.

³⁰³ Paul Berg, *A Stanford Professor's Career in Biochemistry, Science, Politics, and the Biotechnology Industry*, Interviews Conducted by Sally Smith Hughes,, Oral History Office, Bancroft Library, 1997, 77.

Chapter Six

Method: STIR

“Error is eliminated not by the blunt force of a truth that would gradually emerge from the shadows but by the formation of a new way of truth-telling.”

–Michel Foucault ³⁰⁴

Between May and December 2009 I was hosted by the Panke lab of the Department of Biosystems Science and Engineering (D-BSSE) of the Swiss Federal Institute of Technology, Zurich (ETH-Z). It is a bioprocess laboratory, which at the time of my stay was organized around three research clusters: High-Throughput Screening, “Synthetic Biology” and Integrated Processes. It was because of the lab’s engagement with the concepts of synthetic biology that I contacted Professor Sven Panke in order to see if he would accept my proposition to conduct a STIR study in his lab, which I proposed as part of a longer period of participant-observation. ³⁰⁵

On arriving at the BSSE I was put under the care of Sven Dietz, although he was away for the first days at a bioinformatics course in Iceland. In the meantime Sonja Billerbeck had volunteered to participate in my study. She approached me soon after I arrived at the lab, introduced herself and informed me that she would be very interested to participate in the STIR study, and furthermore she knew what I was up to since she had read Bruno Latour. I was hopeful that I would be able to find at least two others, which I did in the guise of Matthias Bujara a PhD student close to finishing his thesis and Giovanni Medaglia a post-doc returning to the Panke lab to finish downstream process refinement for antibiotic production; neither had read Bruno Latour. The methodology prescribed that there be a distinction made between ‘high’ and ‘low’ interaction participants, with the low or

³⁰⁴ Michel Foucault, “Life: Experience and Science,” in *Aesthetics, Method, and Epistemology: Essential Works of Foucault 1954-1984 Volume 2*, ed. James D. Faubion, trans. Robert Hurley and others (New York: The New Press, 1998), 471.

³⁰⁵ Although part of the ETH Zurich, the D-BSSE is in Basel, a city 80 km to the west of Zurich, bordering France and Germany. The reasons for this are straightforward; between 2002 and 2003 ETH Zurich entered into negotiations with the University of Basel, the Federal State Secretariat for Science and Education, the government of the Kanton Basel-Stadt and the two main pharmaceutical players and philanthropists Hoffman–La Roche and Novartis, to set up a new initiative in “Systems Biology.” The ETH benefited from these negotiations through the development of the BSSE and the Department was officially opened two months before I arrived.

no interaction participants acting as quasi-controls relative to any changes in thinking documented in the high interaction participants. In practice this broke down quickly. I spent much time inside and outside of work hours talking with lab members, particularly Sven, as well as two others, however, I rarely, if ever, did any formal ‘protocol’ work with them. With Sonja and Matthias on the other hand, whilst they became my regular cooperators with regards to sitting down and using the protocol to present what they were doing in the lab, the semi-structured interviews constituted the majority of the time we spent talking with one another. The one who came closest to a quasi-control was Giovanni, although our interaction functioned as something other than that. He and I only met formally to do protocol work once, at his request, since he had a particular concern which he wanted to present to me (narrated below).

In order to have a baseline against which to assess the efficacy of midstream modulation activity—and following Fisher’s pilot study—‘pre-protocol’ interviews were conducted with a number of people in the laboratory in order to get a sense from them as to what, if anything, “they considered to be the social, political and ethical dimensions which function to constrain, enable, guide or render problematic their particular form of science.” The aim was to see whether, by the end of the study, changes could be characterized.

How to characterize the object of any such change was an open question: dispositions, attitudes, practices, opinions, values? A range of terms present themselves and in our capacity as scouts in the context of this work, we were to be guided more by our own backgrounds and training, than by principles of method.

Our initial conversations in Basel quickly turned to the vocational stakes of science. My colleagues posed the question back to me as to what exactly I was doing in their lab. The first time I met Sven he pressed me on this; I began to explain the idea behind the STIR project and he interrupted me with a question: “Did you see that paper in *Nature* or *Science* about the glowing pig [he was referring to the work of Randy Prather on transgenically cloned pigs]? They got the pig to express to GFP [Green Fluorescent Protein]. Why did they do it?” I responded with a question as to what he thought the scientific significance of it was. “No, no,” he insisted, “in terms of your expertise, tell me why they did it.” I stumbled a little, slightly taken aback, and suggested it was probably curiosity but that I would want to know what kind of problem it was addressing and then what institutional affiliations the team maintained. He again cut me off announcing the answer; “They did it because they could; because it was fun!” I asked him frankly whether he thought there was in this case or in his work, a need for any further justification. His reply was immediate: “I don’t need any justification.” He took

out a cigarette, picked up his coffee and left the table. Recalling Fisher's orienting comments, this was a constrained space with autonomous actors.

The admixture of ambivalence and acceptance of the questions of significance, limits and justification came more frequently in less theatrical demonstrations from the rest of my interlocutors within a tightly circumscribed narrative: to my initial question as to what they thought were the social, political or ethical issues in their work, the reasoning was in all cases a variation of the following; yes there are ethical issues with regards areas like stem cells, and there should be limits, but not in my work, since I work with bacteria. The question of governance was a question of limits which need to be instantiated and regulated by law. The aim of the STIR project was for the protocol and engagement to perturb this constrained situation, by objectifying decisions in research such that latent values, considerations, and technical alternatives could be observed, discussed and possibly worked on.

Three Scenes.

In what follows I will narrate three scenes of the protocol: one-on-one conversations between anthropologist and biological researcher in which the researcher narrates what work has been happening in the recent past and might happen in the near future and the anthropologist asks himself how to use the protocol in order to open up such narrative so as to enlarge the decision space.

The first scene is of an attempt to use the protocol six weeks into my work in the lab, an effort which was idiosyncratic insofar as Fisher was present to observe it. I will then narrate two further scenes where a topic was posed for reflection by the biologist and which through the protocol could highlight the parameters that affect work in the laboratory. In these examples the protocol renders intelligible parameters through which work practices in the lab depend. What is curious is that of the 70 recorded and transcribed interviews using the STIR protocol in Basel, I found only two instances of a 'decision' that I was able to map with my interlocutor. This is not to deny that 'decisions' were made more frequently than that, but the manner in which one can raise such a process to the level of objectivation and reflection is not obvious. Of course, that is what the protocol was for. In my use of it, however, it was rare that I was able to find an opportunity to re-present the narrative, or interrupt so as to engage the narrative as one of a decision space with considerations and alternatives for how one could proceed.

Scene 1.

In 2009 Sonja was a second-year PhD student. After studying biochemistry, intellectual wanderlust in response to pedagogical specialization and repetition led

to a Master's degree in history, which in fact re-affirmed her interest in protein biochemistry; specifically in the folding kinetics of molecular chaperones (intracellular folding machines). At the time of my residence she worked with Mattias Bujara (biotechnology) and Christoph Hold (modeling) on the insulation and optimization of a metabolic reaction network for *dehydroxyacetone phosphate* (DHAP) built from the glycolysis pathway of *e. coli*. This work is broadly speaking metabolic engineering; the redirection of metabolic pathways using genetic manipulation. The team used modeling as well as biochemistry and molecular biology techniques to make a section of the glycolysis pathway topologically insulated from the rest of the cell and also from itself.³⁰⁶

The prehistory to their work was an EU project called, “a sweeter way to make saccarides.” The idea was to make a platform for unnatural sugars, “because unnatural sugars are interesting,” and they wanted to develop a new approach with DHAP as the intermediate. With DHAP as an intermediate, you can use different aldehydes as substrate to produce different sugars. Since it is an intermediate in glycolysis, the idea was to feed their system with cheap glucose and find ways to insulate and optimize the production of the intermediate. In order to optimize the glycolysis network, one of her first tasks was to detect what was interfering with the production of DHAP. She found that only two large molecular machines were interfering, the beta sub-unit of the ATP synthase and a chaperonin called GroEL. So, in 2009 when I arrived, she was engineering cleavage sites into the system, so that the interfering molecules could be switched off.

It is 10am on a Tuesday morning in July in the temporary building of the BSSE. The temporary location is set within the *Biopark Rosental* on the *Kleinbasel* side of the Rhein close to borders with France and Germany. The Department is sandwiched between the Swiss Telecom building and the *Biomedizin fakultaet* of

³⁰⁶ A word or two about glycolysis; the glycolytic pathway is used by all tissues for breaking down glucose to provide energy in the form ATP, or using ATP to perform intermediate steps. Overall the pathway converts glucose to pyruvate. The process occurs in two stages, the first is an energy investment stage and the second is energy producing. The team in Basel are interested in the first stage of glycolysis in so far as it is the stage in which glucose is converted into dihydroxyacetone phosphate. In the first phase phosphorylated forms of glucose and fructose are synthesized at the expense of ATP. There are five steps from glucose to DHAP. Glucose is committed to the metabolic pathway by being initially phosphorylated by hexokinase, because a phosphorylated sugar molecule does not easily penetrate cell membranes. this step turns glucose into glucose 6 phosphate, which is an aldose sugar. This sugar is isomerized into fructose 6 phosphate (a ketose sugar) with the aid of the enzyme phosphoglucose isomerase. This step is reversible and is neither regulated nor rate-limiting. Fructose 6 phosphate is then irreversibly phosphorylated by the enzyme phosphofructokinase 1 (PFK 1) in a rate limiting step. The step is also heavily regulated, the details of which I will skip (cf. Lippincott p. 90). The product, fructose 1,6-bisphosphate is cleaved by aldolase A into two products, glyceraldehydes 3-phosphate and dihydroxyacetone phosphate. The enzyme triose phosphate isomerase interconverts glyceraldehydes 3-phosphate and dihydroxyacetone phosphate, because the latter must be isomerized into the former for further metabolism. Of course, the laboratory are interested in accumulating dihydroxyacetone phosphate at the least cost of ATP.

the University of Basel and is adjacent to the biotech and chemical companies Syngenta and BASF.

I had previously asked Sonja why the ETH Zurich's newest department for biosystems science and engineering was opened in Basel? She told me that it's something she wonders about, every day in the train, travelling an hour from Zurich where she was living and where the lab used to be located; "why did we have to move to Basel? I don't get it so much, it was of course a political thing, ETH wanted ... last week we had an open house and the official start of D-BSSE, the department chair gave a talk about what is D-BSSE and why it is in Basel, I'm not sure it was a full answer, but he said ETH wanted to spread out and build metastases, [laughing], well, that was one point, to spread, and that was the first island they made, and Basel is a strong research place, and historically Friedrich Mischer saw a chromosome for the first time here, historical stuff .. intellectual stuff, but of course there must be money.. I'm not sure if *UniBasel* wanted us here.. or if pharma gave money... paying some money for the building or the department.. the intellectual reason was to have an island, but about the money I cannot say... this is even just a preliminary building, the real building is near the *Biozentrum* on the other side of Rhine, in seven years we will move again, this building is from the 70s but was completely redone for us, even though it is just temporary, it must have cost millions. It must have something to do with the president, this idea was planned already five years ago, but the former president didn't like the concept and it was stopped, and only under the new president was it realized."

The transition to Basel, however, seems to have been made smoothly, notwithstanding the lack of a lunch subsidy, lack of a library and the ever present question in any institution of how to foster an intellectual environment. Researchers here have very few economic constraints, and if a researcher is not on an EU or a Swiss National Science Foundation project with multiple stakeholders, work packages and annual meetings, then freedom of inquiry is constrained only by agreement with the PI. As a student paid directly by the ETH, Sonja said that "in my project I'm free. We have long term goals, to make these enzymatic pathways orthogonal, or insulated from the metabolism, but I have a lot of freedom to decide in what direction I want to develop my project."

Sonja and I were sitting, as we usually did in order to conduct these conversations, in the 'science lounge' on the seventh floor, in the open plan cafeteria with functional design and lots of chrome. The radio is on (as usual). The meeting is unusual insofar as Fisher is with me. He is visiting Basel as part of his duties as the governor of the STIR project. Although not decided in advance, I thought it would be a good idea to have him observe (an observer

((anthropologist)) observing another observer's (((bioscientist))) observations). Having returned from a research trip to a collaborator in Spain, I ask her to tell me what she is up to and whether she has any decisions, opportunities or blockages at that moment. Sonja narrated to me that she had begun the translation of DNA sequences from a library of mutants into amino acid sequences which could be compared with a homology model (i.e. a model which is well understood enough to act as a base against which to compare the mutated sequences). The purpose of this was to visualize how and where cleavage sites had been introduced. We join the conversation in media res, Sonja sitting across from me and with Fisher to her right, such that, I imagine, she can see him, but he is beyond my peripheral vision.

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SB: So at the moment I'm just sitting and waiting for them [mutants] to grow, so what I plan to do in-between is with the protein variants which have the cleavage sites introduced. I don't just want to stop at the point where I have them. I want to really see where the cleavage site jumped in. So I want to do a structural model of each variant I find, so I have to sequence each variant, to know the sequence, to know very exactly where the site introduced and translate the sequence into an amino acid sequence and then I can do a homology structural model.

Because proteins are folded, they have special structures but for the *e. coli* type of this protein we don't have [a map of] the structure as there is no crystal structure available, but we, well, by "we," I mean in the literature, there are structures which are homologues, or similar proteins, and what you can do is just taking the structure, a map that is encoded in a special file type, you can just put your amino acid sequence above it and just do a fit that is run by a software. And so I did that with my wild type [i.e. *e. coli* before she made her mutant variants] first, to get a good fit. The homology model is coming from rat liver mitochondria, so probably I am going to take that one. I am still searching in case there is a better one.

AS: Why is that a good one? [Looking down at the quadrants of the protocol, 1. Opportunity 2. Considerations 3. Alternatives 4. Outcomes; I begin to note 'choice of model,' under opportunity,

³⁰⁷ Note, the majority of researchers in BSSE are German (not Swiss) and the research language is English. Between themselves, German speakers speak German. When in mixed company either German or English was spoken. Scientific presentations are conducted (in the majority of cases) in English. The conversations that follow were in English.

thinking to myself this might be an ‘entry point’ for considerations and alternatives to ‘enlarge the decision space’ or ‘take more into account’].

SB: First of all it [the mitochondrial model] is solved in a high resolution, the picture is highly resolved, the people are really sure that it looks like that. Second, mitochondria, it might not seem that a rat liver mitochondrial protein would be similar to an *e. coli* one, but actually mitochondria, I don’t know if you know the endosymbiotic theory, so it shows that mitochondria and chloroplasts started out as bacteria, so it’s not so unlikely that they have a common ancestor. So it would be more unlikely to take one from yeast, because yeast developed in a different way, it didn’t do this endosymbiotic stuff, well maybe yes, I’m not too sure. But so it’s fine to use a mitochondrial protein. I did an alignment between the mitochondrial protein and my one [wild type *e. coli*] and I can see that there are over 80% identity, and even the ones that are not identical they are at least related. So what you do in an alignment is just find out which amino acids are identical and which are just related. So you would ask of ones which are similar, are they at least both hydrophobic? Or, are they at least both large?

AS: So, there is enough similarity between *e. coli* protein and the mitochondrial protein to say that there is a baseline match.

SB: Yeah, actually, what you can do is just try it, run the program, and if it does not fit at all then your energy is so high that you cannot accept it, if you have a reasonably low energy within your protein, then it is pressed into your structure, or your amino acid sequence is pressed into the structure, then you need to compare it with one which also has a reasonably low energy. So, what is done is, you calculate the amount of energy that gets free when you fold it, compared to the open state. Because in the open state hydrophobic residues interact with water and there is a lot of repulsion energy in how much energy you gain when you fold it. If you gain a lot of energy—there are cutoffs and thresholds and values you can compare with—it might be fine. I also made a course on that so it will be nice to try that out.

That’s what I’m going to do meanwhile, fiddle around with the software, to get a good homology model for the wild type, so I can use that one to just introduce the amino acids of the cleavage site, at the position they inserted.

And then I’ll take five functional ones and five non-functional ones just to see if there is a difference, or can we find something out

about it. Maybe it's totally random and you cannot say anything about it, but then at least you can say, we can't say anything about it. At least start with five samples and then maybe ten.

AS: And then with the model presumably the advantage is you have visualized the primary and secondary structures [referring to a theme which had become recurrent between us]?

SB: yeah, and again this is for the purpose of visualization.

AS: [the logic of why that model was being used was clear and I couldn't think of how to make the choice of model more problematic, so I shift tack in order to try and open up another topic on the theme of visualization which could provoke 'considerations' and 'alternatives'].

That was the point you were making yesterday when we were talking about Karel's gels [*both laughing*], and the fact that in Madrid, even something as simple as the light box that you use makes a difference to visualizing what's going on.

SB: yeah that was more because all these procedures depends on, you do the transposition and then you look at the DNA again, so what people always do in genetic engineering, the only way to find out if something worked is to cut it with certain restriction enzymes, and then look at the pattern. You have a lot of bands and with our shitty light box it's sometimes hard to see tiny bands, or the good resolution to see, so sometimes you have to do it three times and you're still not sure, so you say okay go to the next step and get a different confirmation. I was pretty surprised that everything worked so well from the first day on but I think it was just due to the fact that I could really see in a better way that it worked.

AS: Is that something that anyone here has considered, buying a new light box?

SB: Yeah, I talked to Andy, I'm not too sure, yeah, we already talked about it, especially because the light is just too energetically too strong, it just destroys your DNA, so the DNA you want to work with you cannot put under the light, so you just have to apply a tiny sample on the gel and look if its right and then you have to cut it out and that is really annoying. Well, whatever, we don't need to talk about this...

AS: [*Still hoping to find a 'decision' to latch on to*] But it does seem important since it's a fundamental piece of equipment, are there considerations for that?

SB: yeah, it's just a matter of discussing with the right person in charge of purchasing.

[realizing that there was either boredom or resistance in trying to pursue the issue of the 'shitty lightbox,' I dropped it]

Outside with Fisher we unpacked what happened. I began by suggesting that I was having trouble with the code of the protocol and how to categorize parts of the conversation. Erik began with a general observation:

EF: For conversation flow it is fine the way you did it. I would say you didn't do the protocol in that meeting, because, there wasn't a moment where you selected a single element, a moment in which you said 'pick that' and then use the model to push the other person to think through the alternatives and the considerations. I would find opportunities to do that more explicitly. Because then what you are doing is testing the protocol, how useful is it?

AS: She referred to a project, the protein recognition site work. She went to Madrid to learn a new technique for the introduction of recognition sites into a protein, so the protein can be cleaved in a specific way, and she wants to compare the molecular structure of the ones that do and don't accept recognition sites in order to find a profile for the kind of mutation, the kind of mutant, that accepts this recognition site. It's been hard to formulate this as a question of what the decision is that she is making.

Generally, for the last month, she tells me the goal she wants to get to. Her purpose is that she wants to make faster enzymes or proteins which accept a specific recognition site. Right now, she explains to me the strategy she is using, and her considerations for it, as an example from today, the use of the homology model. Given this goal, she explained the considerations and with regards alternatives, she says, 'well I'm trying this now and if it really doesn't work I'll have to rethink it and I'll think about it when I get to it.'

EF: Let me reformulate, you were using the protocol but in a light way. A heavier way would be to be in the middle of the midstream to try and open something up. Like you tried with the light box.

AS: Right, and it was clear that she had already gone through the considerations and alternatives, or else there simply wasn't anything there to open up.

EF: Which in some ways makes your job harder and also shows you here you have a different kind of scientist. So, I'm suggesting, more of an exercise of translation where you say "pause" slow the conversation down and map out the decision.

AS: Perhaps that is something I can do post facto when I give her the transcript / model of our conversation? [This is in fact something I tried, after the fact 'mapping' of our conversation through the protocol, which I would then give to SB. I know that she kept these models, they were visible at her desk space, but they did not provoke post-facto engagements on them.]

EF: Yes, I think that is a viable way forward and if you do that a couple of times that might enable you to pause during the middle of a conversation to whip out a protocol and to map it out. That's a way to start it.

AS: Right, the question is how to pay attention to when the protocol can be used. So far there has only really been one such chance to take out a protocol as I've been just talking with someone, with Sven in fact. He put his cells in the fridge and there were unfortunate 'social consequences.' His cells died and it turned out the fridge was a freezer. We tried to use the protocol immediately to map what happened.

Excerpt from a field note, July 9th, 2009

Sven is forlorn; a. sky blue t shirt foregrounds a sad look. His cells may have died and he is uncertain whether he will be able to reproduce his results, a basic element of science; to be able to publish a paper you need reproducible results. He tried to re-plate them but it looks like it did not work. The cause? Perhaps the temperature in the fridge; "I have some in minus 80 and some dirty ones in the other fridge but I cannot be sure it's the same strain." What to do? "Can we talk?" I asked, hoping he might want to do a protocol, "unless you'd rather leave it..." I followed up a little sheepishly, knowing that I had spotted an opportunity of my own. He concurred, "ahh, it's a good moment, because after, if it works, then I'll forget this moment."

The thermostat is not working in the fridge in his lab; "if you knew it already, it would not be a problem, you would use another one." He had been working "in the quiet lab." This led us to talk more about the consideration of the amount of tacit knowledge which depends on social relations, and specifically, talking. As he lamented molecular biology is a craft, "when a protocol says spin sample for 5 mins, in the other lab they spin for 15." The intuition that comes from

the craft is not documented. This led us to talk about a previous episode, before the cells died, where he was wondering whether his cells were growing and one of the other researchers, Angelique had asked, if he was using gloves. Sven replied that he wasn't. She asked why: "Well, Andy showed me how to do this and I put gloves on and then he asked me why I was putting gloves on, so I took the gloves off." She asked him why he took the gloves off, Sven replied "monkey see monkey do." The strain came from a lab in Paris; his alternatives? Train or plane.

EF: In that situation, trying the protocol didn't shed light.

AS: Well, at the least we objectified the fact there are material and social parameters and that they are related.

EF: I was going to suggest another technique is the hypothetical move, which can be something like: if I want to understand a moment as an instance where a practice could change then it is possible to map out the goal, the strategies, the conditions and to get the other to pause and think about some hypothetical situation in which the goal can be reached and how might we reformulate either the goal or the means.

AS: Right, and I tried that: for example, with Sven and the incident with the gloves and the fridge, I suggested, given these instances which indicated a communication problem and problem of tacit knowledge, how might the lab change habits? He responded to me with a very firm statement that "it wouldn't work" on the ground that these forms of interaction are tacit.

EF: right, maybe not to make a new rule, but to keep aware of how things are done.

Scene 2.

In the two scenes that follow, the categories of the protocol were able to be employed in a more explicit fashion. It should be noted that at every interview I conducted, a protocol sheet was used, but it was only in these two scenes that there was the capacity to re-present and map the narrative flow specifically as a 'decision' to be engaged.

In 2009, Matthias was in the final stages of his research, one of four members of the synthetic biology group of the Panke Lab working on the re-engineering of the enzyme network for the production of the valuable intermediate. He was working on an analytic method for finding out what was happening inside the group's cell free glycolysis reaction network.

It is the end of August, approximately three months in to my stay in the lab. We arranged to meet while he was waiting for an experiment to finish. I asked him how the work was going and if there was any particular question with regards to his research that he would like to talk about. He had recently had his “progress meeting” with the laboratory director and needed to think about what he should do with the remaining time in the Ph.D. Matthias’ adviser, the laboratory director Sven Panke, had a suggestion but ultimately he needed to consider what would be best. An opportunity in which a decision had to be made was imminent.

The alternatives to this opportunity, what to do with the remainder of his time were between investigating glycolytic oscillations in the system and working on synthesizing sugars. The considerations suggested were the facts that the oscillator would be a “high risk project” but that it would be also “really novel.” It seemed that this risk was operating as a parameter relative to whether good quality data could be produce in “a reasonable period of time,” whilst novelty functioned as a measure of the worth of the project. By contrast sugar synthesis “is applied,” “it is more interesting,” and he thought he could “learn more.” Interest and capacities seemed to operate as the factors relative to this possibility with the underlying idea being that “scaling up work” [i.e. from a research lab to an industrial lab scale] is an important purpose.

In our earlier conversations he had explained to me how it was important for him in coming to do a Ph.D. to ask himself what is possible in biotechnology. The desire to work with the Panke group came from the approach of the director. Instead of having to choose to do either small scale molecular biology, or large scale industrial processes, the group provided challenges that allow students to work learning different skills on different dimensions of multi-faceted problems. As a bioprocess laboratory the group is oriented to the idea that industrial production is a worthwhile goal to move towards.

As we spoke, these parameters resurfaced and three in particular emerged as crucial: capacities, the temporality of research and then the purpose of work. An industrial approach is important to him in terms of the purpose of science and his self-narrative of a life pursued as a biotechnologist. Matthias reflected;

“why spend time on something if what you can learn from it or what you can show with it is not that meaningful? So if you have something you are convinced in and really think it is worth pursuing then time shouldn’t matter, but it is in relation to purpose and outcome. Is it bringing us closer to our goal? Or does it not?”

The decision to pursue the more applied project, sugar synthesis or the novel project, glycolytic oscillations, was affected as well by one particular dimension of what would be involved in the second path of the fork. He had been ambivalent

about trying to engineer the expression of a number of genes which affect the activity of the three important enzymes in their network. He narrated this as a part of the problem as to why traditional biotechnology failed and the manner in which techniques today are remediating problems in biotechnology; “The traditional idea is that you have one bottle neck in the reaction pathway and if you overcome that your outcome will be huge. For some rare cases that was possible, but most of the time it failed.” The concern was that a lot can go wrong without a clear cause, as anyone who has spent time doing molecular biology knows. The concern over time and outcome is a real one when put in the context of stories such as an old laboratory member who worked for over a year on trying to knock out a gene with no success. As he reflected on the major reason for not trying to engineer the expression levels of several genes, “it might last very long and nothing will come out of it.”

We discussed the elements that might affect the deliberation: “does it really take that much time to do it? Are there by now some standard techniques?” We discussed these questions again when we met later in the week, by which time he had found a kit which can be used to construct very simple operons—multiple genes controlled from a single regulatory point—without any fine tuning. He had spoken as well with his director who suggested that bacteria express their genes in operons so there is no reason why they should not be able to create it themselves. On that occasion, we discussed the fact that the Keasling laboratory with whom I had worked in the US has been developing techniques in this area and I reminded Matthias of an article they had published. The outcome of these conversations was that possibility of making the operon became “worth a try”; although in fact neither project was pursued because a more urgent problem for the collaborative work as a whole emerged.

In addition to laying out the parameters of work, the parameters indicated the indeterminacy of metrics and of the necessity of choosing among them in order to formulate a measure of work and worth.

Scene 3.

The synthetic biology group is funded by ETH resources, the pot of money allocated to each Professor automatically each year. Nonetheless, the lab director spends time applying for grants. Having worked for two years in industry, he has an appreciation for the impact of industrial biotechnology, whilst recognizing that the possibilities and boundaries of a company and an academic laboratory are different. As he put it;

“if you go in a very short time from basically nothing, via a couple of grams of a specific pharma intermediate to, at the end of the year, a couple

of tons coming out of the pilot plant, you work over, I don't know, 4 or 5 locations with 20 or 30 or 40 people on the project, that's a different sort of challenge ... I very much appreciate what successful companies do in the field and I would feel privileged if the research that we do here would at some point, not permanently, not all the time, but in intervals, fuel the activities of these companies."

The laboratory has in the past applied for funds through the Commission for Technology and Innovation, a federally funded body that funds research that laboratories do together with companies. The motivation to go through this route, for Panke, is that he wants to work on things in which expertise is simply not sufficient. He reflected on the fact that in Germany especially the outcome of many years of training and 'academic culture' is a delicate relationship between theoretical knowledge and wanting to apply the methods learned;

"this is tricky when it comes to connecting them, what can seem a somehow more prosaic thing, earning money with it ... I think that people simply lack the connection, also the motivation. So, if you go through a typical German degree, unless it's economics, then the one thing that you will never encounter is a monetary drive to do something. After all, this is Humboldt's idea of the University. Humboldt did not have the idea to make university a money generating institution, right, so the concept that the application of something is an important piece in your overall chain of value generation which in the end also keeps our society going, this is something that simply has to grow and it can best grow in a company. We can do our part here in making people realise that there is a second angle to what we do here, but we cannot really, or I don't want to, create this idea of money as all important. So I think then this is also why I see that Ph.D. students, I guess, are very easily motivated by things like working on antibiotics, because it's a very clear problem, it's not necessarily related to money, but it's related to saving lives."

One of the researchers with whom I had infrequent discussions had heard about the STIR exercises and was enthusiastic to tell me about his current work. He was a Ph.D. student in this group, working on producing a new production method for an antibiotic who returned for a post-doc to see whether he could improve the down-stream processing part with an eye to making an industrially attractive amount. As we spoke, I asked him about the next steps towards his goal, to bring a drug to market This became the object of deliberation with him, proceeding from this starting point. The considerations into such a challenge included the fact that "right now not many pharmaceutical companies invest in antibiotics, while future antibiotic resistance is both a looming health challenge,

and when it gets serious will promote investment.” Giovanni’s reflection was that to address this future possible resistance problem will need synergies and not just competition to find a good solution to the problem. Part of the challenge in biotechnology is the formation of synergies to overcome resistances, both biological and institutional. The alternatives he mapped out were to produce an amount of product necessary for clinical trials, look for partners from the pharmaceutical industry and interestingly to involve cosmetic companies as the drug has also as anti-acne function. Relative to goals of health and wealth and given the drug’s utility, the possible outcome of involving a cosmetic company was just as plausible as involving a pharmaceutical company given the considerations regarding the lack of investment.

Object: Socio-Technical Systems: Amelioration, Description, Intervention.

The use of the term ‘socio-technical’ within the Socio-Technical Integration Research project reflects a general use within STS and not the earlier specific use of the term by the Tavistock Institute of London in the 1950s. This earlier use is of interest relative to STIR’s aims to intervene in such ‘socio-technical’ systems. Members of the Tavistock Institute were practitioners in group psychological work who inquired into ‘socio-technical systems’ for the purpose of understanding and *ameliorating* the mental health of workers.³⁰⁸ The initial uses of the term in science and technology studies in the early 1980s, by contrast, were for purely *descriptive* purposes. For example as Michel Callon asked, one of the questions of the then nascent STS was, “how can we *describe* socially and materially heterogeneous systems in all their fragility and obduracy?”³⁰⁹ Taking the description of socially and materially heterogeneous systems as a starting point, a remediation of such description with interventionist aims, was attempted in Fisher’s thesis work, functioning as a proof of concept for the larger STIR project. The purpose of such intervention was an open question.

The aim of the STIR project was to develop a mode of social science which can study the possibility of integrating the “social” and “technical” elements of socio-technical systems, through social scientific intervention. In this sense it is certainly compatible with the ameliorative sense of the Tavistock group, but it must be emphasize that from the start of the project, Fisher stressed the procedural

³⁰⁸ Eric Trist & K. Bamforth, “Some Social and Psychological Consequences of the Longwall Method of Coal Getting,” *Human Relations* 4 (1951): 3-38, 7-9. Eric L. Trist, et al., *The Social Engagement of Social Science: A Tavistock Anthology : The Socio-Ecological Perspective* (Tavistock Anthology) (University of Pennsylvania, May 1997).

³⁰⁹ Michel Callon. (1980) “Struggles and Negotiations to Define What Is Problematic and What Is Not: The Sociology of Translation,” in *The Social Process of Scientific Investigation: Sociology of the Sciences Yearbook 4*, eds., K. D. Knorr, R. Krohn, and R. D. Whitley (Dordrecht and Boston: Reidel).

and formal character of the intervention, i.e. the project as a whole was constituted by a refusal to name ends aside from ones that emerged from within labs and unfolding of the research in individual studies. The first task for us as researchers in the project, as it was explained to us, was to attempt to perturb our interlocutors discourse and practice by observing practice, developing a shared description of the lab researcher's decisions in which the latent values, social considerations and alternative means are discussed and then giving these descriptions back to the researcher. The ends present in these scenes were knowledge, health and wealth; far from surprising within a biotechnological setting.

The challenge to both the social studies of science and the natural sciences in a project such as STIR is to test whether observations of practice and their discussion can "modulate" research practice. The pedagogical goal is for such observation and discussion to enable the natural scientist to take more into account when doing her work. Fisher's solution to the question of how to 'integrate' observation as an intervention, which I followed, was to 'feedback' observations of research to researchers. The scientific work and the observation of scientific work are thus treated *systemically*, which I would specify as of the cybernetic kind, i.e. they are a system understood as dynamic sets of elements whose relations between themselves—the intra-system elements—and between 'itself'—the system—and its environments are capable of being observed and decided on. For example, the 'department' of the *Eidgenössische Technische Hochschule* (Swiss Federal Institute of Technology - ETH) is made up of 'labs,' themselves made up of project teams, individual and interconnected Ph.D. and post-doc projects and researchers, committees, funding sources, lab hierarchies and divisions of labor, Mass-Spectrometers which produce different results in Basel and Zurich, Sequencing Machines which are too big to fit through the door such that the building managers have to destroy a wall and rebuild it, among other material, semiotic, semantic and pragmatic elements.

For Fisher, as for systems theorist Niklas Luhmann, such socio-technical systems of organized action are partly characterized by the decisions taken by those in the system. As a system, laboratories are structured so as to reduce an overly complex set of relationships to a limited number of decidable alternatives; which homology model to use? Which project to choose? Which industrial strategy to pursue? ³¹⁰ Furthermore, the introduction of a social-scientific experiential system (an anthropologist) into the target socio-technical system (the laboratory system relations) has the aim of the observation and feedback of information to

³¹⁰ Niklas Luhmann, "Reform und Information: Theoretische Überlegungen zur Reform der Verwaltung," in *Politische Planung. Aufsätze zur Soziologie von Politik und Verwaltung* (Opladen: Westdeutscher Verlag, 1971), 181.

such a system. In these cases however, relative to their own ends, I did not give the scientists a different kind of observation than they were making of themselves, through the anthropologist as medium.

Observation: Structure & Organization.

When conducting his thesis research on enacting US nanotech legislation calling for social science mechanisms to develop responsible innovation in nanotech labs, one of the key design principles for Fisher was to acknowledge that decision making is guided by the ‘structure’ of socio-technical systems. For Luhmann—and I use him simply because he has a clear if complicated vocabulary—systems are both structured and organized: ³¹¹ a structure is the combination of how institutions are arranged (their divisions of labor, material arrangements etc.), the parameters and metrics relative to which decisions are made and the habits and dispositions of individuals formed by the institution. *Structured systems* are “open” (i.e. not a closed system of semiotic oppositions; “raw”/“cooked”). Unlike Luhmannian systems theory however, those of us in the STIR project of integration had to assume that in addition to such structural openness there is the possibility of *organizational* openness. ³¹² The organization of a social system, for Luhmann in his systems theory, has as one of its requirements the closed differentiation of the system relative to its environment. This then was the problem probed by these STIR studies; if in addition to the possibility of reformulating the practices of scientific systems—e.g. given my goal to produce something useful and given the parameters of the political economy around antibiotic production, should I pursue links with a pharmaceutical company or a cosmetics company?—can such structured systems re-organize themselves (open themselves) relative to their changing environments and can the structural changes be calibrated to this organizational openness? (I.e. not close themselves off from their environments).

Furthermore, how can a judgment of such calibration, along a differential of better/worse, be made? Is there a position for the human sciences to engage as critical collaborators with the natural sciences in this topological space of systems and environments? As the lab director suggested, with regards antibiotics, the measure of worth is self-evident and cannot actually be discussed. To put in question the solution to a looming health crisis, qua standard of judgment for the worth of work, would be read as tantamount to moral ill-will. Are the only ends

³¹¹ I take the following point from James Faubion. More generally, Faubion’s work is an important conduit for me to be able to take up both human practices and stir if not in a common frame and least with respect to a common problematization of ethics. His elegant combination of Luhmannian systems distinctions, Foucaultian attention to the parameters of the ethical field and a patient anthropological mode of observation and description has assisted me in thinking through what is arguably a problematization.

³¹² Cf. Faubion, *Anthropology of Ethics*, 1-30.

relative to which judgments of the practice of science can be made health and ‘the chain of value generation’? What role can the human sciences have in participating with the natural sciences in posing the question of the contribution of the sciences to our contemporary forms of life?

Traditionally, Luhmann suggests, decisions about how to make decisions were the object of “structural planning.” The problem he diagnosed in his early work on the reform of post-war German administrative systems is that often systems are too complex and the future too unpredictable for goal-oriented [*zweckrational*] planning that anticipates a certain final state.³¹³ As a result, structural reforms imposed from outside of the system often turn out to be inefficient. What is needed instead, Luhmann suggests, is a self-reflexive decision-making process within the administrative system that is capable of modifying itself in response to unforeseen challenges. This is neatly in accord with one of the other design principles of STIR that outside mandates had failed to bring about the kind of intervention on the sciences that they had aimed at producing. As such what is required is to develop mechanisms for self-observation and modulation that leave autopoetically open the question of ends or goals. This openness then is an explicit refusal to put in question the ends or metrics of the system being engaged with. In this sense, one should call this kind of work technical criticism.³¹⁴

Objective? From Method Toward Problems.

There is a difference, however, between a methodologized observation for the purpose of technical criticism and observation for the purpose of narrating, analyzing and diagnosing the cultural significance of a phenomenon. It was the latter which seemed to me to be the precondition for a worthwhile collaboration between a bioengineer and an anthropologist, to be capable of providing observations of the scientific system. Technical criticism is what Max Weber understood as a practice of critique that provides “the acting person with the ability to weigh and compare the undesirable as over against the desirable consequences of his action.” Reflection on cultural significance by contrast, “can offer the person, who makes a choice, insight into the significance of the desired object. We can teach him,” Weber optimistically wrote, “to think in terms of the context and the meaning of the ends he desires, and among which he chooses.”³¹⁵

³¹³ Ibid., 184-186. Niklas Luhmann, "Reform des öffentlichen Dienstes. Zum Problem ihrer Probleme," in *Politische Planung. Aufsätze zur Soziologie von Politik und Verwaltung* (Opladen: Westdeutscher Verlag, 1971), 209.

³¹⁴ Max Weber, "Objectivity in the Social Sciences," in Weber, Max. *The Methodology of Social Sciences*. Translated and edited by Edward Shils and Henry Finch (Glencoe, Illinois: Free Press, 1949), 53.

³¹⁵ *Ibid.*

Within the field situation and through the use of the protocol, it was precisely an incapacity to bring together these two forms of observation that characterized this methodological response to the problem of a relation between science and ethics. If one takes ‘decisions’ as the object of reflection and intervention, unless the means for reflection on “the significance” of the object are developed, then such a method for integrating social concerns will be nothing more than a technical criticism.

As STIR researchers we were not representatives of another ‘function system,’ and so we were not bringing society, an identity, law or anything else ‘into’ the lab. As Fisher described our role, we functioned to reflect back what is already there in this scientific system, to assist in the operation of self-observation of scientists by scientists. My role, as I took it on, was of a social technician, who produced a mirror function rather than, say, a different window onto the system’s environments. The question for the human scientist becomes; *why do this?* Our justification in STIR was to modify behavior in some way: but relative to what purpose? And how could one make a judgment about this activity? My question was answered in formal terms: deliberation within ongoing research (“the midstream”) is the form and the capacity for it is the standard, relative to which the efficacy of the method was to be judged.

After months of protocol work in Basel, I reviewed our attempts at collaboration. Sonja narrated to me the following, that at the beginning of our interaction she thought ethics meant only the question of what is right to do. As she said, “in this case it means limits for what is good. But if ethics is also human interaction then it’s of course something different, opening a broad field. For example, collaboration, which is very very important. In that case the problem of collaboration is not a limitation but is bringing progress, or asking what can bring the most progress.” What we see here then is a communication having taken place. By the criterion of STIR this was a successful collaboration. But this communication also leaves the parameters and metrics of the scientific system qua system intact, whilst having made visible and having apparently produced reflection on how work practices, on the natural scientific side of the relation, can inflect the pursuit of knowledge. The closed differentiation of scientific system to the anthropological environmental ‘irritant’ left the scientific system unperturbed. The human scientist was a medium for self-observation, rather than an actor to be engaged with on questions of collaboration. The same the case with Giovanni; synergies are important, there is a looming health crisis and different industrial, scientific and governmental actors need to work together. What is lacking is a reciprocal use of the protocol which could ‘take into account’ the thought and concerns of the anthropologist—within the dialogical relation and not only in the

form of a PhD dissertation. Such a reciprocal mode would be highly challenging (if not impossible) to methodologize within the space of the scientific system as there would have to be a reciprocal subject position for the anthropologist.

The danger it seems to me is the reification of supposedly self-governing effects of such a communication. What I mean by this is that, just as with the first example I gave, there was no shared problem relative to which this communication could take on significance. As such it remains at the level of a technical communication: ethics is not only limits and includes the self-formative and governance relations internal to the pursuit of knowledge. A reason one might think this is a deficient situation is if one thought a different kind of relationship between anthropological, or human scientific inquiry and natural scientific inquiry could work on introducing a range of problems which are outside of the practice of science as it is currently structured and organized.

If there is not a consequentialist justification, on the basis of the extant ends of scientific work, for what purpose does one engage in anthropological inquiry into the ethics of scientific work? Otherwise said, what kind of problem has collaboration between cultural and natural scientists as a solution? One way to describe such possibly “common” problems that require collaboration would be to describe them as “collateral.” This image was presented to me by the laboratory director when describing interactions with the environmental activist group ETC, who are proponents of a moratorium on new forms of bioengineering such as synthetic biology. He had suggested that whilst representatives of this group were knowledgeable their arguments were made on “collateral grounds,” and so, by implication, could not be engaged seriously. This image is important. There are a range of issues, in bioengineering, as well other emerging and established sciences (bio, nano, nuclear), which are treated “collaterally” and appear outside of the bounds of scientific practice; these range from ecological to security concerns. To work on these issues collaboratively would require bringing into a different relation scientific knowledge, observations of the political context of science, the technologies being produced and the commitments of those producing these forms of knowledge.

I wish to refuse the occupation of a subject position of human scientists as the medium for the autopoiesis of the natural sciences, this autopoiesis taking form as either amelioration of research set ups or the communication of values rendered as opinion. This refusal poses a challenge of how the human sciences can be situated and in what mode, so as to enable the observation of the ethical challenges of the emerging sciences. When the goods of biology and the stakes of collaboration are framed solely in terms of extant ends within scientific systems, such framing produces an incapacity, by definition, to pose ethical questions

outside this rationality and this it seems to me is a deficiency in human science inquiry into the ethics of science.

My claim is not that amelioration and prosperity have no place as “reasons” within a scientific institution. Clearly two important “economies of worth” in the justification for the pursuit of new pathways in biological engineering involve industry and the capacity of these technologies to improve health and welfare.³¹⁶ The observation is rather of the critical limitation of human science engagement with the natural sciences within the bounds of a form of life largely constituted and bounded by amelioration and prosperity. The danger, it seems to me, is in a situation where the human and natural sciences are supposed to be collaborating on the problems (social, political and ethical) engendered by new sciences and technologies, such collaboration may in fact contribute to the production of an incapacity to hear and work on questions outside the parameters of the extant justifications; or, else foster a subject position from which a human scientist is rendered less capable of engaging with natural scientists on such problems.

In the light of this question, which I had attempted to articulate a number of times through our engagement, Fisher offered a reformulation of the diagnosis that had oriented the STIR project: in Colorado, where he did his PhD work, his diagnosis was one of goal-displacement due to the fact that the scientific urge to conduct curiosity driven inquiry requires resources; the means to conduct inquiry becomes the goal. Reflecting on his diagnosis of science and putting the lens back on STIR he asked a question which was a spectral presence during my fieldwork,

“so why then bother to do what we’ve been doing? Why collaborate? This is the tragedy; it may come back to this: that we’re simply doing the modern project better.”

³¹⁶ On economies of worth see Luc Boltanski, and Laurent Thévenot, *De la justification. Les économies de la grandeur* (Paris, Gallimard, 1991).

Chapter Seven

Comparative Metrics

“Evaluations, in essence, are not values but ways of being, modes of existence of those who judge and evaluate, serving as principles for the values on the basis of which they judge. This is why we always have the beliefs, feelings and thoughts that we deserve given our way of being or our style of life.”

–Gilles Deleuze,
*On Nietzsche*³¹⁷

The STIR and Human Practices projects were oriented by differing diagnoses of the problem of the relation of the human and natural sciences. By diagnoses, I mean two different ways of analyzing the problem I began this thesis with, so as to constitute it as the kind of problem which makes available a particular response. These analyses occurred by way of concepts capable of giving orientation to what the problem consists of; different diagnoses rely on different concepts, put to work in different responses to the problem. Simply put, Rabinow and Bennett’s diagnosis was of a problematization of science and ethics requiring *équipement* and Fisher’s diagnosis was of an autopoietic dissonance in the scientific system requiring a technology of auto-correction.

The Human Practices project aimed at producing *équipement* for collaboration oriented to pedagogy and events. The STIR project aimed to develop a technology for the modulation of research practice. How then to compare them? And comparison to what end?

As indicated in the Introduction, my aim in this thesis is not a general classification of kinds. With respect to the epigram of Deleuze, if there is an ontological line of thinking in thesis, then it is at the level of practices which constitute a mode of being. Any judgment produced by such comparison, to use Kant’s distinction, is not determinate but rather reflective.³¹⁸ This is to say that

³¹⁷ Gilles Deleuze, *Nietzsche and Philosophy* (New York: Continuum, 1986), 2.

³¹⁸ Immanuel Kant, *Critique of the power of judgment*, Cambridge: Cambridge University Press, 2000, “IV. Judgment as a Faculty by which Laws are prescribed a priori.: Judgment in general is the faculty of thinking the particular as contained under the universal. If the universal (the rule, principle, or law) is given, then the judgment which subsumes the particular under it is determinate. This is so even where such a judgment is transcendental and, as such, provides the conditions a priori in conformity with which alone subsumption under that universal can be

any determinations I am able to make from comparison are not determined by the concepts employed. Rather, the question at this juncture is how to interconnect what I have inferred from these two experiments, the concepts used to design them and the diagnosis of the problem indicated by such conceptualization. Such an interconnection of inference, experience, concepts and problems is what traditionally in philosophy is called a reflective judgment.³¹⁹ Or to put it another way, concepts are both responses to and constitutive of problems, which in the sciences are transformed into inquiry by experiment out of which inference can be made. The work of making a judgment mediates these relations of problem, concept, experiment, experience and inference.³²⁰

In order to produce a judgment I have picked out a particular concept, which was developed by Rabinow and Bennett in their diagnostic orientation to the problem of science and ethics; the concept of a “metric.” It may seem tardy to introduce the concept at this stage, but in fact it has been operational throughout and it is only now after having gone through the experience of these two modes of engagement that the concept is now available for display, so as to function as a point of comparison. I recognize that using this concept has the consequence that the mode of judgment is oriented by one of the sides in the comparison being judged. To put it plainly, efforts to produce *équipement* and technologies as responses to the problem of human science engagement with the natural sciences is judged foremost from an ‘equipmental’ point of view, since I privilege the concept of “metric,” which is closely connected to my discussion of measure in Chapter One. As will become clear, however, this ‘point of view’ will make available a mode of judgment, which considers the attempt to invent collaborative equipment also from the point of view which produced a technological response to the diagnosis of the problem.

Diagnostic of Equipment: Identification of Metrics.

Rabinow and Bennett produced a *Diagnostic of Equipmental Platforms* in 2007 to assist them in their orientation to ethical and anthropological inquiry in

effected. If, however, only the particular is given and the universal has to be found for it, then the judgment is simply reflective.”

³¹⁹ He also says in section 20, a reflective judgment is the “capacity for reflecting on a given representation . . . to produce a possible concept.” Or as Hannah Ginsborg puts it, “to bring particular objects under empirical concepts”, Hannah Ginsborg “Reflective Judgment and Taste”, *Noûs*, Vol. 24, No. 1, Mar., 1990, pp. 63-78 On the Bicentenary of Immanuel Kant’s Critique of Judgement, 64. Gilles Deleuze, *Kant’s Critical Philosophy: The Doctrine of the Faculties*, trans. Hugh Tomlinson and Barbara Habberjam (London: Athlone, 1984), 8-60.

³²⁰ The question of the objective validity of such judgments is an important one, however, since I am interested in making an anthropological and ethical judgment I consciously sidestep the metaphysical and epistemological problem of the objective validity of judgments. This sidestepping is thus also a stepping toward a form of anthropological inquiry which assumes responsibility for the animating work of thought in the field of experience.

synthetic biology. They were in accord that such inquiry should aim towards the invention of equipment adequate and appropriate to the ethical and anthropological problems to be specified through inquiry. As such, the aim of the diagnostic was to assist in the orientation toward the design and synthesis of equipment. They posited a series of categories for parsing out two entrenched and available ‘equipmental platforms’; a platform is a stable configuration of techniques and forms of knowledge for doing an activity; an equipmental platform functions as the basis for the organization of the activities of specific equipment.

The diagnostic distinguished between two kinds of series, *contemporary figures* and *equipmental figures*. These two kinds of series were a response to two questions: what is being problematized and how is equipment designed so as to intervene on what is being problematized? The diagnostic sought, in the first series (contemporary figures) to provide the categorical distinctions needed to address the question of what is being problematized and then, in the second series (equipmental figures) to provide categories so as to ask how the first series is re-worked for ‘pragmatic’ purposes within a problem?

“The categories in the diagnostic have been selected for their discriminatory power. Further, they provide heuristic utility, aiding the work of composing new equipment as well as orienting inquiry.”³²¹

A key structural joint in the diagnostic between what is being problematized and the techniques for intervening in the problem is “method.” A method, within Rabinow and Bennett’s diagnostic forges a relation between the object being intervened on and the manner in which it is intervened on. This relation is itself ordered by a metric. This is the hinge point through which I will compare STIR and Human Practices and is the hinge point that allows their reciprocal comparison.

Metrics.

Metric is one of four categories for asking, what is being problematized today? Throughout this thesis, the object being problematized was the practice of collaboration between the human and natural sciences. Rabinow and Bennett identified, in their diagnostic orientation to this problem, that the general question of what is being problematized, can be specified by a series; an object relation, a mode of ontology, a metric (which orders a relational field of practice), and a mode of veridiction. The object is the relation between things which has broken down or been somehow made problematic, requiring an intervention of some kind. The

³²¹ Paul Rabinow and Gaymon Bennett, *Diagnostic of Equipmental Platforms*, 2007
<http://anthropos-lab.net/wp/publications/2007/08/workingpaper9.pdf>

mode of ontology is how these relations exist as an object, the mode of veridiction is how you can say true things about such an object and the metric is the standard by which, within this series, “aspects of things are selected and coordinated as elements about which true and false speech acts are made and taken seriously.”³²²

To give an example, in public health practices, the figure which was problematized, is what Foucault called ‘biopower’; this figure is made up of a series including the problematic relation between an individual body and a population, a verificational way of saying true things about this relation, such as statistical analysis, the ontology of these relations are probabilistic (in keeping with the kind of relation it is and how true things are said about it) and furthermore, the metric, “how aspects of things are selected and coordinated” is one of normalization.³²³ This was a key figure in the development of practices associated with modern statehood.

In 2007 Rabinow and Bennett proposed a new figure relative to the question, what is being problematized today? With respect to the general activity of bringing new biological forms into existence and the ramifications of such bringing into the world, they proposed a term to designate a relation which might constitute an object being problematized today. With due nominalist caution, they proposed the term, “forms-pathways.” They proposed that the ontology of such an object is emergent. These two categorical determinations were the safest, referentially speaking, since what was needed was a set of orienting terms to index that which we wanted to work on; namely, the emerging objects from these scientific practices. More importantly, and the point I want to get to, is that the diagnostic’s function was to give some categorical orientation to that thing to which Human Practices as an activity, would be a response. As such, the activity of Human Practices, its *equipmental* figure, was partly constitutive of the *contemporary* figure being problematized. The work in Human Practices was oriented in design to an ethical end, named as flourishing. As the diagnostic work proceeded, it was logical that this ethical end be taken into account as part of that thing which was being problematized. Flourishing was proposed as the metric of the problematization.

Flourishing?

On May 18th 2009 the Human Practices group at Berkeley held a workshop in which Fisher participated along with colleagues from ASU, including the Center

³²² Paul Rabinow and Gaymon Bennett, *Diagnostic of Equipmental Platforms*, 2007
<http://anthropos-lab.net/wp/publications/2007/08/workingpaperno9.pdf>

³²³ See my note in Chapter One.

for Nanotechnology in Society director, David Guston. The workshop was oriented to the inter-relation of three topics which had seemed important to us in Human Practices, topics central to the problem of collaboration: governance, ethics and the knowledge produced by science, both natural and cultural.

In the opening remarks Rabinow oriented the workshop around four questions: whether there is a space of common problems and capacities for those inquiring into the sciences broadly conceived? How to do inquiry given this space of problems and capacities? Thirdly, he laid out a thematic starting point, that all participants are involved in ethics whether they think they are or not. Finally, this starting point allowed a question to be articulated as to how one can inquire into subject-formative practices of inquiry and production, in the light of questions of a 'good life,' and the problem of the governance of the self and others.

This trenchant, direct, simple and troubling question, in an implicit fashion, put the question of metrics on the table; given the problematic object, i.e. given these problematic relations of governance and knowledge between the human and natural sciences, what is the standard or measure by which one could coordinate and assess these relations?

Fisher and I spoke the day after the workshop in preparation for my trip to Switzerland and the conversation circled around topics from the workshop. The stakes that the Human Practices group was attempting to inject into this field of relations were clear for him; "We asked at the end [of the workshop], why are we doing this? Oriented towards what and whether we want to be doing this at all?"

Fisher had, since our first STIR workshop, in January 2009, given a number of responses to these questions and relative to them, he said that he realized that it may not have been visible what exactly the stakes were, for him, regarding the STIR project. On this question, he had equivocated between capacity building for natural scientists, for the human scientists or 'for the collaboration.' The equivocation mirrored an oft cited concern from Guston, Fisher and others at ASU as well as more broadly in the STIR project, with respect to the metric of flourishing: "Flourishing for whom?" Relative to which subject position is the criterion operated?

This question, whilst comprehensible, misses the important distinction between cooperation and collaboration, and misses how flourishing was posited as constituting a metric. If one were engaged in a collaborative scientific activity, on problems, whose manner of collaborating was oriented to a metric of flourishing, then the 'flourishing' of the collaboration would be judged relative to the capacity to pose and work on indeterminate problems relative to the scientific practices under scrutiny. Or in other words, to make a *practice* of thinking about such

problems—problems which highlight deficiencies and excesses and which would thus require ‘growth’ and ‘maturity’ to respond to them—internal to scientific practice broadly conceived.

In chapters 4, 5, and 6 show how a set of ethical questions, which to my mind were constitutive of taking questions of flourishing seriously, were externalized and thus were blocked from co-labor and blocked flourishing from functioning as that standard which could orient how knowledge and governance could be practiced with respect to these biological inventions. The mistake of the question, “flourishing for whom?” is to miss the purpose of collaboration. We thought that a collaboration in which anthropologists could participate in a serious way in the stakes of the problems being produced by the emergent biosciences, would be able to characterize significant problems of “the present.” Such problems would be ones on which human and biosciences could co-labor and which could then contribute to inserting a significant difference into this present.

The question from Guston and Fisher, is comprehensible, however, from within their own modality. If one acknowledges that all activities aim toward some good, but think that there is not a “good-as-such,” at which all activities and things aim, then one consequence is the division of values spheres which can be arbitrated and, following the CNS’s general modality, technologized. Even if one accepts that there is no substantive “good-as-such,” flourishing was precisely the term we used to indicate the need for a question of a shared, mutually comprehensible standard for inquiry, work and reflection on the significance of work in the emerging sciences.

For Fisher, this was not foreign territory. He is the son of a teacher at St John’s college as well as a former student himself. St John’s is one the United States’ oldest institutions of higher education and today one of the few that maintains a Great Books of the Western Canon teaching curriculum. It is a primary text only, strictly controlled sequence of reading the great works in philosophy, literature and the natural sciences which moves students from *The Gorgias* to *The Physical Principles of the Quantum Theory*. This tradition was started by Mortimer Adler and Robert Maynard Hutchins at the University of Chicago in the 1930s and has since found several sites in higher education in the US. To be part of this tradition is to cherish a ratio of fundamental questions of knowledge, unified in a corpus, to actual transformations in thought. To be part of this tradition, normatively speaking, is to cherish *Bildung*.

Existing Modes of Subjectivation.

It seemed to me as though the workshop in 2009 had had an effect on Fisher. During our discussions the following day, he posed the question back to me of

how, and whether, a self-transformative ethics is conceivable in a regime such as the modern laboratory. With regards STIR and these sites of innovation, he suggested

“we can think through three levels: ‘possibility’; to what extent is transformation of these laboratory practitioners possible? To get at that, we have to understand what are their existing modes of subjectivation? On that Paul [Rabinow] and Gaymon [Bennett] give a lot to work with in terms of diagnosis. We should continue to develop that. I’m enamored with the language of ‘learned incapacities.’ The lab as site of pedagogy is crucial. This question of pedagogy relative to the *polis* is not so much just the lab now, but the distributed infrastructure of the laboratory as a space of habituation, or an incubator for ... what?.. civic leaders and participants. The people that Obama is hiring are scientists and they are shaping my world. That’s why I love Dave Guston’s formulation from yesterday, don’t leave science to the scientists Obama!”

This was, I think, the first time I had heard Fisher name an end toward which change should be oriented. It is perhaps not surprising that it was a political end and neither perhaps is it surprising that the end was named relative to the vision for engagement between the sciences formulated by Guston, since STIR is one arm of the larger CNS Boundary Organization.

In light of this, Fisher recapitulated his diagnosis that had oriented the STIR project: In Colorado, where he had conducted his Ph.D. and the first version of what would become the STIR protocol, his diagnosis was one of goal-displacement. The aim or purpose of scientific inquiry, in this diagnosis, is to conduct, curiosity driven science, “in order to explain where I am not who I am.” As he continued with the recapitulation,

“In order to conduct that inquiry practitioners require resources; expensive ones: infrastructures, networks, capital. The means to conduct inquiry thus becomes the goal. Am I doing research in order to conduct research, or am I doing it so that I can get a new grant so that I do research to get a new grant?”

Indicating that such a simple rhetorical mode of frank diagnosis would not be acceptable for those at whom the diagnosis is aimed, he followed up his question with a caveat:

“The lab, like the *polis*, will not tolerate the Socratic torpedo fish. There is an entire economy and culture that develops around these ends means displacements, in terms of resources, funding hierarchy and now in terms of responsibility and public good. This institution of science becomes stable

and pathways open up to communicate with different audiences, in a specific mode, and that's why our work at CNS is interesting because we adapt multiple modes and negotiate multiple firewalls.”

Such adaptation requires that in the case of STIR, Fisher began with these means-ends displacements, and relative to them developed his mode reproducing a relation between scientific means and ends. Such a mode, as he said, negotiates ‘firewalls.’

“I mentioned this before, when I was at Los Alamos, one of the lab managers asked me, so do you think nanotechnology is new Erik? It was a firewall. Depending on how I answered would mean our communication would either deepen or turn more superficial. I said, it is and isn't; it involves new capacities as well as branding, etc. Partly because I was able to pass this ‘test,’ the conversation then actually went public, in a conversation between this lab manager and a second one, who said ‘I don't think we're doing enough to ensure safety’ and the other one says ‘no, the correct answer is that we're doing everything in our power and that when we see that nano is not as dangerous as some think we will relax.’ But at home with the kids or in bed or over a glass of wine, he might say, well, of course its concerning, but I have a job and it's my professional duty to keep this institution moving forward. So there are a series of levels of truth telling, a pluralistic level, I'm hesitant to say that it's not truth telling.”

This had become a recurring topic between us: from what position, and in what manner, can a human scientist speak to biologists or other natural scientists? On what question or problem can she speak? Of course, the lab manager, like many others, will have a plurality of levels, and veridictional modes through which she speaks. But what the CNS in general and the STIR project in particular did not take into account is a specification of their own veridictional mode and its relation to how such speaking should be calibrated to a field of relations. Again, the striking thing is that this was not unintelligible to Fisher;

“So we return to the question Bennett posed [at the workshop], do we really want to spend our time building capacities for scientists? Why bother to do what CNS and I have done? And I think I have an answer. On some level, these are different modes of engagement and I tell the people in STIR and CNS we need Human Practices to keep taking the direct approach, because if we're the only game in town, people won't understand the motivation.”

By not “taking the direct approach,” I read Fisher as having said that adaptation to the natural scientific system requires that he, in his position as human scientist, not affirm any particular mode of veridiction with respect to what was being said or

happening on the part of natural scientists, nor state a metric by which their speech acts should be taken as significant. Nevertheless, he understood how a refusal to make such specifications has a price. A year later, we would be able to revisit the topic.

Reconstruction.

Within our Human Practices work, these fundamental questions and actual transformations were oriented by what Richard McKeon called problematic and operational methods, rather than dialectical and logistic methods, which I will try and show were characteristic of the STIR modality.³²⁴ This is to say that our orientation in Human Practices was, operationally, from a knower to the making of knowledge, and, in a problem based manner, from that which is known to the making of more things knowable. Dewey's term "reconstruction" oriented our thinking on the ramifications of the making of knowledge and the making available of the knowable. Reconstruction had been an orientation to the ethical question of knowledge. Reconstruction was diagnostically posited as part of the series which might be constitutive of a new figure in which we were participating, a figure within which the human and natural sciences would co-labor. As such, the series posited by Rabinow and Bennett in 2007 - to answer the question 'what is being problematized in the emerging biosciences today?' - consisted of an emergent object, a metric of flourishing and a mode of veridiction oriented to reconstruction. Dewey's conceptualization of reconstruction as a veridictional capacity is the following:

"Reconstruction can be nothing less than the work of developing, of forming, of producing (in the literal sense of that word) the intellectual instrumentalities which will progressively direct inquiry into the deeply and inclusively human—that is to say moral—facts of the present scene and situation."³²⁵

Veridiction, in other words, is closely tied in a reconstructive mode, to an ethical end.

In 2007, the Human Practices experiment was designed to establish a collaborative mode of work on the making of knowledge and the making available of the knowable in bioengineering. Clearly this did not happen in the way it had been designed. Nevertheless, as a research team, we continued to profess confidence in the possibility of participatory engagement, even when we began to have significant doubts about its plausibility.

³²⁴ Richard McKeon, *On Knowing: The Natural Sciences* (Chicago: University of Chicago Press, 1994).

³²⁵ John Dewey, *Reconstruction in Philosophy* (enlarged edition, Boston: Beacon Press, 1948), xxvii.

We came to the conclusion that the challenge of reconstruction involves more than unequal power relations, trained incapacities and limited talent of anthropologists. The concept of reconstruction itself, we came to recognize, requires critical re-examination in light of our current conditions and experiences. This re-examination turns on the object and mode of reconstruction. First, whatever else Dewey means by “the deeply and inclusively human,” as the object for the objective of reconstruction, the concept needs to be problematized. For example, given that there are multiple figures of *anthropos* and furthermore since they do not capture the totality of *anthropos*, referring to the deeply human as though it were given, known and regulative, is today implausible and unwarrantable. Second, we know from our experience that even if one is capable of designing, of developing, of forming, of producing conceptual tools and equipment—what Dewey terms “intellectual instrumentalities”—that work is not by itself sufficient for reconstruction.

We were guided by Dewey in our endeavor to “progressively direct inquiry” through our conceptual tools and equipment, into episodes and events which are characterized by discordancy. Nevertheless, Dewey does not give any indication as to the subjectivation or subject position of the one capable of doing “nothing less than” reconstruction. The position of this subject relative to the “facts of the present scene and situation,” however, which are discordant and thus require remediation, indicates that, minimally, without a significant transformation of the scene or situation, such reconstruction has not taken place. Since for Dewey, a situation in the full sense does not exist independently of the need for inquiry, the most we in the Human Practices group were able to accomplish was to determine something about our experience of discordance, but not reconstruction of such discordance.

We argued previously that the capacity to contribute collaboratively to a reconstructed situation constituted a basic parameter of flourishing, which was the metric of our ethical engagement. Today it seems clear that for anthropological inquiry into the actual, flourishing, understood as reconstruction, is just as troubled as the deeply and inclusively human and perhaps for similar reasons. How else, then, to think flourishing in inquiry today? This is a problem we will need to face.

Deliberation and the Dialectic of the Ethical and Thematical.

A question to which I continually returned during my tenure in the STIR project, during conversations with the other students and with Fisher, and a question that still puzzles me today, is, what was the metric of the STIR project? I understand that to ask this question is to place the STIR project within the conceptual framing of the Human Practices project.

One of the striking things was that in asking the question, it became clear to me that even though such a question is presupposed in the very activity we were undertaking, ‘engagement’ of the work of natural science on the content of their practice, its ramifications and the ‘values’ (in the language of STIR) of those that conduct it and of those whose are affected by it, such a question was part of the ‘unsaid’ of the project. Indeed, the more it became clear to me that the question of metrics was really a precise formulation of how one could inquire into the significance of an object and of the inquiry into that object, the more it was clarified that the STIR project was constituted by precisely not asking this question.

I had made a premature and erroneous judgment that the metric of STIR was “normalization.” Normalization orders aspects of people and things according to a dynamic standard of regular distributions for a homeostatic purpose. I had thought that STIR could be characterized this way because as a technology of “modulation” it provides such a dynamic standard from within on-going research, but that insofar as “environmental” irritants, in my experience, were easily excluded, I thought that it could be said to be oriented to a homeostatic end. It was an erroneous judgment insofar as this judgment did not take sufficiently seriously the open and unresolved question as to the *telos* of transformation for the STIR project.

As Fisher described it in 2009, the diagnosis that oriented what would become STIR was a means-ends displacement in which the means became the end. Fisher’s response to this situation was to accept this situation as a de facto situation and to then “work from within it” for the purpose of transformation. Although not necessarily so, a presupposition of this move on his part indicated a minimal dialectical presupposition of the modality of engagement, what we might call, following Faubion, a weakly dialectical relation of the thematical and the ethical. Let me remind the reader that the thematical is that aspect of the ethical domain characterized by system homeostasis and the ethical is the rupture and change of such homeostasis in response to changes in the system and its environments.³²⁶ In its design, STIR was oriented to system perturbation and change.

I think in a strict sense meant by Rabinow and Bennett, there was no metric per se orienting the STIR project, precisely because the mode of engagement is by way of such a dialectical method, rather than a problematizing mode.³²⁷ Even

³²⁶ Faubion, *An Anthropology of Ethics*, 114.

³²⁷ One further question here is what sort of a reflective judgment is a dialectically oriented reflective judgment, one capable of finding a ‘way out’ of the *aporias* of thought and experience? Although Fisher never explicitly said it, I would think that he would not go so far as to say that the Real is Rational. Nevertheless, as will be described, the

though “the problem” is the same, i.e. there was a breakdown to which both STIR and Human Practices were responses, the response from Fisher did not ask the question, “what is being problematized?” and from that question attempt to design an intervention adequate to that problematization. Rather, because the diagnosis was a *technological* one, of the relation of means and ends within relations conceived as an autopoietic system, the response was a methodological one, hypothesized to ‘move’ in a weak dialectical form from de facto modulation to reflexive modulation and to lead to deliberate / goal-directed modulation, which becomes part of the de facto modulations, etc.

To my questions of metrics and my attempts to think between Human Practices and STIR, Fisher, in 2010 at our third workshop, grew irritable with the terminology and the question and pushed back on me; “Why is metric even a question?! Which came first ‘metric’ or ‘flourishing’?” The question was a good one; from his position at the ‘technological’ level, he was putting in question and asking me to justify why I was approaching the STIR project from an equipmental point of view. In effect, the reason for posing the question of metrics is to answer the same question Fisher had; why bother doing this?

Indicating that we had developed at least a semblance of a working relation and a respectful agonism, Fisher worked through the project’s design in response to my question: “Perturbation is the phenomenon, the process, by way of which you judge and through these perturbations one asks is there the possibility of questioning the unquestionable and of re-structuring practices?” This he indicated as a ‘procedural’ metric. He followed up with the dialectical frame of the project to situate how the significance of ‘perturbation’ can be characterized; “it’s a dialectical process, which moves from de-facto, to reflective to deliberative and the way it returns to de-facto may be changed.”

De-facto modulation is the way in which the relation between opportunities and outcomes are mediated by alternatives and considerations. Reflective and deliberative modulations are the changes in such mediation when the fact of such modulation is raised to consciousness, allowing for the deliberation of such mediation. Fisher continued, “There is also an outcome metric, that’s harder, but I think, incrementalism, is the result of perturbation such that there is a qualitative improvement, relative to ... relative to what? ... I think you’d have to say relative to the system in which STIR claims to operate, in which case you have multiple levels; the lab, the funding agency, and it depends whether you bind it with

relation of an activity such as STIR to the ‘unfolding’ and self-correction of consciousness, does point in this direction.

C.f. Robert Pippin, “Avoiding German Idealism: Kant and the Reflective Judgment Problem,” in *Idealism as Modernism: Hegelian Variations* (Cambridge: Cambridge University Press, 1997).

congress and the legislature, or whether you're bounding it as part of the policy process, or whether you're actually opening it up to the political process. Now if you open it up to the political process, then we can't do much better than how Dave Guston has attempted to develop metrics [i.e. justified by a conception of democracy, discussed in Chapter 3], if you're bounding it more narrowly, then it's more and more technocratic."

A Variable Metric of Deliberation: From Democracy to Technocracy.

At our third STIR workshop in 2010, Fisher presented to the group of graduate students in the project an exemplification of the tensions in the stakes of the project, although not an exemplification of the use of the protocol as such. The story was from Bastian Miorin, a then Masters student at Science Po Grenoble who went to ASU for a semester as many of the students in STIR project would do, where he was affiliated with, although not a member of, the STIR project.

Miorin had been perplexed that the laboratory in which he worked, whose research was on the environmental effects of nano particles, was disposing of nano particles in the municipal trash. A conversation ensued between Miorin and members of the lab where a series of alternatives and considerations were named, specifically; either throw the particles in the municipal trash or to transport them to a hazardous waste facility. The latter option would mean incineration and so a larger carbon footprint, and furthermore the hazardous waste facility was over twice the distance, adding to the overall environmental impact. As Fisher narrated the story he told of how this same phenomenon happened in his own study as well. Whereas before he entered the lab and began asking question, nano particles were going in the trash, once he began asking questions, they were then collected in a plastic bag and put into a metal filing cabinet, "that no one was allowed to touch."

A lab meeting was called by one of the laboratory members to discuss the issue. Miorin, however, was not invited. The lab deliberated, in Fisher's recitation of the events, equanimously, and gave reasons for eliminating waste particles by either hazardous waste incineration or the municipal trash: "Then the guy comes back to Bastian and says, we can't figure out what to do, as there are reasons to go both ways, what do you think? And then they have a long conversation where Bastian lets his colors fly, 'you should think about what members of the public think about what you are doing.'"

The phrase, "letting his colors fly" struck me as curious at the workshop, and it led me to reflect on the relation of the individual participants in the project to our discussion of metrics. Insofar as the STIR project was a formal method for integrating two planes, a plane of a 'decision space' relating considerations to alternatives (logical relations) and a dialectical plane moving from 'de facto'

decisions through awareness of such a decision space to a change in how the decision space is worked through, the purpose is reflection, or deliberation on the part of the natural scientist. The moment at which the human scientist says something to inflect the relation of these planes, is in fact a *parameter* of the metric of deliberation.

Thus we have a curious inversion: One might have thought that relative to an object, such as the dialectical relation of the thematical and the ethical, deliberation would be a parameter for moving toward *a changing end of this dialectical relation*, an end which must be deliberated on, posited and attempted to realize. This would be something akin to Habermas' communicative rationality.³²⁸ Whereas in actual fact, ends named by subjects in the ethical field constitutive of the object relation are rendered here as parameters relative to the metric of system deliberation, with no particular end. It is perhaps then not surprising how Miorin's story ends;

“And then at some point the laboratory practitioner says, all this is, is a waste of my time; I'm sitting here talking with you about this decision, which I perform every day, which is potentially problematic, but with no way to resolve it, and meanwhile it's piling up in my lab and I could be doing something useful and instead I'm talking to you about this.”

The Purpose of Deliberation?

“How do I make sense of my findings?” Daan Schuurbiens had been asking himself this question longer than most of us in the STIR project, insofar as he was an early adopter of midstream modulation and had completed his first STIR study in January 2009 when the STIR project officially began. He conducted his Ph.D within a philosophy department housed within a Faculty of Technology, Policy and Management in a technical university in the Netherlands. Schuurbiens presented to us, at the third STIR workshop, a common framing for his STIR analysis which centered on a distinction between first-order and second order reflective learning.³²⁹

“first order is within boundaries of a value system and background theories, so in science and technology this would be improved achievements of a scientists' own interests in a network. Whereas second order reflective

³²⁸ Jurgen Habermas, *The Theory of Communicative Action, Vol. 1: Reason and the Rationalization of Society*, trans. T.McCarthy, (Boston, MA: Beacon Press, 1984). C.f. Martin Seel “The Two meanings of communicative rationality,” in *Communicative Action: Essays on Jürgen Habermas's The Theory of Communicative Action*, eds., Hans Joas and Axel Honneth, Cambridge: MIT Press, 1991.

³²⁹ Poel IR, Zwart SD. “Reflective equilibrium in R&D networks,” *Science, Technology and Human Values* (2009): 23:5–7. Daan Schuurbiens, “What happens in the Lab: Applying Midstream Modulation to Enhance Critical Reflection in the Laboratory,” *Sci Eng Ethics* 17, 4 (2011 December): 769–788.

learning involves taking the background theories and values as the object of learning, so second order reflective learning is a form of reflection on the research system itself. What is important is that this is symmetrical, and in that sense reflective learning is not therapy for the scientist because it can also happen to me. So in the talk I don't want to go into my examples of first order, which are examples broadly speaking of health and safety, responsible conduct of research.

So I have three examples:

The first example is about the discussion of integrating a human genome in a mouse. So this researcher had to insert a transporter gene into his microorganism and he was wondering if he should do a human gene or a mouse gene. This was not a topic of conversation in the research group, but when we were talking he expressed concerns about it.”

The student decided against cloning a human gene and opted for mouse gene. To Schurbiers' question as to why it would matter, since a gene is merely a sequence of base pairs, the student reportedly replied that “where it comes from is a bit ethical.” What this meant, according to Schurbiers, was that the student was experiencing a form of symbolic discomfort (“it's an image thing”).³³⁰ He explained to us,

“To me, that example is powerful because it made the moral considerations that he had explicit. He had never talked about those things in the research group before. And then comes the quote over which we [indicating AS], had the email exchange,

Student: I had given it some thought subconsciously, but I never really gave it careful thought. ... Ethics can be very boring, until you reach dangerous territory, and then it becomes fun.

So my [Schurbiers'] take on this was that this was the first time that ethical reflection appeared as something interesting to do on his own work. So that's the first example.”

Second example: He ordered synthetic genes regularly. I asked, would you call this synthetic biology?

Student: “That depends. What is synthetic biology? Much of what is now called synthetic biology resembles what we do: putting a piece of synthetic DNA in a host. But I think synthetic biology is making all

³³⁰ “I'm cloning a mouse gene, because... I decided like I'm not going to do a human gene. At least, there was a choice between human and mouse, well, then I'll go for mouse, that's a bit... safer.”

components synthetically. ... Really to develop a cell from scratch might take another twenty years.

So basically what he was saying was that I don't want to talk about this, it's too far in the future, speculative, it's not interesting. But then I asked him: think of twenty years ahead, if construction of whole organisms was routine,

Student: Then you would need to think about the use, or the goal. If you can build a cell, then you can build other things as well. We shouldn't go into the direction of synthetic higher organisms. There's always a risk that others move into the wrong direction. You shouldn't be using it for other purposes. It's like a knife: you can use it for good or for bad. ... That's why we should maybe think about these things. Then there has to be extra regulation.

“So by asking him to think a little bit further than the now moment, thinking about what the research that he is contributing to might make possible in the future, he now changed his reflections on the object of discussion itself.”

In the third example Schuurbiens pursued the discrepancy between the “general goods of research” and the perceived particular benefit of individual research for “society.” Participants, in his account, gave the response that the demand for social relevance hampers research benefit, and that ultimately hampers societal benefit. Schuurbiens in one instance of this narrative attempted to challenge the assumption of the student by assuming it. He gave the following hypothetical situation analytic philosophers are fond of: “if we say that one cannot predict the societal benefits following from research and if one supposes that academic research should be free, then how to determine which types of research to fund given that funding sources are always going to be limited.” He gave the student the conundrum: “how do you make the decisions whether I should fund genetic modification of cyanobacteria, or whether I should maybe fund your colleagues who do evolutionary growth of cyanobacteria?”

Schuurbiens then recounted to us the student responding by handing over the political judgment of which research to fund to “politicians” and that ultimately “policy has to be made, eh, like, on the average of what people think. It's not like the policy can't be made on the thinking of one person only, but of what most people think.” Schuurbiens narrated to us at the workshop that he gave the student further hypothetical situation in which a vote was taken and the modification of organisms in labs was voted down, what would the student do then? The student

responded, “if someone would have a good argument I probably would... not quit my job, but find a different approach. I guess, I don’t know.”

Schuuribers, finished the presentation by indicating that these kinds of discussions, to him, “problematize the unquestioned assumption that the demand for societal relevance hampers societal benefits. That’s why I think that this is an interesting dialogue.”

A Critique.

Fisher’s response was illuminating and was important relative to where we had come in our collective difficulties in thinking together about standards of judgment and ends toward which we wished to work. To capture it succinctly, he asked Schuuribers to discount the second example, “because all that shows is that there is some utility perceived in doing this activity [the STIR project], it begs the question, well what are you doing? So I’d get rid of that one.” For Fisher, the first one was potentially interesting, in his words, because within the narrative one can see how, what Fisher calls, “latent concerns that weren’t otherwise expressed,” were stirred up. Instead of labeling them as “moral,” Fisher suggested, “and then moving on,” he asked Schuuribers to dwell on the example.

“But then the next question is, so what? Did this stick with someone? Did it get taken up later? We’re waiting for the other shoe to fall, and I was thinking that, I was thinking do we have to go back to first order in order to see a change in practice? That might be the interesting thing about why, you know, we care about first order.”

DS: I’ve got hunches, I mean I’ve been following them around, but that’s all highly speculative.

EF: unless you’re going to speculate you’re leaving me hanging, all you’re doing is saying, well anyone could go into the lab and say [adopting ironic tone] “what do you think about this?” and the response would be “well I don’t know” and I say “well think more about it” and they say “err ok I guess it could scare me.” That’s not in itself enough to make me think, you know, you’re using a method, you’re systematically applying it, you’re studying it carefully, you have these questions that are both practical and theoretical, where practical might mean political or it might mean ethical, and now you’re giving me some information, some data, to analyze with respect to these things. I’m not completing the loop; I’m not seeing what the point is or why I should care. If I happen to be critical I could say, you haven’t told me anything.

The third one is that you've got a pattern or trend where you've asked more than one person at more than one time in more than one place and there is this consistent response and you've developed a way of separating it out and reflecting it back, you can see it and you hope they can see it, I think that's the strongest thing in terms of standing on its own. It still begs the question: so what? Did they get it? Or do you use that to explain something in terms of what you're trying to do and what the challenges are? You need to take me to the next level.

I'll be critical now: I'm not convinced in any of these three cases we have second-order reflective learning. In your narrative, I was convinced, now I'm not.

AS: Is that insofar as the learning component isn't present?

EF: I don't even see that there is second order reflection.

DS: Second order reflection was defined as reflection on the research system. Now you're asking, *and that needs to have consequences*.

EF: No, I'm not asking that, I'm saying, first I need to be convinced. You could show me a sign that says the research system exist, and you could prove that I just reflected on the research system and therefore it's second order, and I might have to agree with you logically, but somehow it doesn't seem very relevant. So that's my first question.

The second question is: just proving that reflective learning occurred, I'm wondering, why should I care? If there are consequences, I care, if there is a problem, I care. Consequences would be a sure fire way to do it. There has to be something more.

So there might be an additional story, you've got other material, so you could analyze the story where somebody actually went from being more deterministic to a more constructivist view point, you've got other material and I think you might need to put a couple of things together.

During this discussion, Shannon Connolly, another of the graduate students, intervened with the observation that Schuuribiers had been vacillating on whether to include his "lab coat example," fearing that whilst sometimes it is received quite favorably, at other times it is considered trivial. The story is the following, and I recount it because it is important as an indicator that ultimately the mode of justification and judgment for the STIR project had to be first order, instrumental and within the values and norms of existing scientific research.

Schuurbiers had observed several research participants at work in the lab using two plastic gloves to prevent getting acrylamide on their skin, and subsequently getting something from a cupboard while still wearing both gloves. When he was invited to present his findings to the research group at a final lab meeting after completing the study, he expressed concern for compliance with regulations, feeding back his observation of the two gloves. The example, he told us,

“sparked a hefty debate. Some researchers in the group felt strongly about complying with EHS regulations, particularly wearing lab coats, but had been unable to convince others to follow suit. A few days later I received unsolicited news from one of the group members that several lab members had now started wearing lab coats again. When I asked for an explanation of what she thought had occurred, she stated:

It happened many times that when I was handling ethidium bromide gels some drops reached my clothes, or the glove unprotected areas of my hands. ... Meanwhile the lab coat was clean and ironed on my chair since some good months. ... I was thinking that one day I should take the decision to wear mine, even though I'll raise some eyebrows. ... Then came your presentation ... and I remembered how I used to take care of my safety and my clothes. ... Monday, after the seminar, on my way to the lab, I noticed that [S] wears the lab coat - he was spraying nitrogen on some concentrated samples and needed to protect his clothes. I said to myself, "that's the moment. If I come back fast we will be two wearing the lab coat." I took it and wear it for the rest of the weeks.

Fisher pushed Schuuribers to use that example in his presentation and to compound it with the first example.

“In the first example you’re going to claim that there were latent concerns that were not expressed, now this is hard because it’s in one moment. When I tell my story of latent concerns I can show how, first we have the example without the word messy, then we have the example that indicated there might be something there, then I ask what it is and then I’m told it’s messy and then I ask what is messy? So over a three week period there is this progression, it’s convincing that there seems to be deeper and deeper reflection. I think what you might want to do is say, this is exhibit A, before, this guy said nothing, now it all comes out, and there’s some interesting material here, so what? I think that there is some reflection and that there is more stuff coming up, and that it could be taken up, and that’s the kind of work I did. Now let me show you an example, of how that kind of reflection, which doesn’t always have consequences, but it can have consequences.

You don't care that they started wearing lab coats, but it's a marker that there was a change in practice and it can be tied to somebody's latent concerns, which you're saying, you have two examples of somebody having latent concerns, in one case we can show a consequence, in another you can't.

That you were the catalyst for expressing the latent concerns is one point, that there were latent concerns that were expressed is potentially interesting in itself and the fact that they can sometimes lead to changes in practice, I think that brings it home."

DS: Yeah it brings it home but that's like fuddling your data, because that is a different story all together, from what I've presented now, so then to drive your own point home, your mixing things up chronologically just to make them look nice.

EF: They're related logically, because I thought and maybe I'm wrong, that in both cases you're using a similar methodology, on scientists who are in a similar position, where they don't have time to reflect on what they're doing, or they're not interested in it, you ask them certain questions and they come back at you and they tell you things that they hadn't told you about, and we're assuming that they are now seeing things about themselves that they didn't know about before, and that's why I'd say these are two examples, one could be in Delft and one could be in Tempe, and you know you could I've got a hundred examples but I'm pulling these two out because in both cases there is an expression of latent concern, in response to my agitation of them. Why do I care about it? I think it's important on its own, but you might not, so let me tell you that in the second case it led to a change in practice, not just with the individual but with the group.

DS: I could present the lab coat example, then say you know that's EH&S [environmental health and safety] and so it might be more interesting to pursue more broader themes, and then come up with these examples. But I'm not going to follow up this saying 'and practices changed,' because they simply didn't.

EF: I didn't suggest that you should say that, that would be lying. But I am saying you have got example A and example B, and in both cases the responses to a method of feeding back information and going a step further and asking about that information that was fed back, in both cases there is a similar response discursively; you are being told about a person's self that your hypothesizing they didn't know about and you didn't know about. And then in example B but not A there is a change in practice."

Venue-Mode of Subjectivation.

In 2006 it seemed that SynBERC could be a significant venue; not only in itself, but that our participation in this particular venue could connect to my experience in the STIR project. In each of these venues, work was taking place on forms of possible knowledge about *bios* and *anthropos*, in each of these venues there were jurisdictional and subjectivational demands which we in Human Practices were trying to render visible so as to intervene on them through participant-observation. One of the lessons learned from experimentation on the relation of science and ethics is the difficulty in shaping a topic across disciplinary lines. This was as true in SynBERC, as it was in STIR. Prior to fieldwork, we had oriented ourselves to the goal of flourishing, and its implementation in the relations between science and ethics.

Our orientation in Human Practices was for collaboration to be the means for reconstruction, even if we left as an open question whether the “deeply and inclusively human” was an object which we could know anything about. We had argued that the capacity to contribute to a reconstructed situation constituted a basic parameter of flourishing, which was the metric of our ethical engagement. Within SynBERC and STIR the subjectivational demands of the academy were a critical limitation to developing anything more than a cooperative mode of work and hence of moving toward reconstruction.

The final STIR workshop in February 2011 crystalized these thoughts. It was a public event in Washington, in partnership with the Woodrow Wilson Center, in which in addition to public keynote presentations five student presentations were arranged in which there were to be joint presentations between a social scientist and a natural scientist from the STIR projects. For me, the ethical significance in the conceptual distinction between cooperation and collaboration came to head at this workshop.

I had met Sonja Billerbeck, from the laboratory in Basel, the night before our presentation. We went over how we would do it; I would introduce her, she would introduce her work and I would continue with the presentation, outlining what STIR was, mentioning how I got involved and what is distinctive about the project, namely, that unlike a lab study observations and elicited responses are ‘fed back’ to research participants. I had geared the presentation to a description of what I did in Basel, rather than an analysis of “STIR.” I did the former rather than the latter in part because I was not sure how it would be possible to talk about the STIR project, especially under the conditions of the workshop. This I think is important, the fact that whilst we were supposed to be testing and engaging in a *shared* methodology, we lacked spaces in which to really think together about the

method; this is as true for the social-bio science relation as the relations between the social scientists in the STIR project.

The experience at the third workshop had suggested that I was not authorized to ‘represent’ the STIR project. At that time I had written a piece trying to compare STIR and Human Practices, posing questions to those in STIR about the assumptions, externalities and limitations of the project. Two seemed to be of particular importance; what the criteria of judgment relative to the project would be and how the laboratory (and specifically engagement with doctoral students) had externalities and limitations as the site of work. The response to this was that I had misrepresented STIR. I was admonished for not being “cautious” enough by CNS director Guston. The fact that the STIR and Human Practices research occurred in different laboratories makes comparison “technically challenging” although it does not “fatally condemn” such an effort. Guston did not take account of the fact that I was specifically comparing the modalities of the engagements and furthermore, the ‘sites’ of comparison were not the laboratory. I had suggested that STIR, insofar as it is interested in the “midstream” of research, takes the lab as the site and offers (at its best) technical criticism of the mechanisms of decision making in this space. By contrast, in a mode of Human Practices, individual labs were one among a range of sites of which the significance of work in synthetic biology was being asked. The response from Fisher was that “modulation is something that ripples through a system.”

In Fisher’s view it was a misrepresentation to say the STIR project is interested only in the lab. The STIR protocol asks researchers to investigate what the mandates or policy environment is like at the beginning of research. STIR researchers then pose the question of awareness of such policy to individual scientists in the lab. That is true, but the site of intervention is the lab, even if the environment is observed. Observations of the environment are given significance relative to the activities of the lab. My point was to say that this relation was the other way around in the Human Practices modality.

A colleague in STIR, Paul Ellwood was puzzled by Fisher’s response, since he agreed with me that the lab is the site of observation and intervention. Indeed a day later at the workshop in Tokyo, Erik himself said whilst modulating things at one level may have ‘ripple effects’ that show up in other areas, “STIR is not designed and set up to track, because we’d have to look at things over months if not years. And we’d have to look at multiple sites that are connected in way that we might not be focusing on. But still there is this notion that there are temporal spatial human scale changes that ripple over time or place or social networks.”

With this in mind, the presentations at the fourth workshop in Washington were set up as “dialogues” between a “STIR scholar” and a natural scientist, with the aim being to demonstrate the worth of such engagements. What was most bizarre is what some of the other researchers were willing to show as evidence of a *successful* collaboration. Connolly, for instance, had suggested the fact that she did not know what a Single Nucleotide Polymorphism was and that it took her a few weeks to build up the courage to ask was evidence of overcoming “the two cultures barrier.” Further proof of collaboration was the proficiency with which she had done PCR. This example was mobilized towards showing the “price to be paid” to do socio-technical integration. The cost it seemed we were being told, was that one’s own concerns are not important and that the social scientist must do like the other in order to “gain acceptance,” to then “gently” ask questions about why they do things like that, or how they might do things otherwise.

This is a well-worn anthropological method. In many circumstances it is adequate to the problem; e.g. if the question is how knowledge is made in a lab, familiarity with the instruments is necessary. But, if the problem as is implicit in the STIR project, is how is science going and what is it going towards (since there would be no point in asking the how question if there were not at some moment a question of end), then the question becomes what kind of question can be posed, can be heard and can be responded to.

A striking response to Connolly’s presentation was from CNS director Guston; the response was the praise of ignorance as a research capacity. I was angered by this willful self-nullification. Whilst I do not hesitate in acknowledging that it takes time to learn, the valuation of not knowing as a capacity for social science inquiry into science struck me as destructive. My ire was further provoked with a comment from the audience in which a senior social scientist who, from her own self-description, spends her time on interdisciplinary committee meetings “pleaded” with those assembled to wage a battle against “jargon”; since jargon etymologically stems from the ‘chattering of birds’ this is certainly an unflattering portrait of the search for knowledge. The question of what is intelligible and to whom, however needs to be posed. In the context of an attack on “needing to know anything” in order to do social science, an attack on ‘jargon’ can only be heard as a call to “Keep It Simple Stupid.” Indeed, in response to this comment one of the presenters, a natural scientist, had said of her social scientific counterpart, that he had generously “dumbed things down.”

The rest of the meeting was oriented towards securing more funding for STIR and STIR-like projects. It seemed as though two things were clear; a serious evaluation of STIR was not going to happen and that the workshop’s key aim was to brainstorm funding opportunities. It was good to hear at least one other STIR

researcher rather vexed about the fact that Fisher was talking about scale-up before any evaluation had taken place. This in part comes from her twenty years of experience in technology companies; “you can’t begin to talk about scale up before you know what you’ve got!” It shouldn’t take an MBA to see that, but when the bottom line is profit not symbolic capital and grants, the question of evaluation is really pressing. If one were to take evaluations as indicative of a form of life, one might think they were pressing in this situation as well. It seems as though in this case of ‘knowledge production,’ the question of how one would know if it were worth carrying on and if it it’s necessary to change anything, was not deemed worth posing.

Conclusion

Determinations & Double Binds

“One is always responsible for one’s position as subject. Those who would like to may call that terrorism.”

–Jacques Lacan³³¹

“If there are consequences, I care, if there is a problem, I care ... There has to be something more,” Fisher told Schaubiers. And yet, it wasn’t clear to me why Fisher cared. Which consequences? Which problems?

From the Present to the Actual.

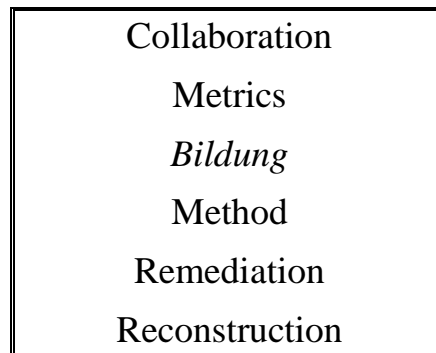
It was clear to me from the beginning of my engagement with STIR that given how I was forging the problem of the relation of the human and bio sciences, the justification for participation and the reason for wanting to contribute to the work of knowing something about this problematic relation, could not be via a consequentialist logic of assisting bioscientists to do more efficiently, or even more safely, what they already think they are doing. This was to a degree the reconstructive orientation of my endeavor; to not only increase the means to pursue the ends we think we already know; but rather, to attempt to develop a practice of reflection on the ends of engineering biology given the manner in which this engineering activity is pursued and given the environments in which it exists. I did not reconstruct any situation. As explained before, what it would involve “to progressively direct inquiry into the deeply and inclusively human—that is to say moral—facts of the present scene and situation” is opaque. Furthermore and more importantly than the conceptual indetermination over the deeply human, reconstruction was not possible because of an incapacity to direct inquiry into the ‘present situation.’ By “direct” I mean to “work over” or “change” the present *qua* ‘situation.’

It has been possible, however, to direct inquiry into the *experience* of having attempted two different forms of intervention into the present situation. Whilst the present *qua* situation was not “directed,” at least in my own estimation with regards to my activity, I have the means to direct reflection on the present *qua* experience of the present. The aim of such direction is to make a judgment.

³³¹ Jacques Lacan, “Science and Truth,” *Newsletter of the Freudian Field* 3/1–2, B. Fink, trans. (1989).

Actual Determinations.

Working with Paul Rabinow and thinking together through what we learned from five years of participation in these setting, we named a list of six terms which we thought would form the basis of that which we could determine about these engagements. *Collaboration*, our own and the failed examples of collaboration; *metrics*, and their existing incommensurability; *Bildung*, the self-formation necessary to be a scientist—broadly understood—with its learned capacities and incapacities and *method*, as the existing problematic manner in which knowledge is produced. Furthermore, two additional terms were crucial for us, *remediation*, which designated the conceptual efforts to deal with deficiencies in situ, and *reconstruction*, which remained as an unachieved goal, increasingly remote and soon to be abandoned.



As we talked about these terms which were so fundamental to our experience of the troubled relation of the human and biosciences, we wondered about how these terms could function as determinations of inquiry. Furthermore, how would they help me make a judgment about these experiences? They would have to do some work. The work that would have to do, as I mentioned, would be to be capable of ‘directing’ inquiry into the present.

The work these terms could do as determinations of inquiry came about in part as the product of our effort to clarify and justify an intuition, discussed and worked over in part with James Faubion, that there was a distinction between the *present* and the *actual*.³³² This distinction turns on the difference between the present, as a medium for any field experience and observation and the actual as a conceptualized domain in which an anthropological position for observation and intervention is possible. It is within this domain of the actual, as opposed to the

³³² This point is taken up as a principle for the motion of inquiry in Paul Rabinow and Anthony Stavrianakis, *The Demands of the Day: On the Logic of Anthropological Inquiry* (Chicago: University of Chicago Press, forthcoming, 2013).

encompassing and ill-defined present, that the identification and curation of determinations of inquiry, taken to be significant, can be asserted in a warranted fashion.

After having arrayed the determinations we came to see that they had a temporal order and that they were paired. This pairing consisted in one term referring to a mode of participant-observation and one term referring to a domain of problematization. In looking at the arrayed list it was clear that collaboration had been central to the design of the experiments from their inception, even if the meaning of the term collaboration, was not stable across the experiments. Nevertheless as a term it provided a similar orientation in both STIR and Human Practices. This recognition showed us that there was in fact a temporal unfolding in these determinations: We in Human Practices began with a highly conceptualized understanding of collaboration as the orientation to our mode of participation and intervention. This orientation was buttressed by specific attention to *Bildung*, which became a fundamental topos for me, and then attention to what had been called “remediation”; the ability to give a new form to a problem, in the experiment of Human Practices this usually occurred through conceptual work, such as the ‘remediation’ of the security problem in synthetic biology as a problem of preparedness. There were also, however, ‘practical’ remediations such as the building and development of several websites to act as a remediated foyer for our work, when the experiences in the venues of SynBERC and STIR closed off, rather than opened up the possibility of co-labor.³³³

In the case of STIR, collaboration functioned nominally although in a looser and intentionally under specified manner. Furthermore, although the term remediation was forged by Rabinow, one could say that the technology Fisher invented was both a change of medium and an attempted remedy for how decision making is made in scientific spaces. *Bildung* was not a ‘foreign’ term for Fisher, however, in so far as the STIR project was parameterized by a method rendered as a technology, it could not be thematized and made part of the self-conception of the project.

Each effort to bring these modes of participant-observation to bear on the experimental situations, collaboration, *Bildung*, remediation, produced specific determinations within an aspect of a more general problematization of science and ethics.

³³³ *Ibid.*

1a. Mode of intervention: Collaboration.

In Human Practices, we maintained that collaboration was essential to what we wanted to do. We recognized, however, that it was only possible under certain conditions and with a recognition of the price to be paid as well as the rewards for doing it. This commitment focused our attention on the goals of science and the question of the worth of a life devoted to such a practice. Within STIR the conception of collaboration was under specified and meant little more than not being thrown out of the laboratory, or slightly more favorably, successful *cooperation* on known problems.

1b. Problematization: Metric:

During fieldwork multiple metrics were present. In the field situation the dominant metrics were prosperity and amelioration. We in Human Practices had thematized the fact that another metric, flourishing, had been central to Western philosophy for millennia and that in the present taken broadly and extending beyond our immediate fieldwork situations it was still part of the available ethical orientations. Our attempts to introduce flourishing into the fieldwork situation through taking participant-observation as a serious challenge proved to be a major site of breakdown. This trouble was, however, also revelatory. By identifying this determination of a specific set of instances, we opened the possibility of inquiring into its place in a broader problematization of the contemporary. The problematization of metrics, it is clear now, is a major part of the multiple discordant responses to the problematic relation of science and ethics today.

2a. Mode of intervention: *Bildung*:

In the present, all of the sciences require extensive work on the self (years of training, socialization etc.) although such activity is taken for granted and held to be exterior to science per se. In *Bildung* such extensive work is explicitly thematized and is not held to be exterior to the pursuit of knowledge and truth. In the present, the available types of socialization for a scientific life are sharply delimited and methodologically constrained.

2b. Problematization: Method:

Experience in the STIR project showed that the dominance of method is easily identifiable and widespread in the present. Its dominant is at odds with a different form of reflexivity which Foucault had called meditation. By contrasting method and meditation, Foucault was indicating the way in which within the history of systems of thought, there was a fundamental transformation, although not elimination, of the work of thought on thought. It is closely related to the distinction between philosophy and spirituality indicated in Chapter Two. Method

secures the rules of what counts as true and false, that establishes the steps to achieve truth or result in falsity. By contrast, meditation has as a fundamental requirement, as a form of the work of thought on thought, that it includes the exercise of a subject on the subjectivity adequate to be a particular subject of knowledge. If one has a commitment to *Bildung*, as well as a commitment to addressing problematic domains through these modes of participant-observation, it is not possible to acquiesce to method's preeminence. Hence, the particular determinations in the various fieldwork situations indicated both the specific breakdowns encountered as well as opening up the possibility and need to question the place of method within a more general contemporary problematization.

3a. Mode of intervention: Remediation:

From the inception of these experiments "remediation" was a way of naming the effort to find new media in which to pose problems and work on them. This I think is as true of STIR as it is of Human Practices. Both projects were oriented to a diagnosed "need" for remediation. In the initial phase of our interventions we sought to remediate the situation in SynBERC through conceptual work. Such efforts were met with indifference and refusal. STIR as a remediative technology sought to give a new medium to the categories of decision making so as to improve decisions made.

3b. Problematization: Reconstruction:

The STIR project delimited the place of the biosciences in a broader contemporary space in a traditional fashion. Such delimitation accepts the reigning hierarchical relations of the bio and human sciences. It further accepts reigning justifications for the worth of scientific inquiry and the parameterization of problems by the biosciences. The Human Practices project had as a necessary condition of activating these modes of intervention in the field situation the transformation of this established relation. The incapacity to change the manner in which problems are posed for collaboration, by whom and how, was a structural impediment to actualizing reconstruction.

What I have outlined are three pairs: three modes of participation/observation (collaboration, *Bildung*, remediation) each paired to a problematized term (metrics, method, reconstruction). The former trio are modes of action. To a degree they were actualized insofar as the unsuccessful effort to activate them in the present (qua reconstruction), could be affirmed reflectively after the fact. The latter trio are diagnostic of the way in which the former trio are to be affirmed. They are nonetheless part of a problematic scene and situation. What is necessary now is a judgment about how they are *actually* problematic.

Double Binds.

Gregory Bateson, in collaboration with colleagues developed the concept of the “double bind” to explain, as well as intervene on, the experience of symptoms associated with schizophrenia.³³⁴ I do not intend to develop a psycho-pathological reading of the relation between human and bioscientists in this inquiry. Nevertheless, the concept of the double bind is illuminating with respect to making a judgment about precisely what is problematic about the relation between these attempted modes of intervention and the broader problematization of science and ethics on which they were supposed to intervene.

According to Bateson, a double bind situation has three characteristics: The first is that a subject considers that the discrimination of the significance and meaning of statements is crucial to the sustenance of a relation which the subject cares about. The discrimination of meaning is critical for the subject, because such discrimination will allow an appropriate response and hence the continuation of the relation. Second, the subject is in a situation in which another subject in the relation is expressing two orders of “message” and one of these orders denies or undermines the realization of the other. Last, the subject receiving the message “is unable to comment [I would add, *directly within the situation*, AS.] on the messages being expressed to correct his discrimination of what order of message to respond to, i.e., he cannot make a metacommunicative statement.”³³⁵

The determinations I began with were determinations from my subject position within Human Practices. The problematic dimension of the relation between the modes of intervention hoped for and the larger problematization in which they were supposed to intervene can be understood as double binds. The constitutive situation of these double binds was not simply a single reciprocal relation between two subject positions, human scientist and bioscientist. Crucially, a third subject position, the one occupied by the NSF, was a constitutive element in the series of double binds. Thus, if a double bind is a message with two orders of communication which renders the message ‘impossible,’ we must now track three directions of message which constituted these impossibilities.

³³⁴ Gregory Bateson, Don D. Jackson, Jay Haley, and John H. Weakland, “Toward a Theory of Schizophrenia,” *Behavioral Science* Vol. I, No. 4 (1956): 251-264.

³³⁵ *Ibid.*

Direction of Message	1 st Order Message	2 nd Order Message
<i>NSF</i> <i>to</i> <i>SynBERC</i>	We want you to transform human capacities to engineer biology to increase national wealth and secure health and welfare.	Increasing human capacities to engineer biology is dangerous and so it must be done responsibly.
<i>NSF</i> <i>to</i> <i>Human Practices</i>	Prior attempts to get engineers and scientists to embed ethical problems within their research have failed. New approaches are needed in which ethical problems of emerging science are developed collaboratively.	We will not enforce the requirement for collaborative ethics if it undermines the first order message to SynBERC.
<i>STIR to Stavrianakis</i> .	Use a method for collaboration to demonstrate that it is possible.	Begin and end with the problems the bio-scientists parameterize.
<i>Human Practices</i> <i>to</i> <i>SynBERC</i>	There is a problem you are contributing to producing that needs to be thought about and requiring the cultivation of an appropriate disposition.	It is the kind of problem that you cannot solve.
<i>SynBERC</i> <i>to</i> <i>Human Practices</i>	We are willing to collaborate with you and want you to participate in our center	We are not willing or able to think about problems beyond the limits of our expertise and will enforce this limitation if you try to engage us on such problems

Since the desired mode of intervention in Human Practices was collaboration and the only terms under which STIR, SynBERC and the NSF would play the game was under the metrics of amelioration and prosperity, there was no way these project were going to succeed in a way that could contribute to what I had understood as a flourishing form of human scientific inquiry. The aim was to do inquiry into problems of discordancy which necessarily pose the question of the good. So either we had to give up collaboration or give up the metric of flourishing and the question of the human good: that was the fundamental double bind of the Human Practices experience. Hence, turning this double bind into an artifact, we decided to give up collaboration in those venues.³³⁶

I specify “we” here because I am referring to a collective subject position forged with my collaborators in Human Practices. The “we” is an ethical term. As we have shown, collaboration, even when unsuccessful in reconstructing situations, nevertheless might yet be the means through which a vocation of anthropology can be facilitated and endured. It is also the means through which the recent past is crafted and it becomes at least conceivable to participate in a near future worth observing. Together. This “we” is not only those of us who participated in Human Practices, but those that might yet participate in a “future we,” to use Foucault’s spectral phrase.

“We” wish to preserve a mode of inquiry as *Bildung*, that is to say, the search for knowledge requires subject-formative processes. We discovered that in these venues the unquestioned supremacy of method blocked these processes of *Bildung*. The choice was, to abandon *Bildung* or method. For all its tribulations I experienced collaboration as an ethical form of inquiry. *Bildung* is an essential element in this ethics of science. From this determination the only way to move forward is to refuse method and find a different venue.

The bind is, if you play this game of social science and are committed to both inquiry and *Bildung*, the situation will force you into position where *Bildung* and its observation will be methodologically rendered as opinion or value. Hence, with respect to the substance of *Bildung*, the questions are, “is this worthwhile and what do I need to do to make myself capable?” These questions are problems of inquiry for which the domination of method is discordant. Method makes a separation between knowledge and *Bildung* and once that happens a discordancy is set up which can never be overcome. It is not that the question of the good life is opaque, it is within the present, but as long as it is not connected to inquiry and not made actual, then either *Bildung* or method needs to be abandoned.

³³⁶ Rabinow and Stavrianakis, *Demands of the Day*.

Given that collaboration is blocked by the dominant metrics and *Bildung* by the dominance of method, it follows that reconstruction cannot take place within venues where these first pairs are dominant, although remediation is still possible. These artifacts of the double binds in field situations are what makes visible the need for—and the apparent impossibility of—reconstruction. Put simply, one can see what needs to be reconstructed in the situation, but the remediative mode, either as a conceptualization of problems, or as a technology of intervention under these conditions, is not adequate to the task of reconstruction.

Bibliography.

- Allison, June. *Power and Preparedness in Thucydides*. Baltimore, MD: Johns Hopkins Press, 1988.
- Anscombe, G. Elizabeth E. "Modern Moral Philosophy." *Philosophy* Vol. 33, No. 124 (Jan., 1958): 1-19.
- Aristotle. *The Complete Works of Aristotle*. Edited by Richard McKeon. USA: Random House, 1941.
- _____. *Nicomachean Ethics*. Translated by Robert C. Bartlett and Susan D. Collins. Chicago: University of Chicago Press, 2011.
- Baker, David, et al. "Engineering Life: Building a FAB for Biology." *Scientific American* (June, 2006): 44–52.
- Barnes, Barry, and RGA Dolby. "The Scientific Ethos : A Deviant Viewpoint." *Archive of European Sociology* XI (1970): 3-25.
- Baudrillard, Jean. *America*. London: Verso, 1989.
- Beck, Ulrich. "Toward a New Critical Theory with a Cosmopolitan Intent." *Constellations* 10, No. 4(March 2003): 453–468.
- Bennett, Gaymon et al, "From Synthetic Biology to Biohacking: Are We Prepared?" *Nature Biotechnology* 27 (2009): 1109–1111.
- Bergson, Henri. *Creative Evolution*. Dover Publications. 1998.
- Bateson, Gregory, Don D. Jackson, Jay Haley, and John H. Weakland. "Toward a Theory of Schizophrenia." *Behavioral Science* Vol. I, No. 4 (1956): 251–264.
- Bloom, Harold. *A Map of Misreading*. New York: Oxford University Press, 2003.
- Bloor, David. "Anti-Latour." *Studies in History and Philosophy of Science* 30, No. 1 (1999): 81–112.
- Blumenberg, Hans. *The Legitimacy of the Modern Age*. Cambridge: MIT Press, 1985.

- _____ “‘Imitation of Nature’: Toward a Prehistory of the Idea of the Creative Being.” *Qui Parle* Vol. 12, No. 1 (Spring/Summer 2000): 17–54.
- Boltanski, Luc. *L'Amour et la Justice comme Compétences. Trois Essais de Sociologie de l'Action*. Paris: Métailié, 1990.
- _____ *De la Critique. Précis de Sociologie de l'Émancipation*. Paris: Gallimard, 2009.
- Boltanski, Luc and Thévenot, Laurent. *De la Justification. Les Économies de la Grandeur*. Paris: Gallimard, 1991.
- Bozeman, Barry, and Craig Boardman. “Managing the New Multipurpose, Multidiscipline University Research Centers: Institutional Innovation in the Academic Community.” *Report, IBM Center for The Business of Government*, 2003.
- Bourdieu, Pierre. *Distinction: Social Critique of the Judgment of Taste*. Translated by Richard Nice. Harvard: Harvard University Press, 1984.
- _____ *Homo Academicus*. Translated by Peter Collier. Stanford: Stanford University Press, 1988.
- _____ *The Logic of Practice*. Translated by Richard Nice. Stanford: Stanford University Press, 1990.
- _____ *Meditations Pascaliennes*. Paris: Éditions du Seuil, 1998.
- _____ “Participant Objectivation.” *Journal of the Royal Anthropological Institute* 9, No. 2 (June 2003): 281–294.
- Boytinck Paul W. *C P. Snow. A Reference Guide*. Prentice-Hall, 1980
- Burke, Kenneth. *A Grammar of Motives*. University of California Press, Berkeley and Los Angeles, California [1945], 1969.
- _____ *Permanence and Change: An Anatomy of Purpose*. University of California Press, Berkeley and Los Angeles. 1954.
- Burke, Tom. *Dewey's New Logic: A Reply to Russell*. Chicago: University of Chicago Press, 1998.

- Caduff, Carlo. "Anthropology's Ethics: Moral Positionalism, Cultural Relativism, and Critical Analysis." *Anthropological Theory* Vol. 11, No. 4 (2011): 465-480.
- Callon, Michel. "Struggles and Negotiations to Define What Is Problematic and What Is Not: The Sociology of Translation." In *The Social Process of Scientific Investigation: Sociology of the Sciences Yearbook 4*. Edited by K. D. Knorr, R. Krohn, and R. D. Whitley. Dordrecht and Boston: Reidel, 1980.
- Canguilhem, Georges. "Machine and Organism." In *Knowledge and Life*. Edited by Stephanos Geroulanos and Todd Meyers. Fordham University Press, 2008.
- Canton, Barry Anna Labno, & Drew Endy. "Refinement and Standardization of Synthetic Biological Parts and Devices." *Nature Biotechnology* 26 (2008): 787–793.
- Cassirer, Ernst. *The Problem of Knowledge: Philosophy, Science and history since Hegel*, New Haven: Yale University Press, 1969.
- Church, George. "Let Us Go Forth and Safely Multiply." *Nature* 438 (24 November 2005): 423.
- Clark, William. *Academic Charisma and the Origins of the Research University*, University of Chicago Press, 2006.
- Collier, Stephen. "Enacting Catastrophe: Preparedness, Insurance, Budgetary Rationalization." *Economy and Society* Vol. 37, No. 2 (2008): 224 – 250
- Collier, Stephen, Andrew Lakoff, and Paul Rabinow. "Biosecurity: Toward an Anthropology of the Contemporary." *Anthropology Today* 20, No.5 (October, 2004): 3-7.
- _____. "What is a Laboratory in the Human Sciences?" *ARC Working Paper*. No. 1 (February, 2006).
- Collier, Stephen, and Andrew Lakoff "Distributed Preparedness: Space, Security, and Citizenship in the United States." *Environment and Planning D: Space and Society* 26, no.1 (2008): 7-28.
- Cook-Deegan, Robert. *The Gene Wars: Science, Politics, and the Human Genome*. W. W. Norton & Company Inc, 1994.

- Czar, Michael J, et al. "Gene Synthesis Demystified." *Trends in Biotechnology* Volume 27, Issue 2 (February 2009): 63-72.
- Daston, Lorraine, and Peter Galison. *Objectivity*. New York: Zone, 2007.
- Deleuze, Gilles. *Kant's Critical Philosophy: The Doctrine of the Faculties*. Translated by Hugh Tomlinson and Barbara Habberjam. London: Athlone, 1984.
- _____. *Nietzsche and Philosophy*. New York: Continuum, 1986.
- Deleuze, Gilles, and Félix Guattari. *Kafka: Toward a Minor Literature*. Minneapolis: University Of Minnesota Press, 1986.
- Dewey, John. *Essays on Experimental Logic*. Chicago: University of Chicago Press. 1916.
- _____. *The Public and Its Problems*. New York: Henry Holt and Co., 1927.
- _____. *Logic: The Theory of Inquiry*. New York: Henry Holt. 1938.
- _____. *Reconstruction in Philosophy*. Enlarged edition. Boston: Beacon Press, 1948.
- _____. "Propositions, Warranted Assertability and Truth." In *The Later Works of John Dewey, Volume 14, 1925-1953:1939-1941: Essays, Reviews and Miscellany*. United States of America: Southern Illinois University 1988, 2008.
- Dilthey, Wilhem. *Introduction to the Human Sciences*. Michigan: Wayne State University Press, [1883] 1988.
- Domarchi, Jean Jacques, Doniol Valcroze, Jean-Luc Godard, Pierre Cast, Jacques Rivette, and Eric Rohmer. "Hiroshima notre Amour." *Cahiers du Cinema* 97 (juillet, 1959).
- Dumont, Louis. *The German Ideology*. Chicago: University of Chicago Press, 1996.
- Edgerton, David. *Warfare State: Britain, 1920-1970*. Cambridge: Cambridge University Press 2005.

Elowitz, Michael B., & Stanislas Leibler. "A Synthetic Oscillatory Network of Transcriptional Regulators." *Nature* Vol. 403, No. 20 (January, 2000): 335-338.

Fallon, Daniel. *The German University: A Heroic Ideal in Conflict with the Modern World*. Colorado: Colorado Associated University Press, 1980.

Fassin, Didier "Beyond Good and Evil?: Questioning the Anthropological Discomfort with Morals." *Anthropological Theory* Vol. 8, No. 4 (2008): 333-344.

Faubion, James D. *Modern Greek Lessons: A Primer in Historical Constructivism*. New Jersey: Princeton University Press, 1993.

_____ *An Anthropology of Ethics*, Cambridge: Cambridge University Press, 2011.

Faubion, James, George Marcus, and Michael Fisher. *Fieldwork Is Not What It Used to Be: Learning Anthropology's Method in a Time of Transition*. Ithaca: Cornell University Press, 2009.

Favret-Saada, Jeanne. *Désorceler*. Paris: édition de l'Olivier, 2009.

Fisher, Erik. "Lessons learned from the Ethical, Legal and Social Implications Program (ELSI): Planning Societal Implications Research for the National Nanotechnology Program." *Technology in Society* 27, No 3 (August, 2005): 321-328.

Foucault, Michel. *The Order of Things: An Archaeology of the Human Sciences*. Tavistock Publications, 1966.

_____ *Discipline and Punish: The Birth of the Prison*. Translated by Alan Sheridan. New York City, NY: Pantheon Books, 1977.

_____ "The Eye of Power." In *Power/Knowledge: Selected Interviews and Other Writings, 1972-1977*. Edited by Colin Gordon, 146-165. New York: Pantheon, 1980.

_____ "The Subject and Power." *Critical Inquiry* 8, No. 4 (Summer, 1982): 777-795.

_____ "What is Enlightenment?" In *The Foucault Reader*. Edited by Paul Rabinow. New York: Pantheon Books, 1984.

- _____ “Life: Experience and Science.” In *Aesthetics, Method, and Epistemology: Essential Works of Foucault 1954-1984 Volume 2*. Edited by James D. Faubion, translated by Robert Hurley and others. New York: The New Press, 1998. .
- _____ “Des Travaux.” In *Dits et Écrits: Vol.4*. Paris: Éditions Gallimard, 2001.
- _____ «Table Ronde du 20 Mai 1978.» In *Dits et Écrits: Vol.2*. Paris: Éditions Gallimard, 2001.
- _____ *The Hermeneutics of the Subject: Lectures at the Collège de France 1981–1982*. Translated by Graham Burchell. New York: Palgrave Macmillan, 2005.
- _____ *Le Gouvernement de Soi et des Autres: Cours au Collège de France, 1982–1983*. Paris: Seuil/Gallimard, 2008.
- _____ *Introduction to Kant’s Anthropology*. Translated by Kate Briggs and Robert Nigro. Los Angeles, Semiotext(e), 2008.
- _____ *The Government of Self and Others: Lectures at the Collège de France 1982-1983*. New York: Picador, 2010.
- _____. “Polemics, Politics, and Problematizations.” In *The Foucault Reader*. Edited by Paul Rabinow. New York: Penguin, 1984.
- _____. *The History of Sexuality Vol. 1: The Will to Knowledge*. London: Penguin, [1976] 1998.
- _____. *Society Must be Defended: Lectures at the Collège de France, 1975-1976*. New York: Picador, 2002.
- _____ *The Use of Pleasure: The History of Sexuality, Vol 2*. Translated by Robert Hurley. Penguin: London, 1992.
- Geertz, Clifford “The Impact of the Concept of Culture on the Concept of Man.” In *The Interpretation of Cultures*. New York: Basic Books, 1973.
- Genette, Gerard. *Narrative Discourse: An Essay in Method*. Translated by Jane E. Lewin. Ithaca: Cornell University Press, 1983.

- Gieryn, Thomas F. "Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists." *American Sociological Review* Vol. 48, No. 6 (Dec.,1983).
- Gossman, Lionel. *Basel in the Age of Burkhardt: A Study in Unseasonable Ideas*, Chicago: University of Chicago Press, 2000.
- Graeber, David. *Toward an Anthropological Theory of Value: The False Coin of Our Own Dreams*. New York: Palgrave Macmillan, 2001.
- Graham, Beryl, and Sarah Cook. *Rethinking Curating*. Cambridge: MIT Press, 2010.
- Gregg, Melissa, and Gregory.J. Seigworth. *The Affect Theory Reader*. Durham and London: Duke University Press, 2010.
- Guston, David H. "Boundary Organizations in Environmental Policy and Science: An Introduction." *Science, Technology, & Human Values* Vol. 26, No. 4, Special Issue: Boundary Organizations in Environmental Policy and Science (Autumn, 2001): 399-408.
- _____. "Integrity, Responsibility, and Democracy in Science." *Science Policy* Vol. 1, No.2 (2001): 168-189.
- Guston, David H., and K. Keniston. *The Fragile Contract: University Science and the Federal Government*. Cambridge: MIT Press, 1994.
- Guston, David H & Daniel Sarawitz. "Real-Time Technology Assessment." *Technology in Society* Vol. 24, Nos. 1–2 (2002): 93–109.
- Habermas, Jurgen. *The Theory of Communicative Action, Vol. 1: Reason and the Rationalization of Society*. Translated by T. McCarthy. Boston, MA: Beacon Press, 1984. .
- Hacking, Ian. *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*. Cambridge: Cambridge University Press, 1983.
- Hirschman, Albert O. *Exit, Voice, and Loyalty: Responses to Decline in Firms, Organizations, and States*. Cambridge, MA: Harvard University Press, 1970.
- Ho, Karen. *Liquidated: An Ethnography of Wall Street*. Durham and London: Duke University Press Books. 2009.

- Honneth, Axel. "Democracy as Reflexive Cooperation: John Dewey and the Theory of Democracy Today." *Political Theory* Vol. 26, No. 6 (Dec., 1998): 763-783.
- Howard, Thomas Albert. *Protestant Theology and the Making of the Modern German University*. Oxford: Oxford University Press, 2006.
- Humboldt, Wilhem von. "Über die innere und äussere Organisation der höheren wissenschaftlichen Anstalten in Berlin." [1808/1809].
- Hymes, Dell, ed. *Reinventing Anthropology*. University of Michigan Press, 1972.
- Hyton, Will. "How We Are Still Not Prepared." *New York Times Magazine* October 30, 2011.
- Jameson, Frederic. *Brecht on Method*. New York: Verso, 1998.
- Jonsen, Albert. *The Birth of Bioethics*. Oxford: Oxford University Press, 1998.
- Kant, Immanuel. „Beantwortung der Frage: Was ist Aufklärung?“ *Berlinische Monatschrift* 4, 1784.
- _____*Anthropology from a Pragmatic Point of View*. Cambridge University Press, [1798] 2006.
- _____*Logic*. New York: Dover, 1988.
- _____*Critique of the Power of Judgment*. Cambridge: Cambridge University Press, 2000.
- _____*Political Writings*. Cambridge: Cambridge University Press, 2001.
- Khushf, George. "Upstream Ethics in Nanomedicine: A Call for Research" *Nanomedicine* Vol. 2, No. 4 (August 2007): 511–521.
- Koselleck, Reinhart. *Critique and Crisis: Enlightenment and the Pathogenesis of Modern Society*. Cambridge, Mass.: MIT Press, 1988 [1959].
- _____*"On the Anthropological and Semantic Structure of Bildung."* In *The Practice of Conceptual History: Timing History, Spacing Concepts*. Stanford: Stanford University Press, 2002.

- Krombach, Hayo B. E. D. *Hegelian Reflections on the Idea of Nuclear War: Dialectical Thinking and the Dialectic of Mankind*. London: Palgrave, Macmillan, 1991.
- Labacqz, Karen. *Six Theories of Justice: Perspectives from Philosophical and Theological Ethics*. Minneapolis: Augsburg Publishing, 1986.
- Lacan, Jacques. "Science and Truth." *Newsletter of the Freudian Field* 3/1–2. Translated by Bruce Fink, 1989.
- Laidlaw, James, "For an Anthropology of Ethics and Freedom." *J. Roy. Anthropol. Inst.* 8, 311–332.
- Lakoff, Andrew, and Stephen Collier. *Biosecurity Interventions*. New York: Columbia University Press, 2008.
- Latour, Bruno. "For David Bloor ... and Beyond: A Reply to David Bloor's 'Anti-Latour.'" *Stud. Hist. Phil. Sci.* Vol. 30, No. 1, pp. 113–129, 1999
- Latour, Bruno, and Woolgar, Steven. *Laboratory Life: The Construction of Scientific Facts*. 2nd edition. Princeton: Princeton University Press, 1986.
- Leavis, FR. *Education and the University: A Sketch for an 'English School.'* Cambridge University Press; 2nd edition, 1979.
- Lienhardt, Godfrey. "Modes of Thought in Primitive Society." *New Blackfriars* Vol. 34, Issue 399 (June 1953): 269–277.
- Lomask, Milton. *A Minor Miracle: An Informal History of the National Science Foundation*, National Science Foundation: Docs., U.S. Govt. Print. Off., 1976.
- Lukács, György. *The Meaning of Contemporary Realism*. Merlin Press, 1963.
- Luhmann, Niklas. *Politische Planung. Aufsätze zur Soziologie von Politik und Verwaltung* Opladen: Westdeutscher Verlag, 1971.
- _____ *Essays on self-reference*, Columbia University Press, 1990
- _____ *Observations on Modernity*. Translated by William Whobrey. Stanford: Stanford University Press. 1998.

- _____. *Risk: A Sociological Theory*. New Jersey: Transaction Publishers, 2008.
- Macintyre, Alisdair. *A Short History of Ethics*. New York: Publisher: Macmillan Pub Co, 1966.
- _____. *After Virtue: A Study in Moral Theory*. Notre Dame: University of Notre Dame Press. 2nd edition, 1984.
- MacKillop, Ian F.R. *Leavis: A Life in Criticism*. London: The Penguin Press, 1997.
- Makreel, Rudolf A. "Wilhem Dilthey and the Neo-Kantians: On the Conceptual Distinction between Gesiteswissenschaften and Kulturwissenschaften." In *Neo-Kantianism in Contemporary Philosophy*. Edited by Rudolf A. Makkreel and Sebastian Luft. Indiana: Indiana University Press, 2010.
- Mannheim, Karl. *Ideology and Utopia*. Translated by. L Wirth and E Shils. New York: Harcourt Brace Jovanovich, 1936.
- Marcus George, and James Clifford. *Writing Culture: The Poetics and Politics of Ethnography*. Berkeley& Los Angeles: University of California Press, 1986.
- Mazzotti, Massimo, ed. *Knowledge as Social Order: Rethinking the Sociology of Barry Barnes*. Aldershot: Ashgate, 2008.
- McKeon, Richard. *Thought, Action, and Passion*. Chicago: University of Chicago Press, 1954.
- _____. "Character and the Arts and Disciplines." *Ethics* Vol. 78, No. 2 (Jan., 1968): 109-123.
- _____. *On Knowing: The Natural Sciences*. Chicago: University of Chicago Press, 1994.
- Mendelsohn, Moses. "Ueber die Frage: Was heist aufklären?" *Berlinische Monatschrift* 4 1784.
- Merton, Robert. K. *The Sociology of Science: Theoretical and Empirical Investigations*. Chicago: University Of Chicago Press, 1979.
- _____. *Science and Democratic Social Structure, Social Theory and Social Structure*. Glencoe, Free Press, 1957.

- Mullender, Richard “English Negligence Law as a Human Practice.” *Law and Literature* Vol. 21, No. 3 (Fall 2009): 321-353.
- National Commission on Terrorist Attacks. *The 9/11 Commission Report: Final Report of the National Commission on Terrorist Attacks Upon the United States*. New York: Norton and Company, 2004.
- Nowotny, Helga, P Scott, and M. Gibbons. *Re-Thinking Science: Knowledge and the Public in an Age of Uncertainty*. London: Polity Press. 2001.
- Nowotny, Helga, P. Scott, and M. Gibbons. “‘Mode 2’ Revisited: The New Production of Knowledge.” *Minerva* 41, No 3 (2003): 179-194.
- Ortolano, Guy, “Two Cultures, One University: The Institutional Origins of the “Two Cultures” *Albion: A Quarterly Journal Concerned with British Studies* Vol. 34, No. 4 (Winter, 2002): 606-624.
- Pascal, Blaises. *Pensee*, Dezobry et E Madeleine, libraries-editeurs, 1852.
- Peccoud, Jean, et al. “Targeted Development of Registries of Biological Parts.” *PLoS ONE* 3, 7 (2008): e2671.
- Peters, Ted, and Gaymon Bennett. *Bridging Science and Religion*. Minneapolis: First Fortress Press. 2003.
- Peters, Ted, Karen Labacqs, and Gaymon Bennett. *Sacred Cells?: Why Christians Should Support Stem Cell Research*. Maryland: Rowman and Littlefield. 2008.
- Pickering, Andrew. *Science as Practice and Culture*. Chicago: University of Chicago Press, 1992.
- Pippin, Robert. “Avoiding German Idealism: Kant and the Reflective Judgment Problem.” In *Idealism as Modernism: Hegelian Variations*. Cambridge: Cambridge University Press, 1997.
- Plato *Plato’s Meno*. Edited by R.S. Blick. Cambridge: Cambridge University Press, 2010.
- Plutarch. *Moralia Volume 1*. Translated by Frank Cole Babbitt. Cambridge: Harvard University Press, Loeb Classical Library, 1927.

Poel, IR, and SD Zwart. "Reflective Equilibrium in R&D Networks." *Science, Technology and Human Values* (2009): 23:5–7..

Purnick, PE, and R Weiss, "The Second Wave of Synthetic Biology: From Modules to Systems." *Nat Rev Mol Cell Biol* 10, 6 (June 2009): 410-22.

Rabinow, Paul. *Symbolic Domination: Cultural Form and Historical Change in Morocco*. Chicago: University of Chicago Press. 1975.

_____ *Reflections on Fieldwork in Morocco*. Berkeley & Los Angeles: University of California Press. 1977.

_____ "Beyond Ethnography: Anthropology as Nominalism." *Cultural Anthropology* 3, No. 4 (November, 1988): 355–364.

_____ *French Modern: Norms and Forms of the Social Environment*. Chicago: University of Chicago Press, [1989] 1995 Cambridge: MIT Press.

_____ *Anthropos Today: Reflections on Modern Equipment*. Princeton: Princeton University Press, 2003.

_____ *Marking Time: On the Anthropology of the Contemporary*. Princeton: Princeton University Press, 2007.

_____ "Prosperity, Amelioration, Flourishing: From a Logic of Practical Judgment to Reconstruction." *The Journal of Law and Literature* 21, no. 3 (November, 2009).

_____ *The Accompaniment: Assembling the Contemporary*. Chicago: University of Chicago Press, 2011.

Rabinow, Paul and Bennett, Gaymon. "A Diagnostic of Equipmental Platforms." *ARC Working Paper*, no. 9, 2007.

_____ "From Bioethics to Human Practices, or Assembling Contemporary Equipment." In *Tactical Biopolitics Art, Activism, and Technoscience*. Edited by Beatriz Da Costa and Kavita Philips, 389-399. Cambridge: MIT Press, 2007.

_____ "Human Practices: Interfacing Three Modes of Collaboration." In *The Ethics of Protocells: Moral and Social Implications of Creating Life in the Laboratory*. Edited by Mark A. Bedau and Emily C. Parke, 263-290. Cambridge: MIT Press, 2008.

_____. “Synthetic Biology: Ethical Ramifications.” *The Journal of Systems and Synthetic Biology* 3, no. 1 (October, 2009): 99–108.

_____. *Designing Human Practices: An Experiment with Synthetic Biology*. Chicago: University of Chicago Press, 2012.

Rabinow, Paul, Bennett, Gaymon, and Stavrianakis, Anthony. “Reply to the Respondents.” *The Journal of Law and Literature* 21, no. 3 (November, 2009): 471-479.

Rabinow, Paul, Marcus, George E., Faubion, James, D., and Rees, Tobias. *Designs for an Anthropology of the Contemporary*. Durham, NC: Duke University Press, 2008.

Radcliffe-Brown, A. R. *A Natural Science of Society*. Glencoe, Illinois: The Free Press, 1957.

Rheinberger, Hans-Jorg. *Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube*. Stanford University Press. 1997.

Rogers, Michael. “The Pandora's Box Congress.” *Rolling Stone*, 1975.

Robbins, Bruce, ed. *The Phantom Public Sphere*. Minnesota: Minnesota University Press, 1993.

Schuurbiens, Daan. “What happens in the Lab: Applying Midstream Modulation to Enhance Critical Reflection in the Laboratory.” *Science and Engineering Ethics* 17, 4 (2011 December): 769–788.

Schuurbiens, Daan, and Erik Fisher. “Lab-scale Intervention.” *European Molecular Biology Organization (EMBO) Reports* 10 no. 5 (2009): 424-427.

Seel, Martin. “The Two Meanings of Communicative Rationality.” In *Communicative Action: Essays on Jürgen Habermas's The Theory of Communicative Action*. Edited by Hans Joas and Axel Honneth. Cambridge: MIT Press, 1991.

Seneca, Lucius Annaeus. *Moral and Political Essays*, Edited by. J. F. Procopé , John M. Cooper, Cambridge University Press; 9th Printing 2006

Shapin, Steven. “Discipline and Bounding: The History and Sociology of Science As Seen Through the Externalism-Internalism Debate.” *History of Science* 30: 333-369. 1992.

- _____ *A Social History of Truth: Civility and Science in Seventeenth-Century England* Chicago: University Of Chicago Press, 1995.
- _____ *The Scientific Life: A Moral History of a Late Modern Vocation.* Chicago: University Of Chicago Press, 2008.
- Snow, C.P. *The Two Cultures*. Edited by Stefan Collini. Cambridge: Cambridge University Press, Canto, 1993.
- _____ *Corridors of Power*, London: Macmillan, 1964
- Strathern, Marilyn. "Ars Synthetica: A Rehearsal." *BioSocieties* No. 5 (September 2010): 393-395.
- Taubenberger, Jeffery K et al. "Initial Genetic Characterization of the 1918 'Spanish' Influenza Virus." *Science*. Vol. 275, No. 5307 (1997): 1793-1796.
- Thompson, Charis. *Making Parents: The Ontological Choreography of Reproductive Technologies*. MIT Press, 2005.
- Trilling, Lionel "Science, Literature and Culture: A Comment on the Leavis-Snow Controversy." *Higher Education Quarterly* Vol. 17, No. 1 (November 1962): 9-32.
- Eric L. Trist, et al. *The Social Engagement of Social Science: A Tavistock Anthology : The Socio-Ecological Perspective*. University of Pennsylvania Press, 1997.
- Trist, Eric, & K. Bamforth. "Some Social and Psychological Consequences of the Longwall Method of Coal Getting." *Human Relations* 4 (1951): 3-38.
- Turner, R. Steven. "Growth of Professorial Research in Prussia, 1818 to 1848- Causes and Context", *Historical Studies in the Physical Sciences*, Vol. 3 (1971), pp. 137-182.
- _____ "The Great Transition and the Social Patterns of German Science." *Minerva* Vol, 25 (1987): 56-76.
- Veblen, Thorstein. "The Instinct of Workmanship and the Irksomeness of Labor." *The American Journal of Sociology*. Vol. 4, No. 2. (Sep., 1898): 187-201.
- Veysey, Laurence. *The Emergence of the American University*. Chicago: University of Chicago Press, 1965

- Voegelin, Eric. *Science, Politics and Gnosticism*. Washington: Regnery, 1968
- Warner, Michael. *Publics and Counterpublics*. New York: Zone, 2002.
- Weber, Max. *From Max Weber: Essays in Sociology*. Edited by Hans Gerth and C. Wright Mills. Oxford: Oxford University Press, 1946.
- Weber, Max. *The Methodology of Social Sciences*. Translated and edited by Edward Shils and Henry Finch. Glencoe, Illinois: Free Press, 1949.
- Williams, Bernard. *Truth and Truthfulness: An Essay in Genealogy*. Princeton: Princeton University Press, 2004.
- Williams, Raymond. *Marxism and Literature*. Oxford: Oxford University Press, 1977.
- Winnicott, Donald. "Hate in the Counter-Transference." *International Journal of Psycho-Analysis*. 30 (1949): 69-74.
- Wittgenstein, Ludwig. *Philosophical Investigations*. Translated by GEM Anscombe, Peter Michael Stephan Hacker, Joachim Schulte. 4th Edition edited by Hacker and Schulte. Oxford: Blackwell, 2009.
- Wright, Susan S. *Molecular Politics: Developing American and British Regulatory Policy for Genetic Engineering 1972-1982*. Chicago: University of Chicago Press, 1994.
- Wohlsen, Marcus. *Biopunks: DIY Scientists Hack the Software of Life*. New York: Penguin, 2011.
- Zigon, Jared. "Moral Breakdown and the Ethical Demand: A Theoretical Framework for an Anthropology of Moralities," in *Anthropological Theory*, vol.7, no.2 (2007): 131-150.
- Zimmer, Carl. *Microcosm: E. coli and the New Science of Life* Pantheon Press, 2008.