Situated Embodiments: A Latourian Interpretation of Transcultural Psychiatry in Montreal

Nathan Ferguson

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By: Nathan Ferguson

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Signed by the final Examining Committee:

Marc Lafrance, Chair
Julie Soleil-Archambault, Examiner
Kregg Hetherington, Examiner
Marc Lafrance, Supervisor

Approved by Mark Watson, Chair of Department
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Abstract

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This thesis involves an ethnography of three field sites related to the scientific discipline of transcultural psychiatry — sites representative of its classroom, laboratory, and clinical activities. This psychiatric discipline, which incorporates conceptual and methodological elements of anthropology, holds that *cultural variability* and *social positionality* are important factors in illness and wellness. As such, the field disrupts both the materialist notions of the body dominant in contemporary biomedicine, and the cognitive explanatory models of conventional psychology. Despite this alternative approach to health and healthcare, transcultural psychiatrists in Montreal continue to position themselves as scientifically credible and medically authoritative.

My research project asks how the role and status of the body in medicine is affected in the context of such a field, which openly troubles received divisions between subjectivity and objectivity, nature and culture, social and material reality, and physiological or psychological stress. In particular, I consider the *diagnostic attitudes and practices* of transcultural psychiatry, which I articulate as an attempt to give name to *culturally-situated forms of embodied disorder*. By introducing ethnographic consultation into the diagnostic process, transcultural psychiatry opens up these scientific names to a new kind of participatory construction. This being the case, the diagnostic practice of the discipline consists of naming otherwise, or naming differently.

To my friends —

you are the source of my dedication.

The beginning of wisdom is to call things by their right name…

— Confucius, *Analects* XIII

Foreign substance only thing that keep the stress off my face…

— Da$H, *Blue Fin Tuna*

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List of Abbreviations

Author, *Title*	*Abbreviation*
Bruno Latour, “How to Talk about the Body?”	*TB*
Bruno Latour, *We Have Never Been Modern*	*NM*
Bruno Latour, “Where Are the Missing Masses”	*MM*
Bruno Latour, *Science in Action*	*SIA*
Bruno Latour, “Give Me a Laboratory and I will Raise the World”	*GM*
Didier Fassin, *Les mondes de la santé publique*	*SP*
Didier Fassin, *L’éspace politique de la santé*	*EP*
Laurence Kirmayer et al., *Culture, Mind, and Brain*	*CMB*
Laurence Kirmayer et al., *Cultural Consultation*	*CC*

Introduction

In this thesis, I present an interpretation of the role and status of the body in science. Specifically, I work to describe and analyze one particular field of body-science, through an ethnography of its seminar, laboratory, and clinical spaces. This is the field of transcultural psychiatry, which I study as it exists in Montreal. Fundamentally interdisciplinary, this field emerged out of a mid-century exchange of methods and values between social psychiatry and medical anthropology. My approach to ethnographic inquiry is also interdisciplinary — grounded in the contemporary science studies of Bruno Latour, I work to build an account of transcultural psychiatry that gives as much attention to its technical or technological elements as its social or sociological dimensions. Part of the work of this introduction, then, will be to sort out some of the questions and problems that arise from this technically-oriented anthropology of a culturally-oriented medicine. In other words, before we get started on this ethnographic excursion, it will be necessary to reflect on the conditions of an immersive inquiry into the technical and literary culture of a medicine which, as it happens, also appropriates and refashions elements from the literary techniques of cultural studies. Finally, we will also have to come to terms with the ways in which this analysis seems to participate in the game of scientific literature — a game which consists of giving new names to objects, and then constructing disciplines around their use.

The first thing to do with these questions in mind is to define Latour's concept of *symmetrical inquiry*: in his analytic vocabulary, *symmetry* refers to those forms of inquiry which hold together the human and nonhuman features of a given object of study (*NM* 42). In other words, both my practice of research and that of my participants is *symmetrical* in nature: I apply ethnographic methods to study the material conditions of scientific action; they apply scientific

methods to study the cultural conditions of mental health. It should not be surprising, then, if both forms of inquiry converge around a similar kind of question. In the context of this project, this is the question concerning naming: how to diagnose a problem, and how to use diagnosis.

From both perspectives — that of symmetrical anthropology as well as symmetrical psychiatry — scientific names function as the point at which 'nature' and 'culture' most clearly interact or inform each other. For medical anthropology, the diagnostic name is the object at the center of psychiatry's intensively literary culture: psychiatrists, that is, are always arguing with each other, through the medium of scientific literature, about how to use names, or which names to use. For transcultural psychiatry, the diagnostic name is the fundamental point of tension between physician and patient, but also between one psychiatric culture and another: names which make sense in one context fail to materialize in another; and different kinds of patients reject, resist, or reify a given diagnostic category in their own ways, thereby modifying its function as a vector of scientific authority and as an object of scientific reality. Under the gaze of such symmetrical inquiries, the body — as the site of that disorder which is named by psychiatry, and as the source of those symptoms which are organized behind a given diagnostic name — is both physical and cultural, materially-constituted and socially-organized. The work of this thesis is to follow scientists around as they enact these processes of giving name to the body.

Part 1. Context

Montreal is one the global centres of knowledge-production in transcultural psychiatry (Kirmayer 14; *SP* 262). The textbook *Cultural Consultation*, coedited by members of McGill University's Department of Social and Transcultural Psychiatry, argues that the history of cross-

cultural psychiatric practices can be mapped on to different periods and patterns of global migration (*CC* 7). An important part of the context of my fieldwork, then, is the evolving response in Montreal to the fact of its cultural diversity. According to the editors of *Cultural Consultation*, the institutional approach to this fact within the Quebecois health care system has historically been the promotion of "interculturalism" — an ethical and political commitment to cultural encounter, oriented around "vibrant exchange in which new cultural forms are created" (8). It is in this social and moral economy of migration that the kind of cultural humility essential to the practice of transcultural psychiatry must be understood: human culture, according to this model, is not a set of static and distinct categories that exist independently of their embodiment; rather, culture is a continuously emerging matrix of different ways of life, which actively inform each other, and are structured by the social and material conditions of a given local context.

At stake in transcultural psychiatry, in other words, is a committed de-essentialization of culture, in favour of an attunement to cultural variability. If our culture is something that we are always performing and enacting in different ways, then a culturally-informed medicine cannot strive for some kind of exhaustive catalogue of specific cultural conditions or disorders. The ground of transcultural psychiatry is always shifting, because the unique bodies of its patients are continuously finding new ways to recite cultural norms and respond to cultural pressures. This principle involves a fundamentally different approach to research than conventional biomedicine: there is no 'answer' to the question of transcultural psychiatry, or no 'cure' to the problems it treats, because culturally-situated forms of distress cannot be neatly mapped or entirely captured, as if from some stable point of view that exists outside of the uniquely-encultured body. This is, by definition, an unstable science — a study of change and of difference.

One of the most influential proponents of this anthropological principle in the context of psychiatric medicine is Arthur Kleinman, who in 1977 identified the "*category fallacy*" of "the old transcultural psychiatry" (Kleinman 4). According to Kleinman, the category fallacy arises when a psychiatrist assumes that the diagnostic taxonomy from their own medical culture will necessarily work well in a different cultural environment. Against this diagnostic universalism, Kleinman argued for a "renewed emphasis on ethnographic research" in the field (Kirmayer 10). The special feature of transcultural psychiatry after Kleinman, then, was not to be an extensive and stable taxonomy of culturally-bound symptoms or syndromes, but rather an emergent and dynamic skill of becoming sensitive to culturally-variable contexts of suffering and healing.

Indeed, in recent years, many practitioners in this field have come to refer to it simply as cultural psychiatry (Kirmayer 4). This renaming reflects a shift in the understanding and approach to mental health across different cultural contexts. While 'transcultural' refers to the interaction and exchange between cultures, cultural psychiatry emphasizes the importance of understanding how culture always already influences mental health and psychiatric disorders, without necessarily implying a distinct 'crossing' of cultures. However, because the name of the institution where I carried out much of my fieldwork preserves the transcultural qualifier, I have opted to do so as well throughout this thesis essay — I began my fieldwork at McGill's Division of Social and Transcultural Psychiatry, and it was through the various academic programs of this division that I encountered and recruited my participants in laboratory and clinical contexts.

Through my immersion at these closely-related sites, I worked to define the specific environmental affordances of the laboratory and the clinic, particularly with respect to the

activity of giving name to the body that takes place in these spaces. The psychiatric concept or shared object of interest that links these three specific sites of scientific action — despite their somewhat different orientations in terms of research and practice — is stress.

A concern of increasing importance in public health and biomedicine, stress was named by the World Health Organization to be the major "health epidemic of the 21st century" (Fink 3). The contemporary science of stress, as it were, has deep roots at McGill University. In 1956, just one year after its division of transcultural psychiatry was founded, the McGill researcher Hans Selye first published *The Stress of Life* — a popular science text which gave public name to his concept of the biological "stress response," and which further solidified the "hypothalamus-pituitary-adrenocortical axis" as the system of the body responsible for mental stress (125; xii). Selye's model — which describes both the internal mechanisms of somatic stress, and their own causal agency in a number of important diseases — situated stress in relation to the most active research programs of contemporary medicine. By identifying stress as a "common denominator of biologic activity," his program proposed to establish "tenets for a new kind of medicine" (397) — one which was to be fundamentally "psychosomatic" in nature (405).

Neither entirely physiological nor simply psychological, stress is clearly an interesting object of study for a symmetrical science such as transcultural psychiatry. The psychosomatic dimensions of stress are at the center of its laboratory research programs: in these spaces, transculturally-minded researchers work to settle the controversies of hormonal chemistry and neurobiology that stand in the way of a straightforward account of the physical stress system, so as to deepen our understanding of how social stigma inscribes itself on human bodies. By studying the physical traces of social injustice, laboratory technicians use the literary concept of

stress to make arguments about the differential prevalence of disease and distress in different communities. In the clinical context of transcultural psychiatry, the "emergence of diagnostic categories like post-traumatic stress disorder" demonstrates a further solidification of Selye's stress science: to the extent that psychiatry now uses the "biological" language of stress to describe what was previously the exclusive domain of "pscyhoanalysis and other forms of psychotherapy," the discipline participates in a 'new kind of medicine' indeed (Kirmayer 11).

As an ethnographer of science, I entered the field in the context of these intersecting histories: on the one hand, a social economy of interculturalism, represented by medical institutions and infrastructures dedicated to contextually-sensitive forms of healthcare, and in urgent need of responding to new realities (or even 'crises') of global migration;[1] on the other hand, an interdisciplinary literature of stress, as a concept credible and capacious enough to link physiological mechanisms and psychological emotions with social pressures and cultural sensitivities. As the diagnostic categories of stress persist in importance in the field of psychiatry, they are eventually "reified and institutionalized as cognitive and social facts" (Kirmayer 11). My thesis, in other words, consists of an ethnography of these social institutions of stress science, and involves tracing their ongoing process of reification — from the initial scientific activities of instruction and division to the facts, names, and realities that result. Ultimately, it will be my argument that the symmetrical approach to stress at work in this 'new medicine' promises to embody the kind of scientific responsibility called for by Latour in his articulation of symmetrical anthropology — that its practice of naming, in other words, works well.

[1] In October 2024, as I concluded my fieldwork, the Quebec government announced new restrictions on migration, in the context of a growing politicization of cultural integration in the province (Ministry of Immigration, 2024).

Part 2. Content

Before properly discussing what *is* in my thesis, I need to take some time to discuss what *isn't*, because there are a few important angles of approach on my subject matter that you won't find in the chapters to come. First, I'll discuss some basic exclusion criteria in the design of my ethnographic research, explaining why I choose to foreground the perspectives of certain participants in these spaces over others. Then, I'll describe the exclusion criteria of my literary research, identifying the other bodies of academic knowledge which might be used to treat the material that I've collected here. Finally, once I've clarified the scope and thrust of my particular approach, I'll outline the content of this essay on its own terms.

The three scientific spaces which I studied are all home to a certain kind of two-sided encounter, constituted by a distinct difference in authority that exists between the counterparts. In the classroom, for example, the authority rests with the instructor, though it is only sustained by the continued interest and credulity of the students. In the clinic, a similar relationship structures the encounter between physician and patient. Authority in the laboratory is more internally-stratified, insofar as junior researchers are subordinate to principal investigators — and the relation between these theoreticians and the technicians who actually operate the laboratory equipment has its own famous complexities (*LL* 16). Yet all these internal articulations exist in the context of a basic separation of authority between the researchers and those who are researched — their participants, or, more starkly put, their 'subjects'. In this study, I describe the attitudes and activities of those with the authority: the directors of scientific action.

The perspectives of the student, the patient, and the participant are thus conspicuously absent from my observations, interviews, and analysis. Similarly absent are certain narrative details about the personalities and motivations of my own research participants — this is not an anthropology of individual scientists, but rather an anthropological inquiry into the activity of science itself. What is of interest to me, in other words, is not the interpersonal texture of these scientific spaces. Instead, I turn my attention to the ways in which a specific literary culture of naming both gives shape to and is shaped by the set of social and technical disciplines that emerge out of these spaces — disciplines which, through the institutional infrastructure of contemporary medical science, remain economically linked to that initial form of literacy.

This more structural orientation of my research is by no means informed by a lack of appreciation for more person-centred ethnographies of medicine. Indeed, such works are foundational to the very field of transcultural psychiatry: ethnographies of the clinical encounter feature prominently in texts such as Arthur Kleinman's *The Illness Narratives* (1988). In the Canadian context, Lisa Stevenson's *Life Beside Itself* (2014) or Margaret Lock and Nancy Scheper-Hughes' "The Mindful Body" (1987) both exemplify the importance of patient-oriented narrativity to strong medical anthropology. The more structural approach for which I have opted in this project is simply one methodology that exists among others for clarifying and critiquing the culture of science. By focusing on the technical processes by which names are given and facts are constructed, I endeavour to illuminate different aspects of this culture than would feature prominently in an inquiry into the personal experiences of scientists or their counterparts. The intention of this choice is to treat these spaces as case studies for a philosophical argument about scientific action: that our names for the body also change how the body works.

Building this argument requires the careful elaboration of Latour's philosophy and anthropology of science. This being the case, much of my own research proceeds from his methodological principle that scientific authority is less about its *content* than its *form*. For Latour, in other words, science isn't convincing because its facts are close to nature; instead, its facts seem close to nature because it is convincing. In a Latourian interpretation of science, understanding the specific claims of a research program is less important than understanding the disciplinary microprocesses and literary context of the research itself. Only after this structural ethnography is complete does it make sense to open up and analyze in detail the actual arguments or claims of a scientific field. It is on the basis of this same methodological principle that I exclude certain academic literatures from my conceptual framework.

To be more specific, my laboratory and clinical field sites involve participants from two 'populations' or communities which constitute or serve as objects of study for distinct research programs in the social sciences. At my laboratory field site, I worked on an experimental project studying the physiological stress of Montreal's Queer community; at my clinical site, many of the case conferences in which I participated concerned refugees seeking asylum in Quebec. The broad academic fields of gender studies[2] and migration studies[3] are rich in their own resources for an ethnography of such spaces. These important literatures clearly and persuasively establish the exceptional sociological and semiotic status of the communities in question.

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[2] Especially since the first volume of Michel Foucault's *History of Sexuality* (1976), the question of a *scientia sexualis* has been at the forefront of a growing literature on the institutional study of Queerness. Texts such as Emily Martin's *The Woman in the Body* (1987), Judith Butler's *Gender Trouble* (1990), and Anne Fausto-Sterling's *Sexing the Body* (2000) have all contributed to this critical thematization of sexuality as a scientific object.

[3] This field's canon can be traced back at least as far as E.G. Ravenstein's sociological essay on "The Laws of Migration" (1885). Hannah Arendt's *On the Origins of Totalitarianism* (1951) offers an early account of modern statelessness. Contemporary touchstones include Castle and Miller's *The Age of Migration* (1993); Schiller, Basch, and Blanc's *Towards a Transnational Perspective on Migration* (1992); and Sassen's *Guests and Aliens* (1999).

In my project, I put these spaces in service of a different kind of ethnographic inquiry. Because my interest is in the structural analysis of scientific literacy, the particular content or subject matter of the projects in question is of secondary importance: in this framework, the sociological status of these communities is to be treated as the consequence, more so than the cause, of scientific action. In other words, I integrate these communities of interest into the Latourian framework as non-exceptional — this allows us to trace the transformation of the material involved on its own terms, as an effect of scientific efforts to construct credible facts and stable names. We will thus still arrive at an understanding of the specific social and semiotic status of Queerness and of migration, but from the point of view of scientific action, rather than that of sociological critique. This is not to deny the important particularities of these sociological categories, but rather to demonstrate that part of what grounds all sociological categories are the laboratory and clinical activities through which they are constructed and operationalized.

As I will argue below, much of the work of science involves the inheritance and rearticulation of conceptual taxonomies. I choose to not 'mark' these communities as inherently 'different' or 'other', so as to suggest the extent to which these taxonomies of the sociological literature are always open to radical reinscription and rematerialization. Similarly, I choose to not approach these scientific encounters primarily through an analysis of their power dynamics, in order to create more space in the ethnographic frame for the technological infrastructures and nonhuman agencies that mediate this literary relation between researcher and researched. It is my argument that a more initially-neutral interpretation of diagnostic naming does not undermine any later critique of science and technology. By allowing for other elements of the field site to come to the forefront, this lens simply shifts our critical attention in a different direction.

I organize my argument into seven chapters. In the first chapter, I draw on some famous debates from the philosophical canon to identify the most salient questions of an inquiry into scientific namegiving; I also indulge in some etymological wordplay, gesturing towards the ethical concerns at stake in an ethnography of transcultural psychiatry. Following this little trick (which is also a treat, which is also a trap), I proceed to a number of important definitions in the second chapter, intended to help orient the reader in this strange symmetrical landscape. The third chapter is the longest of the essay, and it consists of a conceptual framework that reads the critical medical anthropology of Didier Fassin into the critical science studies of Bruno Latour. This synthesis is, as far as I know, a unique one, and it constitutes one of the major contributions of this project. In the fourth chapter, I review the materials and methods of my study, and discuss some of the specific ethical problems involved in researching researchers. The fifth chapter presents an extensive document analysis of the scientific literature in transcultural psychiatry and psychosomatic stress — it is at this point that I begin to apply the Latourian framework to actual materials from the field. In the sixth chapter, I present my ethnographic findings from the classroom, the laboratory, and the clinic, arguing that all three sites involve a specific kind of encounter that can be clearly understood in Latourian terms. The seventh and final chapter introduces the work of Ian Hacking, a Canadian philosopher of science, whose account of diagnosis helps to explain the nature of this peculiar encounter at the heart of my ethnography.

There are a few quirks to this outline that are worthy of brief consideration — both connected to the relatively late introduction of certain features of my argument. To begin with, it requires acknowledgement that I do not begin extensively treating the actual ethnographic

material from my field research until the fifth chapter, almost halfway through the essay. This structural feature of the project results from the careful attention with which I develop the Latourian conceptual framework in the third chapter: this body of work is, to put it plainly, terminologically idiosyncratic — Latour's methodology for science studies involves a unique lexicon, and drawing out the conceptual implications of this vocabulary for the unfamiliar reader is, it turns out, a laborious process. But this labour of love, I argue, bears its fruits throughout the later chapters, as it provides a robust frame in which my Fassinian critical anthropology of the clinic can be clearly and concisely tied to the social construction of scientific literacy. Later on, well after the conceptual framework, I introduce Hacking's work, in the essay's final chapter. The reason for this deferred introduction of a conceptual resource is twofold: first of all, although Hacking's account of diagnosis is helpful to an *interpretation* of my findings, it isn't strictly necessary to the *presentation* of this material; this being the case, and in light of the already lengthy conceptual groundwork, I opted to reserve his work until it was to be put into use.

The key resources for my conceptual framework are those of Latour and Fassin. As I suggested above, the use of these two anthropologies of science together constitutes a unique approach to the information economy of medicine. The practice of symmetrical anthropology, at least in the specific terms by which Latour called for it, has not yet given rise to a clear and distinct research program in the literature or in the field. The originality of my thesis consists of the development and deployment of this analytic lens; I argue that the insights afforded by this unique approach justify the length and density of the work itself. With that said, I want to bring my introduction to a close by acknowledging some of the intellectual debts this project owes to its precursors in the sociotechnical analysis of science, and in the critical study of the body.

Although few explicitly-named symmetrical anthropologies of medicine exist in the disciplinary literature, there are a number of important works in the field of science and technology studies which provide a distinctly Latourian analysis of scientific literacy. Just two years after Latour published *Laboratory Life*, Karin Knorr-Cetina authored her seminal text on the social construction of science, *The Manufacture of Knowledge* (1981), which draws heavily on the Latourian ethnographic framework. At the end of the decade, Donna Haraway published *Primate Visions* (1989), an immersive and discipline-specific study of primatology, which further concretized the study of scientific objectification. Four years later, Beryl Harrison's *The Body in the Laboratory* (1993) was released. Although her analysis focuses more on gendered bias, and the sociocultural processes by which it affects the construction of scientific facts, it remains an important antecedent to my analysis of the body as an object of laboratory attention. John Law's *Aircraft Stories* (2002) reestablished the importance of semiotic analysis into the ethnography of technoscience, making the argument that the technical apparatus of a scientific workspace cannot be understood without attention to the way in which it structures laboratory conversations and social conventions. More recently, a pair of Brazilian scholars published their analysis, "The socio-technical network of a high-school chemistry laboratory under the Latourian perspective" (2021), in the journal of *Cultural Studies of Science Education*. This text carries out a very similar kind of symmetrical interpretation of scientific action as my own project.

Beyond these more closely-related contributions to science studies, a number of other interdisciplinary sources gave important direction and contour to my attitude as an interpreter or critic of scientific culture. The first thread of academic research I have in mind belongs to the field of critical body studies, which influenced my treatment of the human body as an object of

discourse. Texts like Elizabeth Grosz's *Volatile Bodies* (1994) helped me become sensitive to the inherent non-neutrality of the body as a site of research and resistance. Jane Bennett's *Vibrant Matter* (2009), in the highly symmetrical tradition of the new materialisms, helped me to imagine what a political ecology of scientific instruments might look like. Much of this research project is inspired by my engagement with Bessel van der Kolk's *The Body Keeps the Score* (2015), which carefully catalogues the plurality of therapeutic interventions that continue to emerge around the problem of post-traumatic stress. By exploring the intersections between the nonhuman agencies of the physical body and the human social formations that articulate themselves around it, these texts formed the sensitivities that I sought to refine and bring into action in my own work.[4]

For an orienting metaphor of the kind of sensitivity to which I'm referring, consider the grammatical structure known as the middle voice. Neither active nor passive, sentences constructed in the middle voice present situations in which the grammatical subject is somehow both the agent and the recipient of some environmental action. It is in this kind of grammar that we will have to imagine the role and status of the body in science: not just an object, not fully a subject; not the only agent in the laboratory, far more than a mere patient in the clinic. The study of environmental affordances, for example, gestures to this way in which the body both changes and is changed by the world in which it finds itself; this kind of symmetrical inquiry, I argue, belongs in the middle voice. Learning to think in the middle was one of the guiding principles of my ethnography, and is perhaps the central proposition of this essay. For help in this endeavour, we will now turn to one of the most famously middle-voiced languages — Plato's Attic Greek.

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[4] Annemarie Mol's *The Body Multiple* (2003), which I discovered only after completing my own essay, performs a similar ethnography of diagnosis with respect to the disease of atherosclerosis. Putting my work into conversation with Mol would be a strong direction for future refinements to my research. E. A. Wilson's *Gut Feminisms* (2015) also presents a promising avenue for further considerations of scientific research as relevant to cultural critique.

Chapter 1. Parable and Poetics

Part 1. The Parable of the Namegiver

Why don't we begin, then, with a kind of parable to set the tone for this thesis, and to start opening some of the major questions of an inquiry into the conceptual and technical environment of transcultural psychiatry in Montreal. This might be a useful way to establish the themes that animate both the Latourian project of working with scientists — of participating in and coming to understand the nature and culture of their work — and the project these scientists of psychiatric medicine themselves carry out, as they try to bring some order to the cultural dimensions of physiological distress. But what might serve as an orienting parable or psychomyth for a Latourian program of the anthropology of science? Ultimately, this program casts the scientific work of *describing nature* as an inherently *culturally-inflected* activity, binding up the stated goal of science — pursuing empirical facts — with the social forces and habits that, in practice, influence or determine the course of this pursuit. In this sense, Latour's analysis of the production of scientific laws involves introducing an attention to local conventions and contingencies, grounding the rational or factual authority of the laboratory (and all of its literary products) in the less clearly rational reality of those social and economic relations which constitute and operationalize it. This being the case, I propose as a relevant drama Plato's elegant figurative solution to one of the central questions of the philosophy of his time — namely, the question of the natural correctness of the law.

As Plato wrote the later dialogues of his philosophical career, the prosperous Periclean era of Athens' so-called first democracy was in a rapid decline. The optimistic attitude of a firm belief in the rational power of Athenian political subjects gave way to an atmosphere of anxiety

and uncertainty; how was it possible to be sure, the Athenians asked themselves, that their laws were correct and true, in the midst of the successive plagues and military defeats of the era? At stake here was the ability to come up with laws that would guide the city in accordance with the divinely-ordained exigencies of natural, external necessity.

Adding to this political and philosophical anxiety was the rise of a new discipline of rhetoric practiced by a group popularly known as the sophists. Unlike the legendary orators of the democratic period, who seemed to the Athenians to speak with the voice of nature itself — guiding the political will of the city, as it were, through appeals to a shared intuition of correct ethical action — the sophists developed techniques to force their listeners into agreement through the abuse of language (*Sophist* 248a). Turning language into a tool of coercion allowed the sophists to call into question the ethical or habitual conventions of Athenian life which had, until then, been accepted as the right and natural way to live (Sedley, 2003). It is in this historical context of confusion — a socially-dispersed anxiety about the natural correctness not only of political law, but the trustworthiness and rational status of language itself — that Plato wrote the dialogue *Cratylus*, on the question of language as a shared convention, and the famous 'late trilogy', on the possibility of a politics in true accordance with nature.

In these strange and brilliant narrative works, Plato introduces and elaborates the dramatic figures of the nomothete — meaning 'the giver of the name' (*tò ónoma*) or 'the giver of the law' (*ho nómos*) — and the dialectician — that person who puts the linguistic or legal structures of the nomothete into action, so as to discover their affordances and uses, reveal their limits and flaws, and, in so doing, fine-tune their agreement with the natural pattern of reality. The ability to apprehend patterns in the world and externalize those patterns in logical speech is

perhaps the faculty of the human mind with which Plato was the most intensely fascinated in general. In his famous drama of the cave, for example, the upwards motion towards higher degrees of truth is always complemented by a descending return, upon which these truths are made articulate to those still trapped below (*Republic* 250a); this dialectic of contemplation and articulation is probably the fundamental movement of Platonic philosophy. Across the arc of the later dialogues, Plato focuses less on this internal dialectical journey of the individual philosopher, and turns his focus towards how this dialectic might function on the scale of collective social or political efforts such as the use of language, or the establishment of legislation. In writing of these quasi-philosophical efforts — where clear contact with eternal and natural truths seems less assured, and a rootedness in irrational and transient human conventions seems more inescapable — Plato introduces our two figures.

One of the most famous appearances of the nomothete and the dialectician is in the *Cratylus* dialogue, which features a debate between two Athenians — Hermogenes, who believes that language is completely arbitrary and conventional, and Cratylus, who defends the position that all names are natural and correct. Plato has Socrates relieve this philosophical tension by shifting the perspective of the conversation: according to him, individual words are not the source of the natural correctness of language — in fact, he rather agrees with Hermogenes that the individual words themselves have little utility, and even less inherent meaning. Instead, language is correct to the extent that its vocabulary includes names for all kinds of different things; and more importantly, *in their use*, names enable a dynamic representation of the many relations that these things have with each other. To explain both of these properties of language, Socrates provides a sort of mythical figure, characterized by their ability to internalize patterns of

nature, and then externalize those patterns into linguistic conventions in an orderly fashion. The nomothete is the one who comes up with the words: in giving names, they need to able to apprehend correctly the natural divisions between different things, and then externalize those divisions into the phonetic material of their names. The dialectician takes up this vocabulary and puts into action, according to their apprehension of how these named things exist *in their natural relations to each other* (*Cratylus* 435c). Through this double movement of internalization and externalization, and to the extent that it is done properly, Socrates affirms that the philosophical use of language does in fact afford the ability to correctly express the order of the world.

Here, the most relevant feature of Socrates' account of this naming activity is his argument that *it keeps getting better*, through the ongoing use of its names in philosophical conversation. Language, that is, has a continual or progressive aspect; this, in other words, is its dialectical quality. For Plato, the *structure* of language — its more or less stable vocabulary and formal grammar — stands between divine nature and human convention, because the namegiver never does a perfect job: neither in his apprehension of nature, nor in his ordering of the names. (*Cratylus* 383b) The social and cultural activity of its *use* also occupies this middle space, since the full correctness of a shared language is always distorted through its different interpretations, and its informal uses by non-philosophical speakers (*Cratylus* 390a). For Plato, languages bear the weight of their histories, and historical misuses of a given name come between speakers and the truth of things. The role of the dialectician is thus crucial, because they are able to resist these conventions, and bring the pattern of language closer to the pattern of nature. In other words, the work that the nomothete begins when they come up with their taxonomy of names is open-ended: open to being disrupted or distorted by certain social forces, but also to being carried

further by others in the active work of careful conversation. This possibility to restore or improve the use of names is the basic point of departure for the Socratic method as Plato develops it in his mature dialogues, which almost always begin with Socrates asking some version of the question, 'when you say this thing, or use this word, what is it that you actually mean?'

If we can take these concepts from this parable — the ongoing relation between the given taxonomy of names and its reception, interrogation, and adjustment by critical users;[5] the sense that things can only be meaningfully captured through their dynamic relations; and the difficulty of completely rescuing these efforts of representation from social forces — then we are in a happy position to set off on a Latourian inquiry into transcultural psychiatry. As we do move forward, the metaphysical issues of this figurative drama will recede in importance — certainly, it is not the goal of this thesis to determine whether or not there is a stable reality of essential forms, to which language might dynamically refer its users. Nevertheless, the general picture of the continual inheritance and use of a functional taxonomy of names is clearly not too far off from the daily reality of scientists at work, as they deploy and transform theoretical categories through the crucible of the laboratory — an environment which, I will go on to argue, is highly conversational, and which involves careful collective efforts to pin down the correct way to use terms that have been muddied by convention. Similarly, it will be useful to think through this scientific effort as another double movement of division and relation: while the laboratory affords a tremendous power to refine physical material into 'purer' forms and taxonomies, this

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[5] I have focused on their linguistic activities here, but the same structure holds in Plato's political deployment of these figures. The lawgiver writes the laws, and the dialectician is the ethical citizen who embodies them. Importantly, the dialectician does not identify with the letter of the law, but rather with its spirit: should the legal conventions of his time not accord with the dialectician's internal sense of the good life, they will act against them and work to change them (*Statesman* 294). It is not mere historical trivia that Socrates — Plato's friend, mentor, and model for the dialectical critic — was put to death for his resistance of Athenian laws and social conventions.

stabilizing work of isolation is always followed by bringing these laboratory entities into new theoretical and technological combinations. The objects of science only become socially meaningful and practically useful — which is to say, they only become interesting — through the connections and alliances made possible in this second movement of the scientific activity.

Part 2. The Poetics of Transcultural Psychiatry

At this point, I will make use of one more alternative literary technique to build out the aesthetic and conceptual climate of this thesis. This will involve a short poetic analysis of the main subject of the essay, which is the field of transcultural psychiatry. How do I justify such an indulgence? First, I consider the work of ethnographic anthropology to be partly narrative; that is, ethnographic writing tells the story of how the anthropologist encountered and came to understand a culturally specific space. This kind of poetic interpretation simply does form a significant part of my encounter with this field — both the labs in which I worked, and the texts with which I engaged. On this basis alone, I think its inclusion here is merited. But to go further, I also want to argue that the inclusion of this kind of analysis represents the seriousness with which I take the idea — mentioned first in my introduction as a crucial claim of transcultural psychiatry itself, and to be developed further in my section on definitions as an integral element of the Latourian anthropology of science — that the body is a sensitive and trustworthy instrument. Anthropological inquiry in the spirit of thick description involves the multifaceted interpretation of a given environment; as embodied interpreters of science, we are lucky enough to be equipped with an elaborate register for symbolic meaning. Let us be brave, and use all the heuristic tools we have at our disposal, poetry as well as any other.

I will structure my exploration of transcultural psychiatry's poetics as a kind of homage to Cratylus, the humiliated etymologist of Plato's dialogue on language. Cratylus supported the somewhat ridiculous argument that the meaning of words derives from the information carried by the phonetic components of individual names.[6] His rhetorical tactic in the dialogue was to break down terms into their constituent sounds, and explain how those sounds worked together to give the true sense of a name. I can't follow him all the way — Socrates' refutation of this elemental definition of language was simply too successful — but I will imitate his style, and provide a little meditation on the four etymological parts of the name for this young science called transcultural psychiatry. If it isn't rigorous, I promise it will at least be fun. Like the fabled deconstructors of the tower of Babel, who had to work their way down from the top to get to the lost knowledge at the bottom, we will begin with *psychiatry* before moving on to *transcultural*.

The name for psychiatry, like most of our sciences, is an enlightenment-era portmanteau of two Greek words, *psukhḗ* and *iatrós. Psukhḗ* is a term which is slightly hard to pin down in the classical Athenian philosophy, but is usually translated as 'soul' with a secondary meaning more specifically like 'mind'.[7] The major classical text on the subject of this 'psyche' comes to us from Plato's most famous student: it is Aristotle's *Peri Psukhēs*, commonly known to us as *De Anima*, or *On the Soul.* In Aristotle's systematic philosophy, the soul is the governing principle of an organism — the source of its various powers and the unity of its activities. For Aristotle, the body is not distinct from the soul, but in fact its most straightforward expression (*De Anima* 410b); a body engaged in activity is how philosophers can apprehend the soul 'being fully

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[6] Seriously, someone needs to get this *idiṓtēs* a glass of H_2O so he can sober up.

[7] The translations for the Greek here and elsewhere in the thesis are taken from Henry Liddell and Robert Scott's *A Greek-English Lexicon,* the authoritative source for translation between English and Plato's Attic Greek dialect.

itself'.[8] *Iatrós,* meanwhile, is the Greek word for 'doctor'. A fundamentally interdisciplinary figure — one who drew their authority from religious expertise as much as technical skill — the *iatrós* was embedded in powerful social institutions; they practiced the healing arts through the prescription of behaviours as well as drugs, intervening in the ills of the body through surgery as well as spiritual intercession. The origin of the term is in the verb *iasthai*, meaning 'to cure' or 'to heal', which also has a light connotation of bringing something to fullness (*Symposium* 191d). Without too much violence to the Greek, then, we can poetically frame psychiatry as a social and institutional art with spiritual and technical dimensions, fundamentally oriented towards bringing the human soul to a greater degree of fullness, or of bringing the human organism to a fuller breadth of embodied possibilities and activities.

What poetic opportunities does the term 'transcultural' offer? To begin with, there is that prefix of transition or transversion — highly capacious, it suggests movement and fusion, encounter or exchange across difference. It suggests transdisciplinarity, approach from the fringes; or else transaction: mutual benefit, or mutual exploitation. We are also lucky, in our historical moment, to enjoy a radical resignification of the term 'trans', or the reinscription of transness with a radical signature. To be trans is to be transgressive, alternative, to do things otherwise: to engender new possibilities or recover lost ones. In the Latin, to be *trāns* is to be 'on the other side' of something — the wrong side of the tracks, the other side of the future, whatever lies behind the walls we come up against. A psychiatry of transpoetics is one of translation —

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[8] In classical Greek, there are famously two different words for life: *bíos*, more closely related to our technical sense of biological life; and *zōḗ*, which connotes something more like a life story, something enjoyed only by the higher organisms. *Psukhḗ* is at the intersection of these forms of life: the human soul is, for Aristotle, both a natural principle that can be analyzed through the mechanical capacities and habits of the physical body, and the source of our distinctive intelligence and sociality — at once biologically and biographically salient (*De Anima* 412).

getting lost in translation, finding things in the act of translation — and of rendering transparent what has until now blocked our vision and our feeling. ‘Culture’ is what we cultivate; it stands against nature and it grows out from nature. From the Latin *cultus*, culture refers us to agriculture, tending the land; equally culture is something to be worshipped, a cult object. In the laboratory, there is the bacterial culture: carefully controlled, this method of microbiological trial also involves rendering visible what we were otherwise unable to see. In this second term, we can find the poetic resources to imagine an alternative form of care, one which involves crossing boundaries and mixing metaphors to illuminate new concerns and empower new forms of life.

Chapter 2. Context and Definitions

Before outlining my conceptual framework and methodology, I want to provide a brief list of the key terms that structure the story and language of this thesis. Some of these terms are conceptual, and act as important theoretical centers of gravity around which my wider argument about transcultural psychiatry will be organized; others are included to clarify the necessary basic context of my fieldwork — these latter are descriptions of institutional spaces and relations, which constitute the field of transcultural psychiatry as I explored it in the course of this research project. I consider the definitions which I provide here for both kinds of terms to be incomplete — or at least as situated, functional, and non-authoritative. Many of the conceptual terms are used in slightly different ways (or in simply inconsistent ways) across my field sites; in some cases, they are openly considered unsettled according to the relevant contemporary scientific or critical literature. As for the contextual terms, these refer to institutional organizations without entirely clear boundaries, often being constituted more concretely through their relations and interactions with other institutions than through any explicitly-outlined internal plan of their own. This set of definitions is intended, then, to orient the reader in the terminological universe of this thesis, but should not be considered an attempt at an authoritative technical summary or review of these terms as they are used 'in the field'.

As I've suggested in the previous chapter, and as will continue to become clear in the chapters which follow, this ambivalent relation to defining or naming things is not only part of the framework of this project, but also a result of its findings. The Latourian model for the anthropology of science calls for the study of science as it happens — as an activity, and not as a set of settled facts — indeed, this is the first 'rule of method' in *Science in Action*. Latour

articulates this methodology in an effort to treat science like any other object or field of anthropological inquiry — taken on its own terms, but treated agnostically as a set of evolving relations and practices, and without any special claim to absolute 'truth' or 'reality'. This implies that Latourian anthropologists of science engage with scientific definitions in something like the larval stage, as sites of ongoing reinscription and reinterpretation. As I show in my document analysis, transcultural psychiatry as a field itself draws deeply on anthropological attitudes and methods; in this sense, its commitment to cultural inquiry as an element of medicine in some ways resembles Latour's call for an social anthropology of technoscience — both are practices which bridge social and material forms of analysis. In other words, this asymptotic approach to definition represents an interesting consonance between methodological considerations that are 'internal' to my own approach to research, and disciplinary attitudes that are 'external' — part of the unique transcultural psychiatric attitude which has, in many ways, adopted fundamental epistemological and methodological principles of contemporary anthropology.

Part 1. Field Names

Transcultural Psychiatry itself somewhat resists simple definition. As a distinct, named, or 'self-conscious' subfield of psychiatry, it emerged in the 1950s, with the creation of the Division of Social and Transcultural Psychiatry (DSTP) at McGill University. The field grew steadily over the next decade,[9] and Arthur Kleinman's influential 1977 paper in the eleventh volume of *Social Science and Medicine* proposed or articulated a number of concerns and subjects of interest for a 'new cross-cultural psychiatry,' which largely remain in vigour today.

[9] The American Psychiatric Association (APA) and its Canadian counterpart (CPA) established committees on transcultural psychiatry in the mid-1960s, with the World Psychiatric Association (WPA) following suit in 1970.

Chief among Kleinman's contributions was his proposition of the "category fallacy" (Kleinman 5), referring to the problems that can arise when concepts from one medical context are imposed across cultural boundaries. Like any academic discipline, transcultural psychiatry holds together a variety of research programs and attitudes, not all of which are straightforwardly reconcilable. However, its organizing claim holds that cultural context is a fundamental component of mental health or psychopathology; consequently, cultural difference is a meaningful factor which comes to bear both on psychiatric theory and practice, or psychiatric representation and intervention (*Textbook of Cultural Psychiatry* 4). The central inquiries of transcultural psychiatry include studying culturally-specific presentations and experiences of psychological distress, and their relation to overarching diagnostic categories; the dynamics of migration and diversity in the field of psychiatric medicine; and the critical analysis of psychiatric knowledge as a cultural production, with numerous local variations (*Re-Visioning Psychiatry* 622).

The approach to transcultural psychiatry at the contemporary DSTP strongly emphasizes an **ecological science** of psychiatric concerns: it presents culture as one level or system in a more or less concentric ecological structure, which binds together microactors (such as synapses) with macroactors (such as social discrimination, or environmental conditions like climate change). This multi-system attitude expresses itself clearly enough in terms like 'biopsychosocial', which some members of the DSTP have used to describe their nested model or integrative concept of psychological life. Though their primary focus typically remains with an emphasis on culture, or 'the social' — for example, as an influential medium through which features of the other systems somehow express themselves — they resist circumscribing their inquiry in purely cultural terms. In other words, transcultural psychiatrists at this site encourage *integrating explanatory factors*

across this set of systems and scales, without ultimately reducing any one system to another (Gomez-Carrillo et al. 3). In this sense, the DSTP's form of transcultural psychiatry is innately allergic to reductive efforts which go too far in prioritising one level or mechanism of explanation — as is still common in mainstream neuroscience with the neurochemical level, in behavioural psychotherapy with the cognitive level, or in clinical psychiatry with the pharmacological level (Kirmayer & Crafa 10). In my thesis, I use the term 'transcultural psychiatry' to refer both to the institutionally-organized academic discipline (with all its internal tensions and factions), and to those other approaches to psychiatric research that hold cultural difference to be a meaningful component of a broader ecology of mind.

Allostasis is a concept in biology and biomedicine, first proposed by Peter Sterling in the late 1980s (Sterling & Eyer, 1988). It offers a new model for interpreting the regulatory mechanisms of the biological organism, with a key focus on predictive anticipation; in this sense, the conceptual model of allostasis is essentially temporally-inflected. It builds on and departs from the dominant conceptual model of homeostasis, which holds that the organism functions to preserve the constancy of its internal milieu, introducing two important innovations. Firstly, it seeks to embrace more coherently the existence of regulatory mechanisms geared towards *predictively and responsively initiating change* within the internal milieu; and secondly, in so doing, it proposes a more robust framework for *coordinating these internal responses to changes in the organism's environment* (McEwen & Wingfield, 2003). In other words, allostasis critically rearticulates the homeostatic model to embrace the *situatedness of the biological organism* by introducing *a more robust temporality* and *a more robust environmentality.* By decentering the

importance of the present-tense homeostatic boundary between internal and external milieus, this new model makes more room for conceptualizing the organism as intrinsically bound up with the environment and its possible futures (Sterling, 2011). At the same time, Sterling argues that the model accords a more appropriate level of importance to the brain, as the organ primarily responsible for regulating this anticipatory strategy of relating to the world.

The allostatic model is in many ways the orienting conceptual framework at the CESAR; the research efforts at this lab are organized around the related concept of **allostatic load**. Allostatic load builds on the allostatic model as an attempt to define and measure the cumulative cost to the organism of this constant adjustment to an unpredictable biosocial milieu (McEwen & Stellar, 1993). Allostatic adjustments are by nature imperfect: whether they are thwarted by irresolvable challenges of the physical environment and organic body or (as is often the case in human biology) by unbalanced social factors, they carry a cost to the organism in the form of stress. In the context of biomedical research, the working definitions of stress are assembled out of a shifting set of chemical entities which can be measured and quantified by the laboratory apparatus, and which previous studies have reliably linked with both psychological distress and physiological disease. The testing of allostatic load measures for a certain matrix of these biological markers, as a predictor for various kinds of disease or disorder (McEwen, 2000). This representational model of allostatic load is somewhat less brain-oriented than Sterling's conceptual version of allostasis, insofar as it relies on a series of chemical mediators of stress which can be measured in saliva, blood, and hair. Research into allostatic load thus suggests that the body of a given organism operates as a kind of physiological archive, in which the traces of stress index past environmental situations and adaptive habits.

In general, by directing attention away from individual physical mechanisms, the allostatic approach emphasizes higher-level representation of regulatory situations, and thus prioritizes higher-level interventions into situations of disregulation.[10] I will at this point only suggest the inherent critical charge of this concept: in the model of allostatic load, disease can be traced back through the adaptive strategies and hormonal agencies of the body under stress, to the environmental factors which once confronted or continue to confront it. A similar attitude is clearly at work in the transcultural approach more broadly, which understands individual distress as at once an expression of social structures or cultural norms, and an effect of physical structures or bodily habits. At my field sites, transcultural psychiatrists prioritize the intervention that will afford the most leverage in a given case: they mobilize collective-scale resources and social organizations when it is possible, and body-scale resources such as pharmaceutical regimens when it is necessary. Here, from the local perspective of the clinic, concurrent analyses of political systems and nervous systems interact in the same effort to understand stress.

With more representations, come more interventions; from more ways of naming a psychiatric problem, transcultural clinicians develop more ways of becoming useful against it. In other words, these representational models authorize a *medical critique of* or *intervention into disregulated sociocultural situations*, promising new insights into the multisystemic interactions of the organism's internal and external milieus. If the disorders of the body are successfully bound up with social forces in theory, than medical authority over the body can more credibly be brought to bear on social dynamics in practice. Describing these efforts is the work of this thesis.

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[10] See especially Sterling (2011), in part ten ('What is Health?').

Part 2. Important Concepts

Somatic is a polysemic term in this thesis; and the multiplicity of forms or styles of thinking through embodiment which I try to hold together in this project may have already begun to become clear. Part of this polysemy is internal to my approach, but it is also a result of the bright diversity of styles embraced by the transcultural model. In the context of psychiatry in practice, the patient's body is a locus of medically significant symptoms; so too can it be understood as a complex network of causal systems and mechanisms, which 'acts' on the psychological scene as the source of more or less desirable mental states. In allostatic research, it is more so the body which is acted upon or activated by the environment, via the regulatory mechanisms of the brain and nerves; I have also argued that, in this field, the body serves as a source of measurable information, a latent archive of stress which can be resignified or leveraged through medical interpretation. In the practice of ethnography, the body is also an important sensitive instrument, which registers salient details and structures of the cultural environment through specific methods and media of perception. One of the most important resources for my thinking of the body is Bruno Latour's 2004 essay "How to Talk About the Body: The Normative Dimension of Science Studies," which also ties somatic sensitivities proper to anthropological analysis together with the forms or formulations of the body in scientific contexts.

Humanity is obviously an integral concept for any anthropological inquiry. This more or less straightforward term takes on a heightened degree of complexity, however, in the practice of Latourian anthropology. Bruno Latour, across his philosophical career, but especially with his

articulation of actor-network theory after the turn of the century, places a tricky asterisk next to the idea of human nature; through his questioning of the 'great divide' between the human and the **nonhuman,** Latour calls for a *symmetrical anthropology* which would bring those exiled environmental elements back into the domain of critical analysis (*NM* 27). I have suggested that transcultural psychiatry effects this kind of symmetrizing operation in reverse, insofar as the multisystemic notion of the organism and its environment establishes links between biomedical domains like physiology or neurochemistry and various sociocultural factors and actors.

The ability and habit in biomedicine to reduce systems of the human body to a kind of 'mere' materiality is an important source of its strength: the isolation of the 'objective' qualities of these physiological systems in laboratory settings is what enables the development of powerful agents of treatment in fields such as pharmacology or surgery. In the Latourian style, some problematic aspects of this kind of 'objectifying' operation — which cultural analysts like those in the fashion of Michel Foucault have rigorously critiqued as 'dehumanizing' — might be reframed as *nonhumanizing.* A nonhumanizing approach to representation and intervention opts for the 'reductive' formulation of the human body (in terms of its material) in order to unlock and enlist the powerful agencies of its physical systems. A symmetrical medicine would, in such a conceptual framework, enable and demand a *rehumanization* of the multisystemic organism, by crossing the great divide to engage thoughtfully with the cultural agencies and realities that somehow 'make us human'. Consider the common case in transcultural psychiatric consultations of migrants experiencing dehumanizing conditions: transcultural physicians may intervene in these situations using both physiological and sociological sensitivities — prescribing pills and therapies, or advocating for better community resources and conditions of housing and labour.

Stress is, in some ways, the central 'object' or phenomenon of this thesis. It is useful as a linking concept between my field sites, to the extent that the psychiatric endeavour can be framed as an effort to relieve or transform stress. However, as another polysemic term, it presents some analytic challenges. In its common use, the term often carries the sense of an internal or phenomenological *experience* or *mood* — this sense of stress is not irrelevant to my conceptual frame or my sites of research; but, especially at the CESAR, the primary meaning of stress is a different one. Under the auspices of the allostatic model of physiology, stress has a more strongly materialist connotation — it is tied to specific biomarkers,[11] and thus involves a somewhat more objective interpretation. According to this view, stress is the physiological or biological result of an interaction between a given organism and its environment. To the extent that different organisms respond to similar situations in vastly divergent ways, there remains a certain 'subjective' or 'experiential' dimension to the production or emergence of stress, but it unavoidably assumes a more measurable and concrete nature. Bruce McEwen, for example — the namegiver of allostatic load — provides a fairly narrow definition of stress in his 1993 essay "Stress and the Individual: Mechanisms Leading to Disease." For this reason, I make a distinction between stress as a biomedical term, and **distress** as a more general psychological category, object, or problem. I relegate the subjective dimensions to this second term, despite the fact that scientists and researchers across my field sites do not rigorously uphold the boundaries between these concepts, which clearly remain importantly linked.

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[11] Biomarkers are specific physiological agents or chemical mediators that, in the context of medical research, serve as measurable indicators of some biological state or condition. The most common biomarker of stress, for example, is the steroid hormone known as cortisol (Juster et al. 11).

Agency, in this thesis, is similar to the term stress: it implies both subjective and objective senses; and it involves both technical and casual definitions, being used differently across my field sites and across the various sources from which I compose my conceptual framework. In this case, it is the psychiatric sites which afford a more casual usage of the term: at the DSTP, scholars use agency to refer to the difficult-to-describe feeling of 'being in control' of one's thinking and acting (*CMB* 245). This loosely defined phenomenon is in this sense importantly related to both the experience of distress, which involves a sensation of helplessness, and to the technical definition of stress, as both Sterling and McEwen identify the lack of adequate adaptive resources as one possible cause of the physiological stress response (Sterling 1; McEwen 108). Ultimately, despite the fact that producing or generating a healthy feeling of agency is in some ways the main goal of psychiatric care, it remains a somewhat speculative object — the biological foundations of agency in the brain, mind, and body are not yet authoritatively defined, and the experience takes on different meanings and requires different conditions across different sociocultural environments.

A more grounded and materially-oriented sense of agency is at work in the Latourian philosophy which guides my analysis. Latour uses the term subversively, arguing for a critical acknowledgement of **nonhuman agency** (*NM* 51) — in other words, his call for symmetrical anthropology involves taking account of the ways in which the nonhuman elements of an environmental milieu influence, give shape to, or organize human activity. For Latour, it is not possible to think clearly through the contours of the space in which human agency might emerge as an event or an experience without a serious consideration of nonhuman agencies. To the extent

that the medical sciences bring a rigorous critical attention to the 'nonhuman' systems of the human body, they thus lay the groundwork for a mature analysis of this tricky and transient sociocultural phenomenon. It is also interesting to consider that, in the laboratory settings where Latourian anthropologists ply their trade, the term 'agent' has yet another technical sense, as that which sets off the conditions for experimental observation. The agents and reagents of laboratory life are always destined to be controlled, measured, and converted into scientific information. In the Latourian social science — where such boundaries as those between human and nonhuman actors or between cultural and natural systems diminishes — the terminology for environmental analysis can compute agency in both senses, as willful subjectivity and as controlled objectivity.

I have described my own approach to research as interdisciplinary, and I have suggested that my field sites are, to varying degrees, multidisciplinary spaces of research as well. So what is the sense of **discipline** in this thesis? The term requires unpacking because it too involves a double meaning. The first meaning corresponds to the self-conscious organizational structure of contemporary sciences — a structure which is by no means exhaustive or internally consistent. This nebulous structure or process of skill-replication and knowledge-production expresses itself in the naming choices made by university departments and professional associations;[12] as such organizations proliferate, they outline various research programs and delimit a range of valid techniques. I often prefer the term **field** to refer to the complex and shifting inner articulations of

[12] One of the more practically significant articulations of disciplinary boundaries comes from the governmental funding agencies which require researchers to declare their allegiances explicitly. In Canada, most scientific research must at some point identify itself to these agencies — the Canadian research council structure makes the fundamental distinction between three kinds of disciplines: social sciences and humanities, science and engineering, and health. Transcultural psychiatry, however, clearly blends elements of all three of these broadest fields.

this organizational landscape — that term, I think, better suggests the way in which research programs can intersect, influence each other, and exert a background force on the structure of laboratory activities. However, the sense of discipline does capture the way in which scientists *decide to submit themselves to the programs and rules* of a given field — to become disciples, as it were — and as a result gain access to its privileged networks, resources, and alliances.

This second, related sense of discipline concerns the extent to which spaces of scientific activity, in exchange for the resources and status they confer to their members, also demand or impose regulations on behaviour for all of the various kinds of people who pass through their walls, according to their various roles and interests. At the DSTP, professors must meet certain curricular standards, as students are required to behave in a certain way in classrooms and demonstrate certain competencies in examinations; at the CCS, psychiatrists are beholden to ethical and medical regulations set by the professional bodies, and the compliance of patients is a topic of frequent discussion; at the CESAR, student researchers learn how to transform participants into disciplined and predictable objects of research (a process of objectification which recreates them as new kinds of subjects), and lab technicians must follow extremely complex methodological procedures in order to transform biological material into useful information. Just as submission or allegiance to macro-scale organizational disciplines affords a degree of professional mobility and stability to individual researchers, *submission to these micro-disciplines produces and conditions the kinds of subjects and objects that are required for the processes of fact-construction.* A limitation inherent to both senses of the term is that it threatens to obscure the agency enjoyed by researchers and participants to assert their own style, to play with the rules, to refuse compliance: ultimately, the proscription of resistance is futile.

One last conceptual term worth dwelling with is that of the **instrument**. Really, my use of this term is pretty straightforwardly taken from the Latourian analytic lexicon, but I would like to provide a discussion of it because of how many elements of his account of the laboratory it gathers together, and because of how far I've gone in appropriating it as an orienting idiom in my approach to ethnography. For Latour, the fundamental feature of the laboratory is that it is a site of *literary production*, and the fundamental gesture of science — as a discourse, as a body of knowledge, and as a technical practice — is the *act of inscription* (*LL* 45). There are two main kinds of literature at work in the laboratory. The basic kind is the raw data, the results of physical or mechanical observation; the second is the scientific paper — the presentation, interpretation, and rendering significant of this data (*LL* 86). As sites of inscription, laboratories facilitate the continual transformation of material into some kind of *information,* to render things *readable, visible,* or otherwise *describable*. Scientific information is marked by its *stability, mobility,* and *combinability*; it is this peculiar kind of attestable information which *authorizes* the scientific rhetoric of facticity and reality (*SIA* 224). For Latour, any machine, structure, process, or pattern of behaviour which produces such inscriptions is a kind of instrument.[13] Things go in, they are subject to some trial, their performance is tracked and traced, and these traces of activity become objective information — *voici* the wonderful magic of the scientific instrument.

My practice of ethnographic observation is deeply inspired by this affordance in the Latourian philosophy to think of the body as an instrument in at least two senses. The first sense

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[13] At the CESAR, the laboratory work bench hosts one especially conspicuous kind of instrumental apparatus; but, for Latour, the computers that participants use to fill out surveys would be another kind of instrument. Also 'instrumental', then, are the observant and disciplined bodies of the experimenters, who record qualitative descriptions of the participants' affective mannerisms, and guide them through the trials of the experiment.

posits *the body as a sensitive receptacle*, within which measurable material traces of the social order are differentially inscribed and preserved; here, the body contributes to the production of inscriptions through its cooperation with or conscription of other instruments, such as the test tube and cortisol assay. In the second sense, *the body is a sensitive interpreter* of the environment; here, it generates meaningful inscriptions through its submission to or cultivation of disciplinary techniques and technologies such as note-taking, the transcription of audio recordings, or literary textual analysis. In both cases, the information which ethnographers might use to describe the contours of reality is constituted by a collaboration between human and nonhuman agencies, which cannot be clearly reduced to any one governing scale or idea. Importantly, it is also possible to interpret the informational function of laboratory instruments as the work of *giving voice to its objects* — allowing them to speak in our language. The discipline of anthropology is only more uptight about enforcing a boundary between subjects and objects — we insist on the subjectivity of those actors who provide us with our data, but by calling them 'informants' we seem to implicitly recognize that they serve a basically analogous role to laboratory objects. In any symmetrical form of inquiry, this boundary becomes much less sacred.

Part 3. Field Sites

At this point I will transition away from defining the conceptual, theoretical, or analytic terms of this thesis, and provide some brief context regarding my sites of field work, and the names that I use to refer to them. I will move through the field sites as I came to them in chronological order, providing, along with the basic institutional structure and purpose of each site, some sense of how these three organizations relate to each other. Right away, it will become

evident that, whatever the scientific method might be, actual scientific practice is supported by very complicated networks of public institutions; and, needless to say, the concrete relations that sustain and connect these diverse institutions of science blend competing bureaucratic, political, financial, and social interests, in addition to any purely scientific ones.

The **Division of Social and Transcultural Psychiatry** at **McGill University** is my first main field site. As mentioned above, the DSTP was the first official research organization in the discipline of transcultural psychiatry; it remains an important center of gravity for the field in Montreal, and beyond. Laurence Kirmayer, the director of the DSTP, is also editor emeritus of the journal *Transcultural Psychiatry*, which is the major literary publication of the wider discipline. If researchers can translate their work through the DSTP, align their interests with the DSTP, or become members of the DSTP, they gain access to its substantial scientific credibility. My main ethnography took place at a three-day workshop in Summer 2023, called **Culture, Mind, and Brain** (CMB). This workshop, and others like it, are a production of the **Culture and Mental Health Research Unit** (CMHRU), which is a research branch of the DSTP that links scholars at McGill with physicians and researchers at other major centers of authority. The DSTP operates out of numerous buildings on the McGill campus, but especially the **Irving Ludmer Psychiatry Research and Training Building** on the edge of the mountain.

Through my contact with the CMHRU, I was connected to the **Cultural Consultation Service** (CCS), a transcultural psychiatry service supported by the **Lady Davis Institute** (LDI) of the **Jewish General Hospital** (JGH) in Montreal. If the DSTP represents an important site of

the *educational program* of transcultural psychiatry, the CCS exemplifies a practical application of its *medical program*. It would not quite be right to frame this as a distinction between theory and practice: ultimately, in a symmetrical anthropology, theoretical representations can never really break free from technical interventions. Nonetheless, transcultural psychiatrists at the CCS are definitely less interested in reciting or refining explanatory models than they are in situating specific problems and coming up with workable solutions. The CCS emerged as a collaborative project between the CMHRU and the **Institute for Community and Family Psychiatry** (ICFP), an organization that cropped up at the JGH five years after the establishment of the DSTP at McGill. The CCS' current director is Eric Jarvis, editor-in-chief of *Transcultural Psychiatry*; Laurence Kirmayer, of the DSTP, is the founding director. The service is operated by the West-Central Island branch of Montreal's **Centre intégré universitaire de santé et de services sociaux** (CIUSSS).[14] I observed and participated in regular case conferences of the CCS over a six-month period — these consultations were held virtually. This virtual space accommodated the voices of family physicians, social scientists, physical therapists, community workers, nurses, and psychiatrists — in other words, it is a space of highly symmetrical concerns.

My final and main field site was the **Centre d'études sur le stress, l'allostasie, et la resilience** (CESAR), where I was especially involved with their **Stress and Resilience Study** (STARS). The CESAR is a research unit of the **Université de Montréal** (UdeM), operating out of a CIUSSS site in the East Island branch, where it is supported by the **Institut universitaire en**

[14] The CIUSSS is an important infrastructure in the story of transcultural psychiatry in Montreal because of its quasi-symmetrical model — its mandate assumes the sensibility of dealing with medical and social problems within the same framework. As an institute of public health, its influence on the research program and epidemiological attitudes of transcultural psychiatry also marks it as a relevant subject for analysis using Fassin's critical framework.

santé mentale de Montréal (IUSMM). I met Robert-Paul Juster, the director of the CESAR, at the CMB workshop. In general, the CESAR lab carries out projects which inquire into the links between stress and *sexe*genre* (or sex*gender). Obviously complex, and in a certain sense openly indefinite, this binomial term denotes a subfield of research which the CESAR continues to rearticulate, at the same time as they frame themselves in relation to it. In other words, the CESAR projects constitute their new kinds of objects — namely, stress hormones which can speak to sex and gender — through the lab's own generation of the information in question. The heightened symmetry of this application of allostatic load might be clear: CESAR studies like the STARS use social factors to interpret material findings, at the same time as they use 'natural' sciences such as hormonal chemistry to articulate a new aspect of cultural reality. I enjoyed four months of biweekly fieldwork at the CESAR, where I not only attended meetings and asked questions, but also worked as a semi-regular member of the STARS project. The CESAR is located in the **Centre de recherche Fernand-Seguin** at the **Hôpital Louis-H. Lafontaine**.

Now, with that contextual work out of the way, we're in a much better position to unpack the conceptual sources and methodology of this thesis project. After all, understanding the conceptual orientation of symmetrical anthropology is key to grasping the practice of ethnography for which it asks and advocates. This symmetrical approach promises to afford the analysis of new kinds of objects — systems of linked objects or actors spanning the human and the nonhuman, blending scales of spatiality and temporality, and dissolving the boundaries between nature and culture. The concept of symmetry thus oriented my interest in identifying a multiplicity of agencies in any given context: when no one explanation needs to settle the

account, analysis can prioritize a lightness of foot that enables the traversal of many systems and styles of accounting. This ability to traverse systems proved crucial to my effort of following transcultural psychiatrists as they worked to discursively capture and materially reorganize the environmental conditions of stress. The highly pluralistic repertoire of these scientists includes analytic tactics and pragmatic techniques of fields as diverse as biomedicine, comparative religion, radical sexual politics, and statistical probability sciences, just to name a few. If I embody any conceptual or methodological sensitivity to complex structures of causality and agency, it is in large part as a result of my encounter with the promises of this emerging field of symmetrical science, which draws its authority from a unique boldness before the indefinite.

Chapter 3. Conceptual Framework

In the previous chapter, I defined some of the key conceptual language of this thesis in its own terms. In this chapter, I will more formally present the textual materials that compose my conceptual framework. Since these materials are the source of many of those key terms from the last chapter, there will be a certain degree of repetition. This repetition, I argue, serves an important purpose. To begin with, it will make for easier reading of the somewhat complicated philosophy of science and the body that I work to develop throughout this essay. This conceptual framework itself represents one of the main contributions of my project: my reading of the critical anthropology of medicine alongside the Latourian program for science studies is a unique one. So, while the terms of the previous chapter will not be unfamiliar to readers of the texts discussed here, it may by now be clear that my use of them is slightly idiosyncratic. Although these source texts all revolve around the same themes of science and the body, this is an interdisciplinary and thus, to some extent, incongruous collection of works. In this sense, the definitions of the previous chapter can be thought of as a kind of primer for (and foreshadow of) the textual arguments I will make in this one. They represent the end-point of a synthetic reading of these interdisciplinary works; this chapter should thus be read as involving a movement from a diverse body of source materials to the more cogent terminology represented above.

Now, before getting started on this movement, a brief outline of its overall shape is in order. The conceptual framework will be divided into two main sections, each with three parts. In the first section, I will treat the Latourian philosophy of science which structures this research project. The first part of section one covers Latour's more straightforwardly anthropological work: this includes his account of science as a fundamentally *literary* enterprise, and his

concomitant argument for how to do good laboratory ethnography. The second part of section one continues with this literary analysis of science, and begins to transition towards Latour's broader sociological arguments about how this mode of literacy becomes successfully *constructive* — in other words, how laboratory scientists actually organize networks of activity and build systems of knowledge. In the final part of this first section, I will discuss the more philosophical sides of Latour's work — articulating especially his linked concepts of hybridity and symmetry — which are in important ways direct achievements of his practice of laboratory ethnography. These philosophical themes are at the heart of my conceptual framework, and it is in these terms that I carry out my analysis of Montreal's transcultural psychiatrists at work.

In the second section, I develop the elements of my conceptual framework that are more specific to this distinct kind of environmentally-oriented medical science. I do this by reading Didier Fassin's critical medical anthropology into, or through, Latour's philosophical lens; while Latour's program of study provides a firm ground for the ethnography of laboratory science in general, this further resource will supplement my efforts towards the analysis of a scientific practice which moves *beyond the laboratory* and *into the clinic*. Although I will still think through the clinic in Latourian terms — that is, as a literary site of sociomaterial constructions — the clinical dimension of psychiatry clearly merits some unique conceptual structures and critical tools. In the first part of this second section of my framework, I will introduce and elaborate the philosophical idiom of *irreducibility* that characterizes the systems-oriented thinking of transcultural psychiatrists; in so doing, I will bring Fassin's work into play, which describes how public medicine instrumentalizes various kinds of systems thinking in its effort to construct the clinic as a space of *political power*. In the second part of section two, I will continue with my

treatment of Fassin's anthropology of public health, in order to think through the different ways that participants in laboratory and clinical environments might constitute themselves (or be constituted as) specific kinds of medical subjects and scientific objects. In part three, I will return to Latour with this new critical language of public health in mind, in order to consider the crucial role of diagnostic naming in these transformative encounters that take place in the laboratories and clinics of psychiatric science.

Ultimately, it is my argument that, in the act of naming their patients with a given medical condition, psychiatrists leverage the affordances of these scientific environments to effect a double transformation — confirming their own political and cultural authority, while also changing the status of their patients. I argue that, by naming the disorders of their patients in the *scientifically-credible* and *culturally-intelligible* terms of psychiatric diagnosis, clinicians endow their counterparts with a new kind of *stability* in and through the social networks that are organized around the laboratory or clinic. Alongside this diagnostic stability, named patients also enjoy an increased *accessibility* to the institutionally-distributed resources of the public healthcare system. There is a clear and distinct critical charge to this presentation of medical science as social construction — and indeed, describing laboratory science as the construction of powerful regimes of knowledge may seem to undermine its neutrality or validity. Critical studies of science such as this one, however, argue that identifying the 'constructed' or 'artificial' origins of this power in fact contributes to the scientific endeavour, by emphasizing its open-ended nature, and by broadening its possible scope or relevance. If there is no radical separation between scientists and society, then the authority of scientists is valid not only in the laboratory, but instead may make sense wherever their long networks happen to lead them.

This conceptual framework, inspired by the prescriptive side of Latour's philosophy of science as much as the descriptive one, will thus eventually provide a basis for the articulation and identification of responsible medical science. Through his literary definition of science, Latour attributes a strong semiotic power to its authors. Contemporary anthropologists have a well-proven expertise for the incisive denunciation of the negative dimensions of such concentrated social power; in this thesis, I attempt to wield these anthropologies of science to think through some possibilities of its positive dimensions. This being the case, I orient this conceptual framework towards the opening of analytic questions that borrow from the conspicuous optimism of the physical sciences. If there were to be responsible ecosocial scientists in these positions of power, what might their practice look like? How might they wield their broad potential authority to reorganize the social ecologies that produce vulnerable or unsustainable forms of life? How might they intervene in processes that concentrate material and political resources for some members of their networks, while marginalizing or exiling others entirely? And, thinking back to the conclusion of the first chapter, how might their literary activity of namegiving contribute to the fullness of human diversity and joy?

(I) Critical Science Studies

Part 1. Laboratory Literacies, Disciplinary Alliances

Latour's first major ethnography of the scientific laboratory is in *Laboratory Life.* In the first chapter of this text ('From Order to Disorder'), he describes the activity of the laboratory as a kind of drama which, though it assigns different roles to its various actors, is ultimately oriented towards the "construction of sense" through "the production of order" (*LL* 34; 37). What

this double process essentially involves, in other words, is the organization of *noise* — the "apparent chaos of available perceptions" — into "an ordered account" of how things work (*LL* 35). So, by which "socially available procedures" do scientists build their orderly accounts out of disorderly noise (35)? According to Latour, the scientific drama has two main acts: the production of literary inscriptions, and the construction of disciplinary alliances.[15]

Let's begin with the literary side. Latour writes that the anthropologist who visits a laboratory will witness there a "strange mania for inscription," an obsession with writing and recording that is so thorough as to be self-evident (48). He distinguishes between two kinds of laboratory literature (47): 'external' documents, of which the paradigmatic example is the scientific article; and 'internal' ones, on which the experimental data is initially inscribed. These inscriptions constantly circulate in the lab, moving from hand to hand in a process of ongoing metamorphosis that might see them transform from massive spreadsheets into "a single elegant curve," representing the refinement of near-countless data points and hours of work (50). For Latour, the fundamental feature of this trajectory is the incremental disappearance of the original context of inscription. As an inscription moves away from this original context, it moves towards the second type of literature, the scientific article, which takes the "end diagram as the starting point" for its claims to order (51). As he put it eight years later in *Science in Action*: "Nature is not directly beneath the scientific article; it is there *indirectly* at best" (*SIA* 67).

So, what mediates this movement from total noise to the still-noisy internal literature, and then from the semi-ordered internal literature to orderly external accounts? And, conceptually speaking, what results from this movement? In the Latourian terminology, the "instrument (or

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[15] See 'Literary Inscription' (46-53); and 'The Culture of the Laboratory' (53-68).

inscription device)" is the origin (68); the literary devices for combining and representing inscriptions are the means (these may be technical or rhetorical, linguistic or diagrammatic) (*LL* 50); and the formally-named scientific "object" is the ends (65).[16] In *Laboratory Life*, he defines the inscription device as that apparatus which "can transform a material substance into a figure or diagram which is directly usable by one of the members of the office space" (51). If laboratory science is a drama, instruments determine the design of the set and the protocols of the script: they are the crucible in which physical material first becomes literary material, or *information*.

In *Science in Action*, Latour describes this moment of conversion quite clearly. The laboratory instrument subjects an agent or material to some kind of "trial of strength" (87). The actual inscription which this device records is a measurement of the agent's "performance" in the trial (89).[17] In a laboratory, the research team assembles all of the measurements of a given substance's performance under the conditions of the experimental trial. The refining processes of the internal literature exclude any noisy or outlying results; if it is successful, the product of this first literary activity is a "new object," discovered or redefined by the experimental drama (87). In other words, the internal literature's process of refinement is also a "process of reification" (92): it produces "things" (91). Conceptually speaking, this 'reification of things' is the culmination of the laboratory's production of inscriptions, and its importance to the scientific endeavour of constructing facts and networks can hardly be overstated. As Latour argues:

> [These aggregates are] things isolated from the laboratory conditions that shaped them, things with a name that now seem independent from the trials in which they proved their mettle (91).

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[16] See also 'The Construction of a New Object' (*LL* 124-129) and 'Order from Disorder' (*LL* 244-252).

[17] For example, in an antigen COVID test, the instrumental device receives a solution containing physical substances: the resulting visible reaction determines or reveals the status of the solution. In this example, the solution is the agent, the exposure to the chemical solution on the test paper is its trial, and the dreaded second line (or lack thereof) is the result of its performance. This final visible result is the basic form of an inscription.

Whatever 'nature' might be, our access to it in science comes through its ongoing accumulation of instrumental legibility, by which its otherwise noisy visibility or sensibility is rendered orderly and definite. This is the way in which the instrumental process of producing and interpreting inscriptions serves to *construct objectivity*: by *defining objects* in discrete, literary terms. Once a scientific object takes a definitive name in the external literature, it has a life of its own: there, it is put into use by scientists in the assembly of facts, theories, and orderly accounts.

What has happened to the material substance in its transit towards this objectivity? When the inscription device produces its measurements, it allows information to stand for and *represent* this original material. What makes information so useful in the production of objective names and the construction of orderly accounts is its increased *stability, mobility*, and *combinability.* Information *stabilizes* the material by translating its momentary performance in the trial into lasting figures. Its newfound stability increases the *mobility* of the material, insofar as these measurements can pass through many more hands and across much greater distances than the original substance itself. And finally, information is more *combinable* because it can be joined to other information in its passage through the literary devices of scientific interpretation, such as graphical and statistical analysis. These three powerful features of information are what enable the assembly or construction of the new named object out of the many inscriptions in the first place;[18] this translation of material into information also allows the scientific names to then be 'isolated from the laboratory' — as they permeate the external literature and accrue authoritative definitions, the now-independent constructs appear increasingly objective, part of nature itself.

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[18] At the risk of putting the hearse before the corpse, this is basically the best way to describe the activity of the CESAR lab *vis-à-vis* the concept of Queer stress: in the STARS project, thousands of inscriptions concerning the hormonal levels in the saliva of its participants are assembled into a new scientific object — Queer cortisol, or at least cortisol as an object stable and mobile enough to speak reliably to the conditions of life as a Queer person.

At this point, we might remember our visit with Plato in the first chapter, and his figurative drama of the two distinct powers at play in language. The first is the ability to *give good names* — this involves more of what Latour calls the 'production of order'. Here, then, is the first literary activity of science: using instruments and transforming inscriptions, so as to convert noisy material into orderly objects with clearly-defined names. Just like the namegiver, laboratory scientists fashion their material into a taxonomy of clear divisions, with each name meant to correspond to a distinct, natural thing. The only difference is that, for Plato, nature is the cause of the divisions; for Latour, it is the consequence. The second ability is to *use names well* — here is Latour's 'construction of sense'. The eventual use of laboratory names takes shape in the external literature, as scientists try to persuade each other that their interpretation of the information at hand is correct. In this activity, names are 'useful' insofar as they can combine diverse facts, objects, and techniques, such that they *resist dissociation*.[19]

This is an inherently *dialectical* process: the new names must be made sense of in reference to the old ones, either as further articulations, or as opposed definitions. The disciplinary organization of science is most apparent here, in this second literary activity, as controversies form over the 'correct' use of a given name. Once again, as a consensus grows in size, the name grows in strength: this is the social process of constructing credibility, by which the 'thing' that a name represents comes to appear increasingly 'natural', or factual. In other words, the 'reality' of nature is what eventually comes along with this continuous literary 'reification' of things, as the scientific construction of sense finally reaches its conclusion.

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[19] See 'Machinations of Forces' (*SIA* 128-132). In Latour's words, the question posed by scientists on the hunt for credibility is "how to convince others, how to control their behaviour, how to gather sufficient resources in one place, how to have the claim or the object spread out in time and space" (131). When the names used in a scientific construction resist dissociation, then "all the gathered resources are made to act as one unbreakable whole" (132).

Let's proceed with the disciplinary side of Latourian science studies, by properly defining his notion of the literary alliance. One of Latour's fundamental claims about science is that it cannot be strictly separated from the rest of society. This may seem straightforward enough, but making this claim involves deconstructing the conventional distinction between laboratory life (as a 'rational' or 'universal' enterprise concerned with 'nature' itself) and social life (as a locally-variable phenomenon determined by 'irrational' or culturally-specific forces). However, it will still be useful to draw a tenuous distinction between the 'internal' and the 'external' sides of science — identifying one kind of alliance between sets of scientists, and the other between scientists and non-scientists. The basis of both kinds of alliances is the scientist's claim to speak for nature; to arrive clearly at the 'internal' alliances that constitute the disciplinary organization of contemporary technoscience, a brief consideration of these 'external' alliances is necessary.

For Latour, laboratories are constituted by a vast array of social relations — it is not worthwhile to study them, or the science that takes place within them, without an understanding of this social constitution. In *Science in Action*, he argues that "the ability to work in a laboratory with dedicated colleagues [depends] on how successful other scientists [are] at collecting resources" (157). Here is a crucial division of labour: some scientists work at strengthening the literature with their laboratory instruments; others work at leveraging the strength of the literature, so as to gather the intensive resources which that instrumental process requires. This second form of work is fundamentally and thoroughly social; it involves convincing those with the resources that "making a detour through the lab" — that is, forming institutional networks to facilitate the movement of those resources — is "necessary for furthering *their own goals*" (157). An example of this kind of network is that of the federal grant agency; a massive institutional

body, organized to further 'national interests', it mobilizes funds and technologies for those laboratories which promise to deliver some return-on-investment. In such an alliance, the interests of taxpaying citizens, career politicians, university administrators, and professional or student researchers are tightly linked in the redistribution of resources. Without this long network of social alliances, there are no laboratories, no literature, no new objects, and no new facts — just lonely scientists, who now have no claims strong enough to hang their reality on.

I will elaborate further on the specific literary and social mechanics of this persuasive effort below, when I discuss Latour's account of the generational structure of scientific articles and research programs. For now, this basic notion of building 'external' alliances — as a further result or afterlife of scientific literature — is enough to ground the discussion of 'internal' alliances between scientists. At this point, Latour's account of contemporary science involves a fundamental point about scarcity: because the socially-available resources are limited (the public funds and the necessary technological infrastructures, but also public attention and interest), the second act of the scientific drama is structured by a *competition* between laboratories, for greater influence and position in the networks of these external alliances.[20] Professional scientists, Latour argues, fortify their own positions by making alliances with those others whose work supports their claims and shares their names — and by seeking to discredit those who dispute or oppose them. In his own words: as "statements are enhanced or discredited, [. . .] reputations and alliances between scientists are modified" (*LL* 158). This new selective pressure in the external literature joins those internal pressures in the shaping of an 'orderly' (or 'disciplined') object.

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[20] See 'Counting allies and resources' (*SIA* 162-176). Latour does not go far with the latent critical charge of this identification of the principle of competition in capitalist science; I will return to it in Chapters 6 and 7.

What counts as noise, in other words, is not only a question of a name's natural 'correctness', but also of its literary 'usability' in the terms and conventions of a given research program or field.

This is the fundamental structure of the internal disciplinary organization of the sciences: a literary competition, in which different scientists (with different sets of instrumental apparatus, technique, and expertise) try to solidify their own version of reality (their different statements, objects, names, and facts) against or at the expense of others. As Latour writes, "the work of the laboratory can be understood in terms of [. . .] delimiting the number of alternative statements which could be made" (*LL* 151). This describes not only the selection pressure to *include as much information as possible* in the definition of one's own objects, but also to *exclude as many competing or contradictory names* as well. If scientists want to solidify their claim to resources, they need to represent nature in as much order as possible; when there are not enough resources to sustain all the competing research programs, any alternative interpretations compromise the future of the laboratory. The shifting and incomplete lines of inclusion and exclusion map out the literary alliances; larger ones strengthen their claims to factuality and credibility on the basis of their ability to link more and more objects together.[21] Finally, the stronger alliances — with their evidently better method for persuasively constructing the facts — can translate the interests of ever more resource-holders into their own terms, and so they set the direction for future research.

The clear loop in these relations between the techniques of the laboratory, the currency of the names, the objectivity of the facts, and the alliances within and between the disciplines structures what Latour calls *the credit cycle of science*.[22] To understand how credit is either

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[21] See Chapter 6 of *Science in Action*, 'Centers of Calculation' (215-257).

[22] See Chapter 5 of *Laboratory Life*, 'Cycles of Credit' (187-234).

contested or solidified in this information economy, we will have to turn to Latour's more concrete analysis of the external literature. At the heart of this analysis is his identification of a *generational* or *dialectical* structure to all scientific statements. The basis of this structure is the literary activity of citation — the means by which new objects are formed in reference to existing statements. In his words, the definition of any "new object is thus shaped by simultaneously importing many older [objects] in their reified form" (*SIA* 92) — proponents of a new name try to borrow the credit of those authors they cite, and join their object to existing statements which are already considered factual.[23] In learning to read the textual processes by which names for new objects are received by readers as either artificial (and so pushed back into the laboratory for rearticulation) or natural (and thus isolated from the laboratory networks that produced them), we will be able to see the role that disciplinary pressures play in the literary construction of sense.

But first, a quick review of what this argument accomplishes with respect to my own research is in order. The structures of literature and discipline have led to this scene, where objects are judged according to the strength of the techniques which named them, but also by the suitability of the name for existing literary sensibilities and alliances. In the field of psychiatric research, as a branch of medical science, the major names in question are diagnostic categories — either their internal coherence and explanatory accuracy, or else in terms of their correct use and application in particular cases. In moving forward with this Latourian account of scientific literature, it will be useful to keep diagnosis in mind as a specific kind of dialectical object. Latour gives us the conceptual resources to interpret science as a struggle for the ability to name with authority. What does it mean when the objects being named are not nonhuman 'material',

[23] For a critical discussion of those indices which track the citation metrics of articles and journals, and how they influence scientific practice, see Hirsch (2005) or Cronin (1984).

but ourselves? How is the body converted into a site of literary contestation by these disciplinary pressures? How does putting literary names to use in the clinic — adopting them to organize the noisy reality of stress — also reorganize or rematerialize what makes up the body in turn?

Part 2. Speculative Entities, Citational Practices

The Latourian idiom for a fact with solidified credibility or authority is that of the "**black box**" (*SIA* 3); once enough black boxes are closed and linked, members of a discipline only have to invoke these names to make their claims nearly irrefutable. It should by now be possible to see how much in the construction of black boxes hangs off of the site of inscription: it generates the *authority* of the scientist and their claims, as their validity is tied to that of the inscription device's spectacular production of sensible information; it *materializes*, *realizes*, or *naturalizes* the objects and facts over which the scientist wants naming authority, grounding their durable intelligibility in the record of the agent's physical trial; and it sustains and determines the credit cycle of the external literature, thereby structuring the interests of all the disciplines and alliances whose shared futures are determined by this cycle. However, the scene of inscription is exactly what disappears in this process of blackboxing, as the objects lose their artificiality and appear to become parts of nature. How can we study this construction of black boxes in action, in the literary disputes and alliances between competing scientific disciplines?

For Latour, one answer is in looking towards the citational structure of the literature, which demonstrates two features of scientific factuality or objectivity. The first important feature of literary citation is its *modality* — the way in which citations involve some modification of the original statement as they refer to it or put it to use in a slightly new context. The second is its

temporality — in scientific literature, definitions are never completely certain, but rather sites of continuous reinforcement or rearticulation. In this part of the framework, I will first present Latour's overview of the various literary tactics that scientists deploy in their articles to persuade their readers and naturalize their facts; following this more technical catalogue, I will consider how these strategies contribute to the actual construction of resource networks and disciplinary regimes. The important thing to mind here is that this pressure to modify claims works in both directions: the sense of the old literature transforms as authors recite it for their own purposes, but it also sets the terms in which these new authors can articulate their facts in the first place.

Latour describes this ongoing process of modification as an argument over the *empirical status* of the information that constitutes the objects in question: "one set of agonistic forces pushes the statement towards fact-like status, [while] another set pushes it toward artefact-like status" (*LL* 180). In scientific terms, an artefact is any inscription that is a false result: an effect not of the material's performance, but of its interpretation — or of the measuring device itself. A simple example of a debate over artefacts can be found in the seventeenth-century controversy over Galileo's telescopic observations of imperfections in the lunar surface (Finocchiaro, 1989). Christopher Clavius and Orazio Grazzi, two prominent Jesuit astronomers, both disputed Galileo's claim that the surface of the moon was rough and rugged — a claim which contradicted the classical Aristotelian cosmology. They asserted that the crude lenses of Galileo's early telescopes caused distortions in his observations; in this case, the Jesuits were arguing that Galileo's lunar imperfections were not factual, but rather 'artefacts' of his device.

In the first chapter of *Science in Action* ('Literature'), Latour describes what happens to fact-claims during a scientific controversy. He introduces the analytical concept of "**modalities**"

(22): a modality is a sentence that has been altered in such a way to either push the claim towards factuality (positive modalities) or artefactuality (negative ones). If an original statement claims 'cortisol levels in saliva are related to environmental conditions', a positive modality involves constructing new facts on this basis: 'cortisol levels in saliva are related to sexual orientation'. In this case, the original argument about the basic environmental variability of cortisol has become "*tacit knowledge*" (43). A negative modality, on the other hand, might interrupt this construction by reopening an earlier black box: 'the devices for measuring cortisol in saliva are not as reliable as these other authors thought'. The modalization of claims is a major part of the collective action that determines *post hoc* the fate of scientific actors; the textual history of science is a history of this practice of disciplinary modification, while technical histories concern the practices of producing inscriptions and performing trials that *underwrite* modalization.

A Latourian study of scientific texts, then, might involve examining the generational structure of the literature: of interest in this method is not only what happens to the facts, but also who cites whom. Since resisting negative modalization is so crucial, an important quality of scientific literature is the fortification of claims and the restriction of objections through strategic "**positioning**" (50). Latour identifies three main positioning tactics: "stacking," which involves folding "figures, pictures, numbers and names" into the text to combine their strength (50); "staging and framing," which involves carefully delimiting the extension of a claim to prevent the reader from responding critically (52); and finally "captation," an old rhetorical term that Latour repurposes to describe the ways in which scientific literature involves citing "so many tiers of black boxes" from the previous literature that the new fact is impossible to dislodge (59). Latour describes these different kinds of modalities with respect to the laboratory's presence:

> [Depending] on whether the other articles push a given statement downstream or upstream, it will be incorporated into tacit knowledge with no mark of its having been produced by anyone, or it will be opened up and many specific conditions of production will be added (*SIA* 43).

Citations by allies will serve to conceal the laboratory origins by pushing the statement towards the black boxes; those by opponents disrupt the closure of these black boxes by calling the credibility of the information back into question. The factual status of a given claim is always in a state of deferral, subject to how it is taken up and put into use by the future generations of the literature. This is the sense in which, for Latour, shared 'reality' is an ongoing and collective construction, and 'nature' is only ever its consequence — not its cause.

In stacking, for example, the intensive laboratory processes and instruments involved in describing nature disappear in the graphical representation of the information that they produce — but this only works if the graphs stand up to scrutiny. Citing this information as factual maintains the order of the instrumental translation and literary interpretation, and so moves the controversy away from the lab; identifying artefacts contests the sense made of materiality, by drawing the reader's attention to the laboratory conditions — to the inscription devices or to the literary devices. Positive modalities push objects into nature, negative ones back to local laboratory contexts. In one case, the spectacle of inscription has an authoritative spokesperson and the names remain stable; in the other case, the credibility of the name breaks down as it is no longer seen to correctly capture, translate, or filter information from noise. Authors of scientific articles use these positioning tactics to keep the reader on their side and thereby prevent negative modalities — this is the basis of Latour's argument that the reader comes pre-installed in every scientific article as "**a semiotic character**" (*SIA* 53). Fundamentally speaking, the reader is the first 'ally' that the external literature is designed to *enrol*; in other words, controlling the reader's

reaction and capturing the reader's interest is the orienting social concern of laboratory life. According to Latour, the rules of this literary gameplay are "simple enough," essentially congruent with "the rules of the oldest politics" (*SIA* 38). With respect to the available facts, he argues that scientists will "do whatever [they can] to the former literature to render it as helpful as possible for the claims [they] are going to make" (37). This conscription of literary resources is organized around the semiotic character of the reader: after all, "it is much harder [for them] to resist a paper which has carefully modified the status of all the other articles it puts to use" (38). Authors become allies when their claims mutually reinforce each other; as more authors translate their statements into the language of these citational chains, a well-disciplined research program takes shape and grows in strength.[24] In Latour's words, facts only emerge under these conditions:

> [They are] collectively stabilised from the midst of controversies, when the activity of later papers does not consist only of criticism or deformation but also of confirmation. The strength of the original statement does not lie in itself, but from any of the papers that incorporate it (*SIA* 42).

As a field captures the interest of more and more readers, it exerts a gravitational force on the proponents of new facts, who come to see this linguistic alliance of reciprocal confirmations as a means to future credibility. The proponents align their claim with these generations of research, and cite them to fortify their own position.[25] As new articles cite old ones, they contribute to the blackboxing of their new allies' own names. Strong scientific facts thus need to be *mutable* enough to be put to new uses later on, without losing their own integrity.

So, how might this all look in the field? After all, the modalization of information does not only occur in the formal literature; statements and conversations are also adjusted in the informal conversational space of the laboratory. In the second and fourth chapters of Laboratory

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[24] See *Science in Action*, 'Bringing friends in' (31-33), and 'Referring to former texts' (33-38).

[25] See *Science in Action*, 'Being referred to by later texts' (38-44).

Life ('An Anthropologist Visits the Laboratory', 'The Microprocessing of Facts'), Latour introduces the heuristic of "statement types" (75); he describes "a five-fold classificatory scheme corresponding to different types of statements" (76).[26] The goal of scientific work is to arrive at higher-type statements, which no longer require reference to the technical microprocesses of literary inscription involved in their construction: the statement then speaks for itself and enjoys the status of a fact. The higher type a statement is, the less conversation it produces at the lab: old facts *are not* controversial, and their origins are no longer important in practice; new claims *are* controversial, and the circumstances, experimental protocols, and disciplinary alliances involved in their production all influence their vulnerability to negative modalization in conversation.

As prospective namegivers shift their constructed object towards publication, they have to think like dialecticians, and consider usability. The scientists here are building facts with an eye to fashion, projecting the semiotic character into their processes of literary production:

> [Attention is] quickly redirected from an item of knowledge itself to an assessment of its nearness to the frontier [of current sensibilities] and its place and time of publication. As a result, the possibility of controversy [is] raised. Clearly, these kinds of exchanges serve an information-spreading function which enables group members continuously to draw upon each other's knowledge and expertise to improve their own. These exchanges help to retrieve those practices, papers, and ideas from the past which have become relevant to present concerns (*LL* 160-161).

In the same way, the semiotic character structures even the instrumental dimensions of laboratory life, as technicians must consider any possible methodological objections. For Latour,

> These are the verbal components of a largely nonverbal body of exchanges during which reference is constantly made to the correct way of doing things. [. . .] In their more elaborate forms, these exchanges concern the evaluation of the reliability of a specific method. [. . .] The apparently technical discussion [. . .] thus comprises cautious probing informed [. . .] by the desire to avoid working on an artefactual substance and by the group's current investments (*LL* 161-162).

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[26] Type 5: simple, taken-for-granted facts; Type 4: facts which include some explicit technical information; Type 3: modalized or qualified facts; Type 2: claims which include the explicit circumstances of their production; Type 1: conjectures or speculations included at the end of documents or conversations to direct further research.

Disciplinary pressures thus come to determine laboratory conversation and experimental technique through the imagined figure of the reader; in order to be kept away from the laboratory, everything must be designed with them in mind. Scientific authors thus work to install the semiotic character into their texts as a perspective from which their claims are already persuasive, and their facts already make sense. This effort will be a success once the names in question have become so authoritative and so 'natural' that their scenes of inscription are entirely inconspicuous; the facts and objects that emerge from these black boxes are *assumed* directly into nature, incorporated into the literary sensibility of the readers as tacit knowledge.

In the first part of the framework, I reviewed Latour's linked concepts of literature and alliance, describing how the credit cycle, which funds and organizes scientific activity, depends on the successful construction of collective literary entities. In this part of the framework, I have so far reviewed Latour's citational account of how these literary entities actually take shape in formal articles and informal conversations, through an open-ended process of modalization. Before moving on to a more philosophical discussion of this literary account of science, it is necessary to review one more conceptual heuristic from Latour's laboratory anthropology: that of "**translation**" (*SIA* 108), from the third chapter of *Science in Action* ('Machines'). I have so far used the term in two senses: first, for the activity of the internal literature, which moves information from its material origins towards its objectified names; and second, for the activity of the external literature, which assembles diverse objects and facts in order to generate as much credit as possible. In both cases, the process of translation involves practical tactics, by which laboratory scientists enlist the allies and secure the resources described throughout these introductory chapters. In essence, this is a question of translating multiple interests into the same

language, such that diverse groups might all invest in a shared concern. By translating disorder into order — either in the internal literature's production of objects, or the external literature's construction of alliances — scientific activity sets up the laboratory as the only reliable site of convergence between the *agency* of material substances and the *interests* of social organizations.

Latour describes five kinds of translation in total: three involving compromise, and two involving a sort of coercion; the tactic a given lab or scientist might choose corresponds to the relative strength of their existing network. In any case, translation enables two things:

> *to enrol others* [both human and not] so that they participate in the construction of the fact;
> *to control their behaviour* in order to make their actions predictable (*SIA* 108).

When allies start arriving at the lab unsolicited, it has succeeded in becoming what Latour calls a '*centre of calculation'* (*SIA* 233). Its instrumental credibility is so strong that *even the scientists* have to pass through its walls to find authoritative answers; and its social credibility is so secure that it can now *lend credit* — in the form of publications, accreditations, or tenured positions — to those who come to further its interests. University departments, well-funded laboratory clusters, and the most important literary journals all enjoy this status and gravity. Because new allies arrive already invested in or committed to their research program, their claims are not likely to be modalized "beyond recognition" in the future literature (*SIA* 108). The objects in the black boxes are safe, as is the authority of the scientists — as long as they remain at the centre.

Not all scientific claims, in other words, are born equal. Depending on the position of a given scientist in relation to these gravitational centres of the disciplinary alliances, their words carry different weight. The final Latourian term I'll introduce in this part of the essay is "*stratification,*" which describes the "asymmetry [that] modifies [. . .] the visibility of a scientist or of a claim" (*SIA* 166). This asymmetry, importantly, is not simply a question of social bias. Insofar as the *distribution of interest* is stratified, that of *resources* is as well, through differential

access to the infrastructures required to construct facts. Stratification is just one more selective feature of the scientific environment that determines which objects survive as factual — and can go on to generate credit for the lab — and which ones simply never come to matter at all.

In the following chapter, on my methodology, I will describe the concrete ways in which I structured my ethnographic activity through the use of these specific conceptual tools — that is, statement types and literary modality, the role of the semiotic character, and tactics of translation. For now, suffice it to say that these tools allow for a deeper and more articulate analysis of the kinds of work scientists carry out at the lab, both as they prepare literature for publication, and as they consider how to leverage laboratory credit to further their professional interests. All of these activities emerge from a kind of disciplinary anxiety about the stability or authority of scientific names. Resources are scarce, and the citational structure of the literature means that the black boxes *might* burst open at any time. At my laboratory field site, scientists are proposing new names and new techniques for making sense of stress — their efforts are best understood through these dialectical procedures for building literary and infrastructural alliances. In the clinic, the concern is less with whether the names are correct in themselves, but rather with how they can be used to enlist new allies and organize new resources for their 'disordered' patients. The clinical concern, in other words, will still be with 'making sense,' or 'producing order' — only the direction is different. While laboratory scientists leave the local context behind so as to redefine the names, clinical scientists start with the names and use them to reorganize the local context.

Part 3. Hybrid Bodies, Symmetrical Anthropologies

But what grants these two kinds of scientific namegivers such a radical freedom of movement? To answer this question, it will be necessary to move beyond Latour's ethnographic

analysis of the laboratory, to the somewhat more philosophical registers of his later studies of science. In this final part of the Latourian half of my framework, I will draw on *We Have Never Been Modern* (1993), in addition to "Give Me a Laboratory and I will Raise the World" (1983), "Where Are the Missing Masses?" (1992), and "How to Talk About the Body?" (2004). For some help in turning the corner towards the clinic, I will bring the Latourian psychology scholar Rasmus Birk into the conversation, using his article "From social to socio-material pathologies" (2016) to think through the implications of Latour's ontological writings for the question of diagnosis. As I unpack Latour's account of the semiotic moves made by scientists to attain their distinctive social and infrastructural mobility, the political dimensions of the laboratory will begin to emerge with greater clarity. Latour builds his arguments for a more responsible approach to critical inquiry directly following from this political analysis — ultimately, it will be my argument that transcultural psychiatry makes important strides in that direction.

The overall trajectory of this story is simple enough: as we've seen, in order for scientists to *assemble* the authority to speak for nature, they *dissemble* the artificial origins of their facts and objects. This move affords them the credit to claim all their resources; but it also constructs sharp divides between the social and the material, between the cultural and the natural, and between the human and nonhuman. Any social actors interested in objective answers have to pass through the lab, which appears to be the only space in which real objects can speak in their own voice. However, at the very heart of the scientific enterprise is the instrumental scene of inscription, which essentially revolves around a hybridity between the *material's performance* and the *scientist's interpretation*. For Latour, this is the radical mobility of the scientist: the right that they reserve to traverse these great divides, which grants them their ability to construct

longer and stronger networks than anyone else. As long as inquiry remains invested in the semiotic divisions of this conservative idiom of science, he argues, its accounts will be *asymmetrical* — either saturated with social and cultural conjectures, or trapped in the confines of a reductive materialism. One asymmetry ascribes far too much to human subjectivity; the other falls into the passivity of nonhuman determinism. In working our way towards symmetrical analysis, our task will be to learn a middle voice, and articulate a new grammar of hybrid agency.

Latour begins *We Have Never Been Modern* by discussing this "asymmetrical [. . .] constitution" of scientific modernity (13-14). According to him, the result of this modernist (or 'humanist') asymmetry is that, just as it constructs a "separation between humans and nonhumans," it also allows for "hybrids [of the two] to continue to multiply" (13). We can map the two stages of the asymmetry on to the two stages of scientific literature. First, at scenes of inscription, the activities of "translation" involved in generating information produce "entirely new types of beings, hybrids of nature and culture" (10). Second, the positioning tactics of the external literature construct "two entirely distinct ontological zones: that of human beings on the one hand; that of nonhumans on the other" (10-11). Good citizens of modernity, invested in the credit cycle of science, have no language for their isolation in this artificial humanism:

> The essential point of this modern Constitution is that it renders the work of mediation that assembles hybrids invisible, unthinkable, unrepresentable. [Yet] *the modern Constitution allows the expanded proliferation of the hybrids whose existence, whose very possibility, it denies* (34).

This asymmetrical consciousness of 'modern' culture is the condition that grants scientists their increased mobility: only they have the authority to make translations and assemble hybrids.

The question remains: what is it about the laboratory environment in specific that enables this hybridity, and the apparently peculiar mobility of technoscience that comes along with it?

According to Latour, the trick is not in some feat of cognitive athleticism that allows scientists to see the whole world at once; instead, it is in *making the world come to and pass through the lab*. This is what grounds and constitutes the authority of those 'centres of calculation': all of the inscriptions, if they are to become relevant parts of the strongest facts, have to move through the same instrumental filter. How do the scientists at the Signature Biobank know so much more about Quebecois cortisol than anyone else? Simple: they are the only ones with the multiplex assay — if a research group in Chicoutimi wants to translate their saliva across the great divide and into human terms, they will have to build a network to connect to that resource. All of a sudden, the scientists at the centre, in Montreal, have extended their mobility five hours north. The networks along which information travels are thin: until and unless the saliva passes through this very narrow filter, the cortisol hidden within is like all the other nonhumans of modernity — mute and meaningless. The realities which emerge from these networks, however, are thick: once it has passed through the instruments of the lab, we moderns come to see and accept cortisol as a part of nature, and its testimony concerning stress — assembled over time, from countless inscriptions — is far more objective, durable, and factual than that of any individual human's.

What is at stake in this scene, in which the "meaningful behaviour of nonhumans" is now the most reliable way to interpret, explain, and adjust the behaviour of ourselves (*NM* 24)?

> Here [. . .] we witness the intervention of a new actor recognized by the new Constitution: inert bodies, incapable of will and bias but capable of showing, signing, writing, and scribbling on laboratory instruments before trustworthy witnesses. [. . .] Endowed with their new semiotic powers, [they] contribute to a new form of text, the experimental science article [. . .]. From this point on, witnesses will pursue their discussions around the [instrument] in its enclosed space. (*NM* 23-24).

In order to understand how this translation has taken place — according to which nonhuman objects come to assume the active voice, displacing human subjects as the explanatory agent —

it will be necessary to trace out the process by which laboratories 'enclose' themselves against society. In "Give Me a Laboratory and I will Raise the World," Latour introduces the concepts of *scale* and *leverage*, using them to describe three tactics or moves that labs must carry out to place themselves at the fulcrum, the centre of action. The first move will be familiar enough — "*capturing others' interests*" (144) — which, as we know by now, involves "giving one of their own names for each of the relevant elements of the [outsider's] life" (145). The second move requires "moving the leverage point from a weak to a strong position" (146). Scientists may not be strong enough yet to defeat stress *out in the world*, but in the "idiosyncratic setting" of the laboratory they are constantly "manipulating new objects and so acquiring new skills" with respect to the particular *elements* of stress that their instruments can reliably isolate, translate, enrol, and control (148). The third move is the most difficult and the most precarious — leaving the lab and "moving the world with the lever" (150). In the context of the medical sciences, this outwards movement takes its first tentative steps in the still-controlled space of the clinic.

It is at this point that the full political activity of science comes into play. After all, "no science can exit from the network of its practice" (*NM* 24): the leverage over newly-defined agents of stress that emerges in their controlled trials can only spread into the world as far as scientists extend the relevant laboratory conditions. Once a discipline establishes itself as "the only credible and legitimate authority" over its new agents, it can "intervene in the daily details of life," rearranging the social order to confirm the scientific order (*GL* 158). Take one example from the literature on stress and physical habits: only after Kirschbaum et al. (1992) have identified the link between nicotine and cortisol, for example, can Steptoe and Ussher (2006), citing them, so authoritatively direct clinicians and politicians to mobilize resources against

smoking. First, the scientists constructed a point of leverage over cortisol by testing the reduction of smoking in the lab; then, they try to extend this leverage by reducing smoking in the world. Kirschbaum, in the first place, is able to identify this agency of nicotine not because he speaks in the native tongue of reality, but because he has the right resources: the radioimmunoassay device to produce the inscriptions, modifying the scale of the problem from thirty-one human bodies to one spreadsheet of data; and the time, money, collaborators, and workspace necessary to stabilize, mobilize, and recombine all the information until the facts resist dissociation.

This last point is crucial. What makes laboratory scientists stronger than anyone else? In their 'enclosed discussions' about all this noisy information, they can make "more mistakes than the others 'outside' who cannot master the changes of scale" (*GM* 164). The political power of the laboratory lies in this special quality, by which the literary activity around its "technological devices" can "invert the hierarchy of forces" (163). Before Kirschbaum's trials, stress was stronger than us; afterwards, we are stronger than it. First, the lab translates the world into its own disciplinary or semiotic terms; then, it sets about extending this discipline by "*transforming society into a vast laboratory*" (166). This effort is successful only insofar as the humans agree that they are "better off appealing to the nonhumans" (*NM* 23); once this constitution is in place, and the "long, narrow networks that make possible the circulation of facts" blend right into nature, then it should be no surprise if "most of the really fresh power [in society] comes from the sciences" (*GM* 167; 169). The scientists and clinicians seem so much more mobile because all of the relevant actors now come right to them — and why not? In their well-constructed networks, they can invert strong forces, invent strong hybridities, and even reorganize the whole scene of social and material activity until things 'make sense' on their terms again.

If we are to follow medical scientists through their labs and clinics and out into society, we will have to embrace the 'work of mediation' that assembles humans and nonhumans into these hybrid associations. Such is the renewed practice of inquiry that Latour calls "symmetrical anthropology" (*NM* 106) — a form of ethnography that interprets human activity by making space in its accounts for nonhuman agency. For Latour, the objects of 'modern' disciplines of inquiry, on either side of the divide, have *always been hybrid*: symmetrical analysis only reveals what conservative sociologies or biologies seem to conceal. Without delving too much into the specifics of this Latourian ontology, I will close this first part of my conceptual framework by drawing out the implications of a symmetrical anthropology of medical science. The goal in this closing movement is to understand how the name-giving activities of diagnosis are *already hybrid in practice*, insofar as they work to "relocate" or "[re]situate the human" within the technological assemblies that emerge from the laboratory and come to structure society (*NM* 136). In order to do so, it will be useful to draw further on Latour's philosophical concept of hybridity, to find some clear language for describing this activity of mediation.

The clearest analytic vocabulary for this relation comes in "Where Are the Missing Masses?" In this essay, he argues that asymmetrical social studies involve a "*law of the excluded middle*," insofar as they cut out the nonhuman agents that mediate between our bodies and our norms (152). These nonhumans, he writes, "*give shape* to humans," by prescribing specific "programs of action" necessary for their use (160). For Latour, nonhuman objects have moral and structural agency in the sense that we are always *organizing ourselves* around them; as we construct our social environments through the use of (and in interaction with) nonhumans, they come to influence or partially determine the horizon within which our own activity and morality

takes shape. The basic mediation at the heart of this hybridity is what Latour calls "*delegation*" (154) — the process by which we humans try to make use of nonhumans to transform "a major effort into a minor one" (154). Scientists are experts of delegation: helping someone feel better is hard, for example, when they have a stress disorder; by leveraging the agency of certain pharmaceutical nonhumans, it might get easier. In this kind of delegation to nonhumans, many objects are brought into association that first proliferated in laboratory or clinical spaces. In this example, the structures of the brain had to be inscribed and interpreted, the reactivity of chemicals had to be trialled and modulated, and proprietary compounds had to be defined and made interesting to resource-holders before any drugs could be mobilized in such scenes.

Latour's argument concerning the social strength of laboratories gives us the resources to understand the practical power of these technological agents. They represent all of the leverage gained over stress through the enclosed laboratory's facilitation of mistake-making, and through the literature's high-pressure refinement of useful facts; it is in this sense that "time is *folded*" in any object which escapes the laboratory context and becomes a part of social life (168). In order to incorporate the strength of these new agents, we humans must make room for them; there is always "a new behaviour imposed back onto the human by the nonhuman delegates" (157). At stake in any scene of delegation, then, is a structural exchange of competence: "when humans are displaced and deskilled, nonhumans have to be upgraded and reskilled" (157). One way to read the clinical space, in Latourian terms, is thus as a site of the "*distribution of competences*" (158). Here, the agency of new laboratory nonhumans finally comes to reorganize and resituate the structure of human agency in practice (158). The job of clinicians, like Latour's engineers (or Plato's dialecticians), is "to continuously cross this divide" as they "transform texts, drafts, and

projects, into things," and into the habits of the user that activates them (158). As technological agents pass through the clinic and into the world, they clearly cease to be simply nonhuman.

Thinking in terms of hybridity thus involves opening up both the hermetically sealed human bodies of biological analysis *and* the immaterial norms of conventional social analysis, in order to find the ways that they are both constituted by "a rich diversity of delegates" (162). In the terms of Latour's theatrical metaphor for science, all the hybrids involved in a scene of delegation are equally "actors, that is, entities that *do* things" (163). Importantly, he argues that the modern claim which holds that nonhumans are better *witnesses* also applies to their reliability as *actors*. This is the basis of what he calls the "deskilling thesis," according to which technoscientists seem to "never rely on undisciplined people, but always on safe, delegated nonhumans" (168). An asymmetrical sociological analysis of science might only critique this reorganization of the scene, pointing to the disappearance of the "embodied skills" it involves (167). It is through such a lens that conventional biomedicine appears so reductive: with its translation of human problems into the material and mechanical terms of the body's physical systems, the medical model thus inherently seems to involve a kind of *dehumanization.*

Symmetrical anthropology allows us to read the situation in a slightly different way. In *We Have Never Been Modern*, Latour writes:

> If the human does not possess a stable form, it is not formless for all that. If, instead of attaching it to one constitutional pole or the other, we move it closer to the middle, it becomes the mediator and even the intersection of the two. The human is not a constitutional pole to be opposed to that of the nonhuman. [. . .] Their alliances and exchanges, taken together, are what define the *anthropos*. The closer the *anthropos* comes to this distribution, the more human it is. (*NM* 137).

If there is simply no coherent way to think of human agency without giving clear and official representation to the nonhuman delegates — if "the human is in the delegation itself" (*NM* 138) — then a scientific practice which openly represents more delegates cannot be dehumanizing on

that basis alone. Instead, as I suggested in the previous chapter's definition of 'humanity', the symmetrical anthropologist would describe this proliferation of hybridities as at worst *nonhumanizing*, and at best as *rehumanizing* — or simply as *reorganizing.* Biomedicine describes human life through the nonhuman agencies of the physical body; its result is more hybridity: the incorporation of new technologies, and the skillful techniques or habits required to make use of them. Symmetrically speaking, this is (or at least may well become) a positively constructive process, redefining the human organism by mediating (and adding to) its agency.

This conceptual structure of symmetry and hybridity requires the new grammar we have been pursuing: in its 'middle' voice, this style of inquiry ascribes the activity to neither humans nor nonhumans alone. Instead, it speaks in terms of "quasi-objects, quasi-subjects" (*NM* 51). Such a practice is fundamentally pluralistic in terms of its analysis: within this descriptive framework, "every piece of an artifact becomes fascinating," as both a site and a determinant of human delegation (*MM* 168). Since symmetrical anthropologists can start from any point along the hybrid networks or gradients that constitute a scene of delegation, they finally have the philosophical mobility necessary to follow scientists and engineers. By giving representation to every participant or agent in these hybrid assemblies, symmetrical inquiry enables more responses to instances of disorder or breakdown. In this pluralism, we are much like those clinicians who, also at home in the middle, move between the lab and its society.

A last set of analytic terms for symmetrical anthropology is found in Latour's article "Where Are the Missing Masses?" In the footnotes to this document, he proposes analysis "in terms of *scripts* [. . .] played by human or nonhuman actants" (176). *Description* involves "retrieval of the script from the situation" (176); in other words, this is the ethnographic

technique of working out the roles that people (or, now, things) play in a given cultural milieu. *Transcription* is the process by which engineers introduce "*more durable*" repertoires or technologies in order to execute or regulate a given script (176) — this concept is very clearly observable as experimenters work to refine research design and protocols. *Inscription*, as I've already discussed at length, involves translating the activity of an object into the much more stable, mobile, and combinable language of information. *Prescription* is the term for analyzing the expectations a given 'scene' imposes onto its actors — captation, for example, is a tactic for prescribing the reader's responses. Acquiescence to this prescribed role is *subscription*, efforts to delimit the scene are *circumscription*, and any attribution of agency is *ascription* (177). Finally, the enlistment and organization of allies and actors is all part of *conscription*. The force a given environment can exert on its conscripts is an effect related to the length of the networks which constitute it — in this sense, the 'scenes' of science enjoy a certain cumulative gravity, and once a scripted space gains momentum, it is very difficult to alter its trajectory or displace its actors.

(II) Critical Medical Anthropology

Part 1. Clinical Medicine, Public Health

In the previous section, I began by articulating the conventional disciplinary structure of the contemporary technosciences, and ended by defining the speculative literary objects of Latour's symmetrical anthropology of science. Relationality, ecology, emergence, and distributed competence were the aesthetic idioms of this proposed form of analysis. Fundamentally pluralistic, this approach to inquiry held that more methods of representation will lead to more methods of intervention, more techniques of redistributing skill and agency. I also began to make the argument that the clinic might be an existing space of highly symmetrical inquiry, balancing

the technical concerns of laboratory life with the political concerns of social life. For more clarity on the political constitution of this hybrid space, I will now turn to Didier Fassin's critical anthropology of the clinic, drawing principally on his *Les mondes de la santé publique* (2022). Although Fassin's body of work emerges from a very different form of ethnography than Latour's, and involves a distinct philosophical program, I will argue in this section that the two may be read together fruitfully. Ultimately, I argue that Fassin's description of public medicine provides clear conceptual material with which to ground my claim concerning the symmetrical constitution and hybrid activity of the clinical space.

In Latour's study of the laboratory, he argues that the process of constructing scientific fact both *relies on* and *results in* concrete networks of social and material resources, and that these networks support the circulation of facts by structuring society in their image. As different kinds of actors or entities form associations, their networks fold together different temporalities and moralities. In the clinical network that supports the fact of psychosis, for one example, linear juridical temporalities are often mediated by the cyclical pressure to clear space in hospital wards, as the daily dosing schedule for administering anti-psychotic drugs also interacts with multi-year longitudinal studies of their efficacy. This coexistence of multiple forms of reasoning relates to what Birk calls the "principle of irreduction" in Latour's work (195); in symmetrical anthropology, many different scales of analysis might be opened up and held together at a given ethnographic site. Fassin, in his study of public health, also identifies what we might call *irreducibility* as a feature of clinical activity. For both analysts, approaching a problem involves translating between the many systems of activity and styles of literacy which constitute it.

In *Les mondes de la santé publique*, Fassin describes how medicine constitutes its objects by weaving together multiple styles of accounting — what we have called the *hybrid constitution* of a diagnostic entity in the clinic. Fassin describes this multiplicity in his own terms as:

> sa construction sociale par les agents, humains ou non, médecins et experts ainsi que les dosages biologiques qui ont conduit à la reconnaissance de sa réalité, de sa gravité et de ses causes; et sa production sociale par des phénomènes structurels mêlant l'histoire, l'économie, le droit, les politiques de peuplement et de logement (*SP* 27-8).

In such a process, each explanatory factor might become the most fascinating and most relevant, but none can ever be fully reduced to any other. In order to follow clinicians around as they traverse their scenes of diagnosis and treatment, Fassin argues that it is necessary to balance substitute readings, so as to get at the diverse histories which underwrite those public-facing accounting practices of these scientific spaces (51). As he wrote in a monograph fifteen years before *Les mondes*, when it comes to ethnographies of diagnosis, it is "the whole of this 'scene' that must be made comprehensible [. . .] from the diverse local vantage points" (*BR* xiv). A conceptual heterogeneity that will be familiar to any good Latourian, then, remains integral to Fassin's critical anthropology of the clinic, and of the diagnostic names in which it traffics.

To the extent that clinical networks are also political networks, the disciplinary 'order' that diagnostic names work to produce is not just scientific or intellectual. These are spaces in which certain social practices or somatic events are named as 'disorder' — and some expressions of disorder 'make sense' to clinicians more so than others. For Fassin, critical anthropologies of the clinic need to consider the *social procedures* by which the *technical conversion* of noise into information generates and sustains *different kinds of order and disorder*: categories which are at once medical and political. At stake in his account of the contemporary clinic is its integration of (or reconstitution by) a second form of medical science, which he calls public health. The two forms, he argues, each have their own styles of producing order and constructing sense:

> Récit d'un côté, chiffres de l'autre. [. . .] Les premiers se servent d'examens biologiques [. . .] ; Les seconds font appel à [des structures environnementaux]. Les uns parlent de diagnostic ; les autres, de stratégies. Deux démarches, deux langages, deux politiques (*SP* 32).

This new double dimension of clinical activity only heightens its heterogeneity. As each approach conscripts its own allies and resources, the status and situation of the patient changes. Under its reconstitution by public health, the clinic becomes a space that is increasingly "normative et administrative" (25); as its representations shift along the gradient between these two disciplinary poles, so do its interventions (which are both against and on behalf of the public). Even more so than for Latour, his account of scientific authority thus depends on an attention to the public authorities that preside over the distribution resources and funds (48).

What specific concepts does Fassin offer for an analysis of the politics of public health within the clinical space? First of all, he describes its new form of diagnosis, as a kind of tool or resource within the "économie morale" of health — one which both structures and is structured by *public sentiment or interest* in a given problem (52). In crises of public health, the first place that diagnostic representation intervenes is in "la production, la circulation et l'appropriation des affects et des valeurs autour [. . .] d'une réalité intolérable et pourtant tolérée" (52). Whereas a laboratory fact needs to cast the reader as a semiotic ally, clinical facts need to enrol as allies everyone who will come to see the patient in their new diagnostic terms — including the patient themselves. The task of the clinician under public health, in other words, is to construct a diagnostic story that might "sensibiliser les pouvoirs publics," so as to mobilize the resources required for reorganizing the relevant "agents sociaux" around the scene of disorder (53). Just as with the laboratory facts, the construction of facts in public health involves a set of persuasive tactics, designed to render a problem as legible, as interesting, and as urgent as possible. In both cases, scientific persuasion consists in presenting the relevant allies with *a way of making sense*.

As medicine moves away from treating individual patients, and towards treating environmental problems, it increasingly assimilates or utilizes the strategies of public health. The distinctive feature of this movement is the adoption of *exposure* or *risk* as an element of the diagnostic category.[27] Public health literature reassembles and reinterprets clinical inscriptions, until the problem is no longer tied to "malades" in the local context of the clinic, but is instead constituted through the calculation of probable 'cases' or gradients of exposure in the public environment (36). Through this *extension of the domain* of a given problem, the contemporary discourse of health extends its credible relevance to more allies, and so increases the hybridity of its resources (36). In the clinic, the doctor has authority over the bodies and habits of their patients; in the networks of public health, they can intervene in environmental structures as diverse as housing conditions, utilities infrastructures, labour practices, sanitary behaviours (26) — their authority may come to concern the whole social and material arrangement of our lives.

The Latourian glimmers in this study of medicine should be clear. Consider the language with which Fassin brings the first chapter of *Les mondes* to a close:

> [Un] problème de santé publique n'existe pas en soi mais a besoin d'être construit pour devenir tel qu'on l'appréhende, [et] un problème de santé publique n'est pas un simple affaire de nature et de biologie mais qu'il se produit d'actions humains, de rapports de pouvoir, de jeux de forces, et de systèmes de valeurs (54).

Analyzing the *social construction* of a public health problem involves following its semiotic transformation into a well-disciplined diagnostic object — here, we can make use of Latour's ethnography of scientific literacy. Analyzing the *social production* of the same problem involves following the diverse actors, agencies, and historical trajectories that contribute to its ongoing emergence — here, we can draw on Latour's symmetrical anthropology of hybrid networks. Ultimately, as I describe both my laboratory and clinical field sites, Fassin's framework for a

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[27] See *Les mondes* 33-35, and the book's second chapter, 'La verité du chiffre' (59-100).

critical reading of public health will allow me to describe with greater clarity the choices that my participants make, as they work to name and frame the problem of stress. Latour's propositions for symmetrical science allowed us to understand the broad hybridity of transcultural psychiatry; Fassin's articulation of public health will allow us to question the effects of this kind of authority.

Part 2. Statistical Asymmetry, Ethnographic Storytelling

In the first part of this second half of my framework, I drew some philosophical connections between Latour's concept of hybridity and Fassin's concept of public health. By expanding the authority of the clinic, the discourse of public health lengthens and diversifies its potential networks of resources. What results is a scientific practice — a regime of knowledge — that troubles the distinction between social and material reality, as clinicians use technological leverage to intervene directly in political affairs, all in the name of medical fact. We might be tempted to read this shift positively: in this scene, it seems like science is going symmetrical, disavowing its allegiance to the modern constitution that keeps humans and nonhumans apart. In this second part of the section, I will articulate Fassin's critique of statistical and culturalist forms of public health, so as to check this Latourian optimism; however, following Fassin's account to its conclusions will give us the resources we need to imagine a better clinic. To the extent that clinicians attenuate their use of statistical power with a careful attention to situated stories of distress and resilience, they resist the disciplinary pressure to leave local context behind.

Fassin develops his critiques of public health by first drawing our attention to the noisy complexities that its statistical mode of ordering accounts tends to conceal — in Latourian terms, we could say that he critiques the *methodological asymmetries* that result from this *disciplinary blackboxing* of *statistical objectivity*. He describes the political "volonté de quantifier" that

underwrites the conversion of physiological and psychological suffering into information — that data which is required for the credible production of authoritative "faits sociaux et sanitaires" (18). Describing it as a signature of modernity (20), Fassin links this new literary sensibility in medicine to the political endeavour to conserve social authority. In networks of public health, medical science and the state are tightly linked as allies: medical *representations* of health are grounded in the governmental *interventions* that they authorize. If the interventions work, the science remains credible; if the science is credible, the interventions are justified. This is just another credit cycle, and in this game too, statistical power is the strongest around. Remember Latour's rule: quantitative information is highly stable, combinable, and mobile — as long as the relevant players accept its validity, it can support more translations than any other language.

Indeed, according to Fassin, the statistical gesture in public health translates the entire world into an "environnement malsain" (30); anything that can be made to speak in the quantitative language of risk becomes a relevant 'factor', an object or property of the medical discipline. In this approach, systematic testing efforts lead to "l'invention de nouveaux cas" (33); this is the very same process of "redéfinition" by which new objects are constituted in the laboratory (34), and it has the same result: "l'invention d'une nouvelle réalité" (35). As medicine moves towards this statistical reality, and invests more in its reproduction, the site of diagnosis shifts from the local clinic into centers of calculation (42). The resulting "dissociation entres cas et malades" reflects the now-familiar process by which facts are isolated from material in the laboratory literature (36) — the same kind of statistical representation that allowed scientific objects to exist *out there* in nature now grants increased independence to diagnostic constructs. With respect to this process of constructing order, Latour remains neutral; he does, however, lay the groundwork for a critique of scientific construction, to the extent that he identifies the role of

selective pressures in influencing what gets left out, or what gets interpreted as noise. It is just on these grounds that Fassin builds his critiques of the statistical rationality of public health.

There are two main elements to Fassin's argument: first, his critique of the process of quantification itself; second, his identification of discriminatory pressures in the interpretation of statistical objects. The first element is straightforward enough: to begin with, the statistical black boxes of public health, which determine so much "en termes d'allocation de ressources," are simply not as stable as they seem (70). Thresholds change, calculations vary, and results shift, as statisticians settle controversies of method and interpretation. Fassin also notes a basic exclusion pressure inherent in this approach: "on ne quantifie que ce qui est quantifiable [. . .] et une partie importante de ce qui caracterise [. . .] les activités humains échappe à la quantification" (72). In effect here is the *technological* form of asymmetry: disproportionately, it is the nonhumans — those agents who can reliably pass through the inscription devices and straightforwardly enter into the objective realm of pure numbers — who get to 'count' in statistical accounting.

The second element is closely linked to Fassin's argument about the disciplinary alliance within public health to social power. Statisticians with social biases will encode them — as 'artefacts' — into the statistical objects that come to compose medical reality. Fassin describes this mistranslation, misinterpretation, or misrepresentation as 'practical culturalism':

> [Il] s'agit d'interpréter les conduites de personnes a partir de leur altérité […], ce qui permet de rendre compte d'un ensemble de faits posant problème […]. Ce culturalisme pratique est ainsi une idéologie défensive souvent mobilisée par les agents de santé publique lorsqu'il s'agit de rendre compte de l'échec de programmes dans des sociétés dont ils ne sont pas familiers (46).

In other words, what is at stake here is the *sociological* form of asymmetry. When these human actors — who are not aligned with *normal morality* — get sick differently, they generate *noise* or *disorder* in the accounts of public health. Because they do not fit into the standard *program of action* that prescribes the interactions between well-disciplined humans and nonhumans,

practical culturalists ascribe this *statistical abnormality* to the mere fact of their social difference, or *moral abnormality*: in these cases, their agency as social beings counts too much.

For Fassin, the reality is more complicated — which is to say, more concerning, more interesting. Citing anthropologist Vincanne Adams, Fassin argues for "un retour à *storytelling*" in the clinical construction of social and material problems (82). Importantly, this argument is not oppositional, but additive: the translation of these histories into storytelling does not displace statistical inquiry, so much as it grants accountants the mobility required to access "les espaces non couverts par les nombres," thereby inviting "plus de rigueur dans l'analyse et l'interprétation des données" (82; 83). I argue that this call for storytelling as a dimension of clinical inquiry represents a principle of symmetry in Fassin's anthropological approach to diagnosis, a middle voice between 'positivism' and 'relativism' (94). As his argument runs, diagnostic names work better when they are supported not just by nonhumanizing figures, but by human stories as well. However, since he also rejects the humanist asymmetry of practical culturalism, these are always stories of hybridity — stories of *humans inhabiting environments that act back on them*. Here, Fassin rearticulates a Latourian lesson in the context of clinical medicine: sensitive inquiry emerges through a plurality of perspectives, when we are able to consider *ecological stories* of the agency, competence, stress, and resilience of both humans and nonhumans.

Part 3. Critical Problems, Patient Resistance

We now have a clear way to frame the public health clinic symmetrically, as a literary site which reorganizes both bodies and environments, habits and collectives. This clinic generates its leverage by *identifying sociomaterial problems* and *administering sociomaterial programs*, both constructing order and producing it through a credit cycle that links political and disciplinary

interests. In this assembly of diagnostic order — as a naming process that transforms affective subjectivities *and* sociomaterial objectivities — we have also found blind spots or asymmetries, disciplinary limitations that result in a failure of full representation and proper intervention. In this final part of the conceptual framework, I will explore Fassin's account of the disparities and precarities that these asymmetries of public health produce — in terms of differential access to delegates or resources, and differential representation within networks or alliances of care. This analysis concerns the political implications of diagnosis as a namegiving practice: if these names have the power to reconstruct the world, then who has the authority to name becomes a critical question, and the quality of their names becomes an important site of sociomaterial controversy. Finally, the clinical spaces and encounters in which these names are put into use — assigned and assumed, modalized and reinscribed — become a vibrant matter of anthropological concern.

In the third chapter of *Les mondes*, Fassin describes the forces at work in crises of objectivity, in which diagnostic names "se redéfinissent, où elles se trouves contestées, où memes elles ébranlent les certitudes de la medicine" (105). This dialectical process of redefinition often consists less of intradisciplinary "percées diagnostiques" than of "restructurations sémantiques et revendications semiologiques" (106). In such cases, this reconstruction of the patient as a new kind of semiotic character often results from "la mobilisation d'autres acteurs [que les médecins], notamment les malades" themselves (106). In these mobilizations, human actors that have been excluded from the orderly accounts of public health (and thereby from the political networks to which that order is linked) form their own alliances: they *resist conventional diagnostic authority* to *reconstruct their own sociomaterial reality*. In identifying the capacity of these 'undisciplined' actors to shift the balance of forces in their favour, Fassin touches on the Latourian principle that scientific nature is only ever a consequence of how the controversies play out: "reality," as

Latour writes, "is what resists" (*SIA* 93). If the allied patients resist dissociation with more strength than the conventional names, they *win the right to name themselves*.

In explaining this process of contestation, Fassin makes reference to the same concept of the ecological niche as Birk, in his review of Latour (cf. Birk 201): a diagnostic name is 'useful' when it results in a sociomaterial arrangement that *shifts burdens away from its users*; if, however, the niche constructed by a given diagnostic object fails to make proper sense of its users — if the zone of order that it circumscribes results in their exclusion, or even renders them illegible — then the contours of that niche may become subject to organized resistance (*SP* 110). This process, according to Fassin, consists of resolving an "impossibilité du dialogue," by translating between the languages of *culturally-situated subjectivity* and *medically-authorized objectivity* (117). As we learned from Latour, translation is only a question of generating enough 'interest' and 'credit', or constructing enough 'mobilizations and alliances', to convince the scientists to displace their own names in favour of the new propositions (cf. *SP* 117): eventually, with enough social pressure, a new "choix du mot" will emerge from the counter-laboratories, backed up by "l'établissement tardive de preuves scientifiques" (119; 118). Once the new word is spoken by the scientists in their centers, its own institutional credit cycle will begin to unfold.

Latour's concept of the literary credit cycle gave us the resources we needed to interpret and understand the choices that scientists make with respect to their definitions of orderly names; Fassin's writing on this *credit cycle of semiotic resistance* demonstrates the choice that faces the users of names on both sides of the clinical or diagnostic encounter. If the name works, the receiver of a diagnosis *learns their body through the niche that the name organizes*, *subjects themselves to the authority of the clinician*, and *reorganizes their subjectivity through the delegations that they prescribe*; if the name fails to capture their interest, they will *resist its*

objectivity, *mobilize against its delegates*, and the diagnostic order will *never fully materialize*. Symmetrical anthropology allows ethnographers of the clinic to make sense of this interaction between social action and scientific materiality; symmetrical science, for its part, would allow clinicians to "ratify in public what is already happening," and to use names that correctly represent the hybridity, irreducibility, and plurality of the body and its delegates (*NM* 144).

Because naming controversies disrupt the representative authority of received names — that is, they negatively modalize existing diagnostic constructions, calling into question the credibility of received divisions between fact and artefact — they suspend the material reality and social authority of scientific 'nature'. Importantly, as Fassin argues, when there is no order to be found in nature, interpreters will turn back to culture; the black boxes of material empiricism will be swapped out for those of practical culturalism. In his own words:

> faute de pouvoir nommer un diagnostic, [les experts] rejettent la réalité de la maladie, ou lui cherchent des mécanismes psychologiques, voire des raisons culturelles (127).

Where patients ally together against diagnostic discipline, the moral economy of public health intensifies into a political economy, under which certain bodies make more sense, solicit more resources, and simply seem to matter more than others. Those bodies that are aligned with and well-represented by the semiotic figure of statistical 'normality' enjoy the credit of objectivity, and their problems register easily in the accounts of public health. Other bodies, lacking both representation and the resources necessary to construct it, are reduced to the empty account of their own abnormal suffering; beyond the natural order of diagnostic reality, the problems of these patients can only be explained by reference to their unmediated and interior subjectivity.

The role for critical medical anthropology, according to Fassin, is to intervene on behalf of the nameless or the misnamed, to find the critical material necessary to enrich our diagnostic accounts, and to bring these precarious bodies back into the fold by making their problems make

sense again. Where anthropological and medical discourses converge, translations between the laboratory, the clinic, and society might become more symmetrical and less exclusive. In a case study of infantile saturnism in his first chapter, for example, Fassin argues that only by combining statistical literacy and sociological sensitivity could interpreters of public health account for the increased incidence of diagnosis in migrant communities (*SP* 50-54). By first defining the physical agency of lead, then identifying the economic geography that concentrates lead paint in underfunded housing, and finally reading this differential spatiality into the cultural history that excludes the 'exiles' from resource networks, symmetrical accounting enabled a new description of this public health crisis — one which finally accounted for the mediations between social class and material habitat that constituted and structured it. By constructing its account through this sensitivity to mediation across explanatory levels, critical anthropological medicine displaces asymmetrical practical culturalism, substituting the reading that ascribed the crisis to immoral individual habits with one that ascribes it to immoral sociomaterial disparities.

In *L'éspace politique de la santé*, Fassin begins his critical history of public health by identifying the field's three major axes of activity:

> Inscription de l'ordre social dans le corps, légitimation de l'action des thérapeutes, gestion collective de la maladie, trois figures par laquelle le pouvoir se manifeste. [. . .] L'inscription de l'ordre social passe d'un marquage des corps par la violence à *une incorporation de l'inégalité devant la maladie et devant la mort*. Le pouvoir de guérir se sépare des autres [. . .] et donne naissance à *un champ médical différencié*. Le gestion collective de la maladie devient *gouvernement de la vie* (3, emphasis mine).

In the context of inquiries into the health sciences, any method of ethical problematization involves taking careful account of these three historical transformations of health and care.

My ethnographic methodology will thus involve a special sensitivity to the differentiation of bodies by illness. For Fassin, this process of differentiation is what constitutes the body as an "entité politique," and so it is impossible to conduct research on such processes without a

politically-inflected ethical sensibility (*EP* 20). Of course, spaces of transcultural psychiatry — especially the CESAR's inquiries into allostatic load — are exactly concerned with articulating (in cultural terms) different kinds of illnesses, different therapeutic methods, and different forces or dynamics of social inscription in the body. Methodologically speaking, Fassin identifies three vectors of political power in spaces of health science:

> Si l'on retient comme définition de pouvoir ce qui permet aux individus et aux groupes d'agir sur les hommes, sur les choses, et donc sur le cours des événements, *le geste* par lequel une personne en [aborde] une autre en lui jetant un sort [diagnostique], *l'acte* par lequel un thérapeute, guérisseur ou médecin, délivre le malade de sa souffrance, ou encore *la décision* par laquelle un ministère de la Santé rend obligatoire le dépistage […] relèvent tous de manifestations de pouvoir (16-17, emphasis mine).

I take up these conceptual categories — the diagnostic gesture, the relieving act, and the legislative decision — in my ethnographic analysis. In my note-taking practice, I seek to frame and interpret the gestures, actions, and decisions of my informants according to these conceptual categories, though certainly without presuming to assert any higher authority or critical expertise than my participants themselves. Instead, my goal is to articulate certain features and qualities of their activities, so as to contribute to their ethical project of problematizing stress and distress.

This approach accounts for the ways in which certain political structures lead to differential exposure to precarity or vulnerability: here, critical medical inquiry draws on the public health discourse of environmental risk, but still remains grounded in the local contexts and historical trajectories out of which health problems emerge. Alongside statistics, storytelling; alongside physiology, society — only through a pluralization of their modes of inquiry could interpreters of the body properly represent something like this "'écologie raciale du saturnisme'" (224). For Fassin, the exclusive logic of state power over borders is just one example of the social production of public health problems: as a result of its deeply asymmetrical constitution —

in which certain communities are reduced to the mere fact of their social difference — this kind of *political geography* produces a *moral ecology* in which some bodies matter more than others.

In this chapter, I have argued for a synthetic reading of Latour's study of science and Fassin's study of public health. In the coming chapters, I will use their *descriptive analysis* of these related activities to frame my own interpretation of transcultural psychiatry's spaces of research, education, and practice. An anthropology of clinical medicine also benefits from the *critical analysis* of both authors, which cast the clinic as an inherently political space, one that mediates between the laboratory and society. Fassin, in his critique of the scientific accounting of the clinic, argues for the incorporation of anthropological storytelling; this looks very much like Latour's call to symmetry in reverse, which held that sociological accounting would benefit from an attention to the nonhumans of science. Both authors, then, argue for a form of inquiry that is philosophically ecological, in order to interpret and understand the emergence of knowledge practices and health problems that link diverse actors across sociomaterial networks. As I turn in the coming chapters to the literatures and practices of transcultural psychiatry itself, I will read them against these propositions for a better science; ultimately, I will argue that, through its work to incorporate anthropological sensitivity into the technoscience of mental health, and to redefine stress as an ecological concern of public health, this body of literature clears the space for a morally-charged technical practice of *symmetrical namegiving* and *hybrid reorganization*. But first, in the next chapter, I will discuss the materials, methods, and ethics of this field study.

Chapter 4. Methodology and Ethics

In the Attic Greek of our friends Plato and Aristotle, which is the origin of our vocabulary for methods and methodology, the word *méthodos* refers to the activity of 'following after' or 'pursuit'. The word comes from the conjunction of *hodós* ('road, way, or path') with the prefix *meta* ('with, beyond, across, and between'). For the early philosophers of Athens, with their bold goal of working out a systematic account of the entire universe, method was a means of keeping thought on the right path — an organizational strategy for linking clearly the point of departure to the ultimate conclusion. By the time of Aristaenetus, a neoplatonic author of epistolary love stories nearly a millennium later, *méthodos* had acquired the additional meaning of a 'trick or ruse' — Aristaenetus writes in his letters of the 'erotic methods' by which lovers might try to charm and capture the beloved objects of their pursuit. It will be perhaps useful to hold together these two originary definitions of method as I present my approach to research in this chapter: methodology as a kind of sober way-finding and trace-making, designed to render the pursuit of knowledge both regimental and replicable; and methodology as a set of affective tricks and traps, designed to shape the desired object according to the will and interest of the imaginative lover.

To the extent that methodology involves the establishment and articulation of a path for thinking, we are in a good position to consider how I assemble the methodology of my field research out of the conceptual sources that guided my thinking in the previous chapter. These names will not be unfamiliar at this point. I inherit most of my practical methods for observation and analysis from Latour's early works on science studies, most notably *Laboratory Life* and *Science in Action*. The ethical dimensions of my methodology come primarily from a reading of Latour's later works *We Have Never Been Modern* and "How to Talk about the Body?" alongside

Didier Fassin's *L'éspace politique de la santé* (1996), and *Les mondes de la santé publique*. In other words, in this project, the framework structured the fieldwork — coming from my training in the philosophy of science, I approached my field sites and conducted my ethnography with certain conceptual tools already in hand. Much of my methodology for research involved using these existing heuristic categories to organize my observations and interventions; this being the case, much of my approach to gathering data is deductive in nature. At the same time, the framework I presented above consisted of a synthetic reading of two different fields in anthropology; to the extent that this synthesis was guided by an ongoing interrogation of my field notes, the research has an important inductive element as well.

Furthermore, as much as I have already suggested that transcultural psychiatry draws on anthropological methods, I also borrow methods and sensitivities from the researchers that I worked with in the field: Laurence Kirmayer's account of cultural affordances changed the way I approached participant observation,[28] and Eric Jarvis' descriptions of the Cultural Formulation Interview (CFI)[29] were very influential to my development of interview guides, and to my overall attitude or comportment as an interviewer as well. Because this project was my first experience in performing ethnographic or field research, I relied heavily on this assembly of existing methods to make decisions about collecting, organizing, and prioritizing different kinds of data. Ultimately, however, the approaches I adopted in the field and the findings I have included in my analysis are only one way of exploring my field sites, a matter of my situated judgement about how to best construct an account of this science. The primary criterion of inclusion or exclusion

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[28] See Ramstead et al., "Cultural Affordances" (2016).

[29] The Cultural Formulation Interview is one of the main methodological innovations of transcultural psychiatry as a distinct subfield; it was adopted as a recommended practical technique in the *Diagnostic and Statistical Manual-IV* (DSM-IV). The CCS uses a slightly altered version of this interview in their case consultations. It is in part based off of the McGill Illness Narrative Interview (MINI), authored by Danielle Groleau in collaboration with Kirmayer.

in this document, in other words, is whether or not I found a given fact or event to be part of an interesting and responsible story — one which articulates some of the underrepresented elements of these scientific endeavours, making intelligible what was before unintelligible.

Latour's most comprehensive prescription of a methodology for laboratory ethnography comes in *Science in Action*. As appendices to the text, he lists seven 'Rules of Method' and six 'Principles' for science studies (*SIA* 258-9); I attach these appendices unaltered at the end of this thesis. The rules of method call for an anthropology of science "*in action* [. . .] before the facts and machines are blackboxed" (Rule 1); similarly, Latourian anthropologists judge scientific claims and technologies not on the basis of "*intrinsic* qualities," but rather through "the transformations they undergo *later* in the hands of others" (Rule 2). Symmetrical anthropology, that is, studies science as an historical and collective process, where scientific reality is not settled and stable, but rather undergoes an indeterminate process of iterative reconstruction. The third and fourth rules hold that the controversies involved in this ongoing construction cannot be explained by reference to the essence of either nature or society; the apparent stability of those explanatory factors is, in fact, an effect of the controversy getting settled — not the cause. What settles controversies are "the efforts to enrol human and non-human resources" (Rule 4); in other words, whatever happens to get involved in the processes of enrolment is "what technoscience is made of," so no clear exclusion criteria can be developed before entering the laboratory (Rule 5). The final two rules of method concern cognitive factors, of which Latour is broadly dismissive. Rationality, he argues, is an effect of longer and stronger networks (Rule 6), and the only truly special quality of the scientific method is its obsessive assembly of inscriptions (Rule 7).

There is an important methodological implication of Latour's understanding of this activity of constructing sense and order: the major concern of science anthropology is to describe

the situations and processes by which *material things become socially articulate*. Insofar as science involves a movement towards *literary articulation*, it is possible to approach scientific ethnography through an attention to the *transformations of literary entities*. The emergence of certain literary figures — stress, cortisol, the diagnostic categories — is the rising action of my ethnographic drama, because it is around these things (as they are named and represented in scientific fact) that the effort to construct networks of sociomaterial resources will be organized; the scientists themselves are important, but I interpret their activity as in service of (or in relation to) these powerful names. In other words, insofar as the social extension of science involves training diverse actors for participation in this kind of materially-constructive literacy, I primarily approach the scientific environment as one which generates sufficient pressure or offers sufficient incentive to *adopt or conform to its literary sensibility*. So, beyond the general ethnographic toolkit — now widened to account for the habits and activities of objects as well — what are the methods that Latour provides to carry out this work of tracing the disciplines, measuring the controversies, and navigating the networks which structure this specific kind of naming?

There are two elements to a study of science in this program: first, the ethnography at the laboratory or clinic, where the science in action takes place; second, the literary analysis that grounds an interpretation of that scientific activity, by situating it in its disciplinary context. In other words, although Latour directs the ethnographer away from the *blackboxed* entities of the formal literature, the activity of *blackboxing* is of course still determined by literary sensibilities. Latour's methodological rules direct the ethnographer towards the site of scientific action, but his interpretive precepts frame this site as one which facilitates a collective and continuous effort to construct sensible accounts by producing orderly objects and definitions. The most relevant features of scientific activity, then, are those situations in which the categories at play in a given

research program are subject either to modification or solidification. The articles or the solidified literary objects are no longer the end-all and be-all of science, but they are one very important expression of scientific activities and sensibilities. The framework I developed in the previous chapter serves as a kind of conceptual code against which it is possible to read and interpret, in these terms, the ethnographic material taken from the field sites and their respective literatures. In this research, the methodology is designed for a study less so of the scientists themselves than of their names: when they are given, how they are chosen, and why they are changed.

Part 1. Materials and Methods

As I described it in Chapter 2, my research consisted primarily of ethnographic fieldwork carried out at three distinct organizations of transcultural psychiatry — the educational space of the DSTP, the virtual space of professional consultation at the CCS, and the research and training space of the CESAR. My main involvement at the DSTP was my attendance of the three-day 'Culture, Mind, and Brain' (CMB) workshop (approximately twenty research hours); at the CCS, I attended and participated in twelve consultations between June and December 2023 (approximately twenty research hours); at the CESAR lab, I worked as a collaborator on the STARS project between September and December 2023 (approximately sixty research hours).

My primary research activity at the DSTP and CCS was note-taking — through which I recorded the minutes of the seminar discussions and case consultations — and question-asking, through which I participated in these conversations and solicited relevant clarification; at the CESAR, my involvement was more integrated with the lab activity, where I both participated in the regular meetings of the research team, and underwent a series of training protocols in order to contribute directly to the advancement of the STARS. My field notes from this site thus concern

not only discursive information, but my experiences in practicing the technical dimensions of scientific research as well. In addition to this participant observation, I carried out interviews with three researchers affiliated with the DSTP, the directing psychiatrist of the CCS, and the director of the CESAR lab. The final major methodological component of my research was textual analysis — each of these sites circulated different kinds of literature, and each one was geared towards the production of its own kind of literary artifacts. At the DSTP, I analyzed the educational materials; at the CCS, the primary literary documents were the almost-Aristaenetean 'letters of support' concerning specific cases; at the CESAR, the main texts were the research publications, as well as the various protocol documents and data archives of the STARS project.

For the participant observation portion of this methodology, my ethnographic process involved two related forms of attention: first, to the *technical activity* at the laboratory and clinic; second, to the *technical conversations* between participants about this activity. In both cases, I recorded my observations in a field diary, closely following Latour's example from the introduction to *Laboratory Life* (cf. 15-16). With respect to the technical activity, I organized my note-taking in an effort to define the *scene of inscription* and the *process of blackboxing* — in other words, to follow the continuous statistical or literary transformation of the inscribed information into defined objects. With respect to the technical conversations between participants — the laboratory, clinical, and classroom meetings in which my participants openly discussed their activities, shared information, and distributed work among themselves — I directed my attention more towards discourse than practice, using Latour's heuristic of *statement types*. Whereas my observation of the technical activity was organized around *defining the scene of inscription*, my observation of these conversations was organized around *the modalization of statements*. Here, I was concerned less with the experimental or therapeutic protocol itself than

with the seemingly informal social processes by which noisy information is transformed into orderly accounts and literary products. When these meetings took place, I supplemented my note-taking with field recordings, which I later reviewed during preliminary analysis and writing.

These observational practices were supplemented by both formal and informal interviews with participants, in order to collect data on how they understood their work as contributing to this scientific production of facts. In my interviews, I followed a loose interview structure — the guide is attached to this thesis (cf. *LL* 16). The questions included in the guide mostly concern the participant's perspective on their own work, and ask them to situate transcultural psychiatry within the disciplinary structure of the sciences. I also used these interviews to solicit, in a bespoke manner, any necessary clarification concerning technical details of the laboratory or of clinical procedures and protocols. In all instances and forms of participant observation, no effort was made to conceal the observational role. I openly and directly discussed preliminary drafts and my thematization of activities and conversations at the field site with participants (cf. *LL* 16). Preliminary analysis began soon after initial participation. At the DSTP and the CESAR, it was possible to carry out this work during my placement, because desk space was made available to me at the field sites themselves. Due to the virtual nature of the CCS space, my analysis of field diaries was more separate from my direct involvement in the clinical environment. I refer to these field notes throughout my discussion by the volume and page number of the field diary.

My ongoing and embedded analysis of these observational field notes — the main source of my ethnographic material — followed along the lines of the two distinctions outlined above. First, as I became familiar with the technical activity through participant observation, I described and analyzed the features most relevant to my research, using the Latourian terminology and conceptual architecture of sense-construction and order-production. Second, as I also became

more fluent in the protocols and procedures of the field site, I participated more actively in the discursive meetings, thereby directly soliciting information specific to my research questions. I continuously analyzed the contents of these meetings, coding my notes both in terms of their internal themes, and the relevant concepts from my framework. The notes contained in my field diaries thus consists of two distinct kinds of information: first, the initial inscriptions concerning the field site, in the form of minutes-style observations and quotations; second, the preliminary translation of these noisy inscriptions into a more refined conceptual analysis. My reflections on my own experience — working as a technician in the laboratory, or participating as a member of clinical and classroom activities — provided a further source of data. The parallels between Latour's description of scientific literature and this form of ethnographic literature should not be overlooked: like my scientist-participants, my method of accounting fundamentally involves constructing sense and order through an intensive and ongoing refinement of information.

In addition to the field notes and analytical observations, I amassed a wide range of documents relevant to each field site. Drawing on Latour's distinction between 'internal' and 'external' scientific literature, I divide my document analysis into two parts: first, the constitutional scientific literatures that organize or otherwise contextualize the daily activities of their respective spaces; as well as the journal publications or literary products which leave these field sites and move outwards, so to speak, into society. Second, the 'internal' documents and manuscripts that actively circulated within the field sites. At the DSTP, the 'internal' type of text includes educational materials, and formal documentation of the division's educational program; at the CCS, it includes those documents that were shared or referred to in clinical consultations, as well as official descriptions of the service's mandate; at the CESAR, I enjoyed unrestricted access to the majority of the laboratory archives, draft papers, and other protocol documents.

My treatment of these internal documents is integrated into my ethnographic reports on the sites in which I encountered and read them; my analysis of the constitutional external literature is the subject of the next chapter. My selection process for these external literary products involved open discussions with the directors of each field site to determine which articles or letters would be most relevant to my research question. All three of these scientific spaces are highly prolific in terms of publication in their own right; often, the most relevant constitutional articles were those that had previously emerged from my own field sites. After gathering this pool of potential external documents, my analysis consisted of initial readings, follow-up discussions with participants, and the eventual selection of exemplary products from each major site. I based this final decision both on the degree of the document's relevance to my research, and my degree of literacy with respect to its technical contents.

I analyzed the documents from my field sites using a bespoke protocol, essentially interpreting them as a form of genre-bound and culturally-specific text. This strategy allowed me to both comprehend the content of the article or paper, and to situate it in a wider disciplinary context or literary sensibility. It also follows from the claim, proper to both science studies and transcultural psychiatry, that there is some distinct and identifiable 'culture' of science — one which varies across local contexts. I read each text in three ways: first, to understand its technical information; second, to interpret its literary devices; and third, to identify its blackboxing modalities. In other words, I describe *what kinds of objects they name*, *how they persuade the reader to modify or solidify existing names*, and *the citational chains or research programs in which they participate*. By structuring my analysis of both the field site and its literature around the generational use of names in this way, I use Latourian concepts to build an account of how a specific form of scientific literacy exists in relation to the activities of the laboratory and clinic.

In both my participant observation and my document analysis, my concern is to describe and analyze the processes by which the body and its systems are reconstituted and redefined through the production of new objects or definitions, and the construction of new accounts about them. This methodological sensitivity involves an attention to the modification or solidification of statements both in the laboratory or clinic through the note-taking practice, or of claims in the literature through the textual analysis. I organized my inscription of the field site, in other words, around these processes of modalization. I gathered this information about the modality of definitions in order to describe social processes of translation or negotiation, through which scientists work to enrol new resources and control new allies. In my observations and interviews of lab members and clinical actors, I focus especially on the indications these informants offer about what kinds of translation they are attempting: for example, while students must conform to the interests of directors, professional scientists are much more likely to ask each other to introduce detours into their programs for the sake of pursuing some mutual benefit.

In review, my methodology for research involves assembling the analytic structures and conceptual terms from Latour and Fassin to describe what happens in the various scenes of action of transcultural psychiatry in Montreal — its classrooms, consultation meetings, and laboratories. A comprehensive list of these concepts can be found in the appendix, alongside a summary of my fieldwork practices and protocols. The methods Latour offers for identifying kinds of statements was the primary technique I used in my textual analysis; the analytic vocabulary of scripting and translation enabled me to organize and interpret data as I undertook my participant observation, and shaped the kinds of questions I asked in my interviews. This methodology blends flexibility (insofar as all kinds of objects and activities can be under

consideration) with structure (insofar as they are considered as part of a scientific effort towards factuality, with clearly identifiable features and stages). My research protocol brings together these conceptual structures to trace out the processes that link the production of new objects in the laboratory to the construction of new networks around the clinic; first, I observe the articulation of new names, then I observe the ways in which existing names are put to use.

Part 2. Ethical Concerns

I am aware of three main ethical dimensions of this project. The first is related to my research methodology, and concerns the kinds of bias involved in its design and application. Bias is a part of any research effort, but this problem seems especially salient in qualitative analysis like ethnographic anthropology — particularly in its more deductive forms. The second ethical dimension of the project concerns the challenges posed by researching researchers, especially those engaged in the human sciences and in their studies of vulnerable populations. Psychiatric research has a fraught ethical history, and the field today is dense with lively ethical quarrels and quandaries; my inquiry into the field tries to remain alert to this fact, and to recognize where my claims might be adding noise to sensitive conversations. The third dimension involves the issues raised by Fassin's problematization of health science: if transcultural psychiatry exists as a field of public institutions with both implicit and explicit political positions, then how do I situate myself as an ethnographer of the discipline? In other words, even though my main goal in this project is to provide a *descriptive* account of transcultural psychiatry in Montreal, the nature of the research means that the project also bears an implicit degree of *critical* responsibility.

There are four main biases involved in this study: a selection bias related to my process of determining data inclusion and exclusion; the confirmation bias which is part and parcel of

short-term fieldwork; a bias related to my positionality and goals as a researcher; and a courtesy bias which stems from gathering data from members of ongoing professional projects. The Latourian method for science studies involves going to the laboratory and following objects of interest out from there. However, because of how broad the symmetrical approach is, it does not provide rigorous exclusion criteria beforehand. I began my fieldwork by making contact with the DSTP; from my connection to that institution, I made my connections with members of the CCS and the CESAR. In other words, there is a bias involved in my selection of field sites, because I found my informants through a pre-existing social and institutional network. This style of recruitment combines the methodological principles of multi-sited ethnography, triangulation, and snowball sampling (Parker et al., 2019). For a theorist like Latour, the kinds of bias inherent in this approach to qualitative research does not diminish the value of the data, it only requires room to be left for alternative articulations and future contestations (cf. *TB* 214).

The second ethical issue related to the design of the research project is that of confirmation bias. One way in which this kind of selective bias arises is from the need to organize ethnographic attention in the field. In short-term fieldwork, decisions about where to direct one's attention need to be made quickly and on the fly. Because of the breadth of my inquiry, mitigating this issue was especially challenging in this project. Ultimately, I made choices in the field about what kinds of questions to ask, and in what parts of the working environments to participate, based on my instincts about what data I most needed in order to tell an accurate and useful story. My behaviour at these sites was also informed by a desire to 'make sense' as a participant or inhabitant of the environment in question — that is, to contribute to the work of these spaces as an active member. Certainly this approach to decision-making invites

bias into the project; I stand by my choices on the validity of the suggestion in *Laboratory Life* that even "crude and qualitative" research tools can be "consistent with the scientific ethos" (12).

There are a few issues of my positionality that raise ethical questions. The main consideration is related to the effort in Latourian science studies to occupy the position of an "'inside' outside observer" (*LL* 12). Although I participated for four months in laboratory and paraclinical settings, I did not have the normal level of scientific or technical training required to work in such spaces. This being the case, I lacked fluency in the kind of literacy that I've described as being the center of the scientific enterprise. My illiteracy in this regard was important to my ability to observe the working processes of researchers as if from the outside, but it also limited my ability to engage with them in their own terms. According to the medical doctor and researcher Jonas Salk, who wrote the introduction to *Laboratory Life*, this outside position and alternative literary skill set of the anthropologist in the laboratory is simply a feature, and not a flaw, of the ethnographic study of science (13).

The final kind of bias that is clearly at work in this project stems from the social pressures involved in asking researchers to provide information about their own field, and the conditions of their work. Latour spills much ink belabouring the idea that professional and institutional science consists of a network of social alliances; this being the case, it is reasonable to expect that members of a scientific working environment may not share information which would negatively impact those alliances, and therefore reduce their own mobility through the networks of a given field. Ultimately, the interest of this project is more so the effort to describe how transcultural psychiatrists interact with stress — as a conceptual object and technical problem — than the efforts they make in interacting with each other. This limits the impact of this kind of bias in my research findings, which Cicarelli and White (2014) name 'courtesy bias'. Following Latour's

example, I have tried to steer clear of "the kind of gossip, innuendo, and embarrassing stories, and of the psychologizing often seen in other studies" (*LL* 12). However, in the parts of my field report where discussions of labour conditions or the social politics of the laboratory become relevant, I make efforts to identify and mitigate the possible role of courtesy bias.

A similar kind of courtesy bias is also salient in my involvement with the CESAR, where the lab director was enthusiastic in his support of my research. Through my alliance with Rob, I wielded a kind of ambient positional power, especially with respect to newer student researchers, who may have thus felt a social and professional pressure to contribute positively to my findings. This is one of the peculiar ethical quandaries involved in researching student researchers, who enjoy a limited professional agency, stability, and mobility. In research environments, the pressure to produce literary products is very high, and getting involved with group publication efforts is a crucial element of professional success. Student researchers experience an even greater pressure to work well with others, and to assist each other in producing useful results — such environments do not leave much room for the kinds of dissent or refusal called for in contemporary texts of critical anthropology such as "On ethnographic refusal" or "R-words" (Simpson, 2007; Tuck and Yang, 2015). Conversely, I experienced a similar kind of pressure in my own involvement with more experienced and established professional researchers: to be overtly critical of their laboratories or practices whole in the field — or in this text — would compromise my ability to make alliances in their networks in the future.

With respect to student researchers, I have attempted to limit this courtesy bias by carefully explaining the privacy practices of my fieldwork: as I coded the ethnographic material, all contributions by non-interviewed participants were anonymized. With respect to higher-status researchers, especially the directors of my three field sites, I attempt to limit this issue through

my emphasis of description rather than outright critique. My primary concern is to describe the instrumental environment of the laboratory, and to explain how it contributes to the construction of somatic stress as a certain kind of object or reality. Beyond that, any critique of the conditions of this construction need not invalidate the research or personally condemn the directors — rather, this critical approach serves to situate the laboratory as a social environment subject to the wider cultural and economic context of contemporary biomedical science.

The second ethical quandary in this project, related to my selection of participants, concerns the status of the psychiatric subject in my field work. My research draws its participants from the institutional organizations associated with the CMHRU and the CESAR. Participants include members and associates of these groups. The perspectives and practices of clinical physicians and psychiatrists, professional and student researchers, social scientists, and hospital administrators and staff are all relevant to this study. Neither psychiatric patients nor participants of the CESAR projects, however, were solicited to participate in my project, due to their more vulnerable positions within these institutional structures.[30] The result of this methodological decision is that the sufferer from distress *en tant que tel* — who is, in some ways, the primary object, subject, or concern of transcultural psychiatry — is conspicuously absent from my own study. Ultimately, I argue that my exclusion of the clinical or laboratory subject does not constitute a silencing of their voice; by concerning myself with how doctors and researchers took up the voices of their patients or participants, I hope to contribute to the ongoing efforts to reduce their vulnerability and increase their visibility. As a participant in these spaces, I adhered at all times to the formal ethical protocols established at each space — outlined as they were in the

[30] Part of my reasoning in making this decision was related to the ongoing crises in Quebec's public health system. At the time of my research, health and social services had not yet recovered from the COVID pandemic, and a major labour strike was underway at both hospitals where I was conducting fieldwork. In these conditions, I felt that attempts to get involved directly in settings of patient care were more ethically complicated than worthwhile.

STARS researcher formation handbook, and covered by the CCS' clinical handbook, both of which clearly define standards of practice and medical confidentiality.

Latour and Fassin both make the case that political projects are an *implicit* component of any organized scientific effort; all three of my field sites, to varying extents, are home to forms of science which involve *explicit* political projects as well. The CMHRU webpage lists among the primary research priorities of the department political concerns such as inquiry into the social determinants of mental health, critical evaluation of mental health services, and responding to diversity in mental health care.[31] The CESAR webpage also identifies a political mission:

> Notre objectif est de mettre en évidence les caractéristiques propres aux personnes issues des populations sous-représentées dans la recherche en santé, tout en contribuant à l'avancement des connaissances scientifiques quant aux concepts de sexe*genre, allostasie et résilience.[32]

Here, through the leverage-point of scientific representation, the laboratory work of giving name and body to a new constellation of technical concepts is linked to the political act of intervening in social perceptions. Just as Plato argues in *Statesman*, the nomothetic and dialectic projects of refining our taxonomies of reality necessarily entail the reorganization of political life.

As political spaces, my field sites are concerned not only with strengthening the position of their vulnerable participants, but also with navigating and altering the priorities of the governmental agencies that support and direct scientific inquiry in Canada. These two kinds of political bodies — lab and agency — can be tied together under Fassin's rubric of public health: one way of expressing this relation would cast the granting councils as powerful agencies in 'the government of life'. Through its role in the direction of a *differentiated* field of health science, the Canadian Institute of Health Research (CIHR) influences the gestures of treatment and the diagnostic categories by which the bodies and lives of political subjects are medically managed.

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[31] This webpage can be found at https://www.mcgill.ca/tcpsych/.

[32] See https://www.cesarlab.ca/a-propos.

One ethical project of my thesis is to articulate how transcultural psychiatry strives to generate new kinds of names and cultivate a new literary sensibility of stress, in order to afford new and potentially radical niches to the vulnerable or 'disordered' bodies with which it concerns itself.

In the case of the CESAR lab and the STARS project, the focus on studying Queer participants merits some attention. I did not set out in this project to get involved at a field site of Queer science — in this sense, the specific status of Queerness or the vulnerability of Queer bodies is only an incidental issue in my research. Nevertheless, the CESAR lab is a self-conscious site of activism, at which the pursuit of scientific articulation is framed a means of giving back to the Queer community.[33] In *Science in Action*, Latour describes the scientist as a "spokesperson" for the instrumentally-produced information that they assemble and arrange in their articles (73); in the context of my work with the CESAR, this activity of 'speaking-for' acquires a new and more explicit ethical and political dimension. While my main goal in this project is to descriptively represent transcultural psychiatry, I remain aware of Latour's point that representation always also implies intervention: my treatment of the subject cannot involve any kind of simple recusal from the effects it might have on the lives in question.

Ultimately, I situate myself in relation to the Queerness of the CESAR's research participants through the Latourian protocol for describing science as *in action*: by focusing on sexual minority as one kind of statistically-salient variable among others, I argue that, in an important sense, these scientists are only seeking to articulate a new kind of scientific object, or to capture a new kind of scientific information. In other words, in my project, there can be no *a priori* objectification of Queerness as a social or material category. Until the black boxes have closed around it at the CESAR, the term must remain, to a certain extent, indeterminate.

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[33] See https://www.concordia.ca/cunews/offices/vpaer/aar/2017/09/20/keeping-lgbtq-people-healthy-and-happy.html

A similar disclaimer is necessary in relation to the communities in question at the CCS. Most of the CCS case consultations involve the physicians of migrants (or 'exiles') in Canada on asylum claims. The extreme political vulnerability and complicated moral status of these patients is an explicit question and concern of these clinical activities. With respect to these questions and concerns, I deferred to the ethical protocols of my field sites, because, unlike my own research, they were initially designed with the specific status of these subjects and collectives in mind. Because the specific status of these communities was not a concern of my initial research question, I did not enter the field with equipped with any specific methodology or ethical precepts of my own to account for their distinct vulnerability (or resilience). Here too, I wished to avoid subscribing *a priori* to existing articulations of their status in the anthropological literature, which would only reify them as a certain kind of subject or object. Instead, I learned from the ethical commitments of these experts through my ongoing participant observation. Instead, my relation to the ethical problems that they present (to myself as a researcher, to my participants as scientists of care, and to us as members of the society which allows for or even results in the precarity of their forms of life) emerged organically throughout the research project, in conversation with my own participants, and through later consultation of the relevant literature. I discuss this *a posteriori* emergence of an ethical sensibility in Chapter 7.

Since (at least) the debates of Nancy Scheper-Hughes and Paul Farmer in the mid-90s, concerning the question of a 'militant' or activist anthropology, the standard ethical approach in the discipline has been to support, where possible, the political struggles of our informants. In the fifth chapter of *Les mondes de la santé publique* ('Crises éthiques'), Fassin describes how an anthropology of public health implicates the anthropologist in crises of the "traitement des vies vulnérables" (213). For Fassin, any such crisis demands a critical attention, an inquiry into "ce

qui la produit et comment on la construit" (215). He argues that "la production sociale s'etudie en [trouvant] des enjeux économiques et politiques," while study of "la construction sociale [demande] ce que ce langage de la crise permet de révéler" (215). I would like to argue that a symmetrical anthropology of transcultural psychiatry contributes to the political projects of the field by uniting these axes of sociomaterial production and taxonomical construction. To the extent that Latourian inquiry is concerned with the scientific techniques of literacy — the processes by which scenes of inscription render an object visible — it is well suited to respond to the problems presented by the fact that "cet aspect éthique des crises sanitaires est souvent la part la moins visible, et peut-être la moins dicibles" (*SP* 216).

To review: for this inquiry, I have assembled a symmetrical method which follows scientists and doctors as they move the hybrid agents of stress through sociomaterial systems, traversing scales of political and physical bodies. This inquiry carries an ethical charge, insofar as the political project of transcultural psychiatry is to intervene in the ecologies of diagnostic representation, so as to redistribute resources along the networks of biomedical science and the government of life. Situating stress within these systems is the first step in their speculative pursuit of more just futures for the ecological niches which their patients inhabit, of relief from distress, and of a world more livable for the bodies with which this science is concerned. Both Queer communities and political exiles exist outside the conventional margins of norm and habit; both kinds of bodies are differentially exposed to vulnerability and violence. The scientific endeavour to redefine stress in their terms — in terms that make more sense to them, and make better sense of them — is an ethically-charged project. This effort at translating across difference involves shifting the old margins, using names in new ways to give more articulate accounts. Through my ethnographic analysis, I endeavour to contribute to this project in my own fashion.

Chapter 5. Document Analysis

Having now articulated this critical methodology, along with the Latourian conceptual framework to which it is tied, it is time to turn to the ethnographic material of this project itself. In this chapter, I will begin the analysis of my field sites through a review of primary documents. The documents that I present here serve to contextualize the *practical activity* that occurs in these spaces in two important ways: first, they elaborate the discursive themes or *literary context* in which this discipline of scientific action takes place; second, they represent or exemplify the *literary productivity* around which laboratory and clinical life organizes itself. In other words, this chapter is oriented towards clarifying the *literary sensibility* of Montreal's transcultural psychiatrists, both in terms of its *content* (what they write) and in terms of its *form* (how they write). The purpose of this analysis, then, is to afford a deeper understanding of the technical activities and arguments of these scientists, but also to demonstrate how their field is structured by literary credit cycles: those in which it participates, and out of which it emerges. In this Latourian reading of the discipline, we learn how to speak its languages and interpret its gestures at the same time that we begin to discern the social semiotics which give them shape and colour.

The chapter consists of two major sections. In the first section, I treat documents from the CMB workshop and the CCS, both of which exist under the organizational rubric of the CMHRU. These sites are tightly linked in a number of ways. Institutionally, as projects of McGill's DSTP, they involve the same disciplinary materials and attitudes; socially, they are also connected through the DSTP — Laurence Kirmayer, the director of the DSTP, was both chair of the CMB workshop and a founding director of the CCS. On the basis of these clear connections, I read the documents with which I was provided at both of these sites together, treating them as

components of a shared literary constitution. In the second section, I treat documents related to the CESAR laboratory in general, and the STARS project in specific, drawing mostly from contemporary work on the allostatic theory of stress. In places, I refer to documents from other bodies of work on stress hormones, and the experimental analysis of human sexuality — two research programs on which the allostatic literary sensibility of the CESAR bases its claims.

(I) The CMHRU's Transcultural Psychiatry

Part 1. Missing Masses, Missing Persons

In my conceptual framework, I discussed at some length Latour's mid-career essay, "Where are the Missing Masses?" But what are these 'masses' to which his characteristically Gallic title refers, and in what exact sense are they 'missing'? The essay opens with this claim:

> According to some physicists, there is not enough mass in the universe to balance the accounts that cosmologists make of it. They are looking everywhere for the 'missing mass' that could add up to the nice expected total. It is the same with sociologists (152).

The problem for sociologists is that their ledger of social bonds or forces, with its two sole columns of human actors and cultural norms, cannot produce any figures sturdy enough to account fully for why societies work the way they do. Latour's solution, we've seen, is elegant enough: social theorists must add a column for nonhumans into their accounting for social activity. In this part, I build out my argument that a similar problem exists for conventional psychiatrists: like social scientists, their ledgers make room for human habits; and, as a medical science, they account for plenty of the nonhuman entities of biology — what remains missing, according to the distinctive literature of transcultural psychiatry, is a deeper feeling for how the *embodiment of culture* might link and mediate these forces. In this literature, then, is a movement

towards the same kind of symmetry called for by Latour, only from the other direction. The task at hand here will not be to find the missing *nonhumans*, but to bring *ourselves* back into the fold, and find some way to connect the psychiatrically-salient activity of *physiological material* with the *sociocultural gravity* that gives it such discursive weight in the first place.

Let's begin our reading of the transcultural psychiatric literature with a brief overview of the field's history, starting with some insight from the founders of the DSTP at McGill — the psychiatrist Eric Wittkower, and the anthropologist Jacob Fried. In a 1959 address to the annual meeting of the American Psychiatric Association, they identify the importance of their new research program by drawing attention to wider discipline's lack of interest in the "socio-cultural environment" of its patients (Wittkower & Fried 1959, 423). They go on to argue that, through the integration of concepts and methods from anthropology, their research program has begun to solidify as a "separate entity" in the disciplinary landscape, with an emerging "nomenclature" of its own (423; 427). Where Latour argues for the integration of a materialist sensibility into social inquiry, Wittkower and Fried integrate social sensitivity into their own scientific discipline; the result is a new kind of naming practice, through which "the socio-cultural dimension joins with the genetic, biological, and psychological interpretation of human behaviour" (Wittkower & Fried 1958, 245). Latour's propositions for inquiry resonate well with this pluralistic effort toward reinscribing and rematerializing scientific reality: the more angles of approach, the better.

As transcultural psychiatrists continue to positively modalize this pursuit of symmetry, the basis of their claim to disciplinary credit remains an identification of 'missing masses' in the wider field, and their assertion that the anthropological perspective can help to find them. Half a century after Wittkower and Fried, the DSTP's director articulates its research program in the

same terms, arguing for "the inclusion of social processes" as a means to balancing psychiatry's accounts (Kirmayer & Crafa 1; cf. Kleinman, 1977). One recent article from the DSTP's 'Culture, Mind, and Brain' program puts this in no uncertain terms — coauthored by Kirmayer, "Restoring the missing person to personalized medicine and precision psychiatry" argues that "precision psychiatry cannot reach its goal" without including "the agency and the experience of the person" (Gomez-Carrillo et al. 1). Latour claims the social sciences need to account for *nonhuman* agency to represent human culture more fully; here, Kirmayer bears out the same point by arguing that the reverse is true for the material sciences. Symmetrical anthropologists seek missing masses; symmetrical psychiatrists are on the search for missing persons.

Indeed, in much of his contribution to the transcultural literature, Kirmayer argues for this distinctive research program by situating it in relation to a thoroughgoing asymmetry that he identifies in the wider psychiatric discipline — an asymmetry which he ascribes to the foundational literary sensibility of conventional positivist science. Describing contemporary psychiatric research, Kirmayer identifies a "methodological strategy [. . .] underwritten by a kind of biological essentialism that assumed that psychiatric disorders would turn out to be discrete biological entities" (Kirmayer & Crafa 2). Under this style of naming, he argues that "research has contributed to a process of reification in which the accrual of evidence about the [diagnostic] constructs makes them seem more solid and natural" (3). The proximity to Latour's account of science could hardly be clearer; here, Kirmayer seeks to disrupt a strong credit cycle, by arguing that the closure of black boxes around 'the constructs' depends on an asymmetrical assumption — one which excludes anthropological material from the 'process of reification'. The result of any asymmetrical inquiry is an incomplete definition: the biologically-objectified disorders of

this form of research may seem solid, but they belong to an inherently unstable taxonomy, because psychiatric disorders are, in fact, "not a single type of entity" (Gomez-Carrillo et al. 3).

An important feature of this critique of conventional psychiatric taxonomy is Kirmayer's *negative* modalization of the *Diagnostic and Statistical Manual* (DSM), the "de facto standard" handbook in psychiatric research and practice (Kirmayer & Crafa 2). For Kirmayer, textually-formalized nosologies like the DSM do not simply *propose* "implicit models about mental disorders" (1); it also contributes to the *reification* of these models, due to its "dominant role in training and research" (2). As Latour would argue, manuals are such powerful texts because they prescribe programs of action; texts like the DSM thus have a gravity of their own, because of how many allies organize their entire scientific practice around its social authority.[34] Kirmayer further supports his negative modalization of conventional psychiatry (and its asymmetrical, materialist constructs) with another Latourian gesture, connecting the blind spots in its literary representations with shortcomings in its clinical interventions (2). He argues that this research program results in a set of clinical tools partly designed to bear out the authorized disciplinary names, which thereby solidify the social credibility of discrete diagnostic categories (Kirmayer 7). When he argues that "artifacts have not been eliminated" from its "accompanying technology of highly structured diagnostic interviews," Kirmayer reopens the black boxes around the DSM's tools of clinical intervention and laboratory research, drawing critical attention to the scenes of inscription at which psychiatric facts have been and continue to be constructed (8, 7).

This being the case, the recent publication of the *Research Domain Criteria* (RDoC) by the National Institute of Mental Health (NIMH) — an alternative nosology to the DSM — might

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[34] See *Science in Action* Chapter 1, Part B.1 ('Bringing friends in,' 31-33).

represent an opportunity for subfields like transcultural psychiatry, as a moment when the major institutional authorities of the wider field are parting ways "very publically and polemically" (4). According to Kirmayer, the RDoC marks the ascendence of a new dominant research program in psychiatry, one founded on an investment of credit in "neuroscience as a path to understanding mental disorders and developing more effective treatments" (1). However, from the perspective of transcultural psychiatry, the neuroscientific approach of the RDoC remains "impoverished and conceptually flawed," founded as it is on "disciplinary commitments and interests that are at odds with the larger concerns of psychiatry" (1). Essentially a "vision of the future," the new manual seeks to shift psychiatric research programs into a new credit cycle, based on the promised order of neuroscientific reality — but this order is still firmly grounded in biomedical essentialism (9). The new manual demonstrates thus Latour's concept of stratification well:

> Much like the DSM, these domains of research were established by roundtable discussions among leading experts. As such, they represent a snapshot of current work in neurobiology. [. . .] Although this may be a long-term goal, the framers of RDoC believe that heavy investment in this program is warranted (4).

These 'experts' at the center, such as the NIMH's director Thomas Insel,[35] leverage their institutional positions to establish new lines of credit and authority. Any other *alternative statements* of what might constitute a valid concern for psychiatry are newly excluded: their intensified prioritization of research into the neurobiological dimensions of mental distress and disorder thus comes at the continued expense of its anthropological dimensions.

Let's have a look now at some of the specific literary gestures through which, by situating transcultural psychiatry in relation to the neuroscientific black boxes of the RDoC, Kirmayer seeks to *positively* modalize his own field's research program. First, Kirmayer ascribes to

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[35] On Insel's naming regime, see Pickersgill (2013), "Debating DSM-5: diagnosis and the sociology of critique".

neuroscientific accounting a certain kind of reductive, simplistic, or at least linear causality: the RDoC, for example, orients research towards "definitive treatments" through the articulation of "underlying [. . .] mechanisms" (1). This approach to representation and intervention relies heavily on the testimony of the *nonhuman elements* of human bodies: at play in this conception of "mental disorders" are "physiological expressions of pathology that could be measured through biomarkers" (1, 3). Ultimately, the goal in this effort of "'translational research'" is to "replace clinical diagnoses" by capturing a given disorder in discrete "biologically defined parameters" (6). By appealing directly to material reality through these biological measurements, neuroscientists promise to translate the problem of mental disorder to a point of greater leverage, into the terms of their laboratory apparatus (3). If the representation can be fully shifted to a description in nonhuman terms, then the intervention could be fully delegated to predictable nonhuman actors, as the unreliable human "patient is asked to stand to one side," their own agency 'displaced' by that of their body and its disease (6).

Neuroscience has its own kind of diagnostic entities, fit to suit its particular instrumental skills; for Kirmayer, the problem is that its accounts remain asymmetrical across this trajectory. The neuroscientific translation of leverage towards the laboratory simply represents a renewed "investment in 'normal science'" (6). This intensification of an existing credit cycle may, he concedes, "increase the likelihood of finding certain lower-level mechanisms" (4); however, due to its still-unbalanced reliance on nonhuman agents, its form of accounting "will not provide a complete explanation" of the predicaments faced by the human participants of the psychiatric encounter (4) — some mass is still missing here. It is in these terms that Kirmayer motivates or modalizes the transcultural psychiatric "effort to humanize care" (6). As this field sets about the

work of articulating a new literary and technical sensibility, a new kind of underlying substance will displace the 'merely' biological material of conventional neuroscientific psychiatry. In other words, the field's *methodological shift* towards the instruments of anthropology will require an *ontological shift* towards the material reality of cultural variability — and *vice versa*.

In order to understand the specific modalities of scientific accounting involved in this critical effort, it will now be useful to turn towards some of the diagnostic discourses, aesthetic idioms, and literary entities of transcultural psychiatrists on their own terms. In response to the "unique causality" of the neuroscientific account of brain circuitry, they propose a "complex, multifactorial" form of systems-oriented causality (4). The material of neuroscience is not simply 'natural', but inherently culturally-inflected as well, according to the "wealth of research" which they cite as describing the social constitution of the brain and body (Gomez-Carrillo et al. 2). This social rearticulation of the brain defines specific mechanisms of hybridity by which culture and biography are "inscribed in the circuitry" of the brain and the biology of the body (Kirmayer & Crafa 7).[36] This reinscription of the special, well-disciplined object of neuroscience involves a questioning of its basic research program: it is because neuroscientific instruments exclude the social agencies which shape the brain that they necessarily result in artificial definitions.

The counter-program of transcultural psychiatry must then resist falling into the opposite asymmetries of social relativism or practical culturalism: in his description of "stress response systems," for example, Kirmayer urges transcultural psychiatrists to think through both "neurobiological and sociocultural dimensions," arguing that physical and cultural coping mechanisms "modulate" each other through the mediations of the body (Seligman et al. 15).

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[36] On Kirmayer's positive modalizations of research which emphasizes the hybridity of the brain and body, see also *Culture, Mind, and Brain* (2020), especially pages 14, 35, 496, and 507; and in Seligman et al. (2016), pages 3-5.

Kirmayer also critiques early work in the field, dedicated to the description of 'culture-bound' illnesses, for remaining blind to the biases of cultural essentialism, thanks to which a given "phenomenon better understood in terms of power, conflict, and social change [is] reified as a psychopathological entity" (Kirmayer 6). Critical cultural consultation, on the other hand, understands "problems that are initially attributed to characteristics of patients or their family [. . .] in terms of biases or assumptions of biomedicine" (*CC* 13).

Kirmayer commonly defines the paradigm of an encultured body in the terms of an ecological sensibility: "Together," he writes, "brain, body, and the social environment form a mutually regulatory adaptive system" (Kirmayer & Crafa 9). This literary aesthetic involves its own kind of scientific objectivity: not the linear mechanisms and discrete entities of biological essentialism, but "multi-level systemic concepts" structured by the cyclical regulation of "'feedback loops'" (9). Here, then, is the new ontology for a science of hybrid materiality: the matter of the human mind is not at all a static or passive nonhuman substance, which might be captured once and for all through progressive laboratory trials and refinements; it is, instead, an active "ecological system" that is constantly "reorganizing itself through dynamic interactions with the larger systems in which it is embedded (the body and the social world)" (*CMB* 2; 1). Kirmayer positively modalizes this new kind of object by tying it to a number of citational chains in other scientific subfields: the '4E paradigm' of cognitive neuroscience (*CMB* 8-14); the theory of cultural affordances from contemporary cybernetic philosophy (in Ramstead et al.); and our familiar, well-cited sociological concept of the diagnostic niche (Seligman et al. 11). Together, these ecological literary idioms reinforce each other against the still-dominant strength of a conventional scientific aesthetic sensibility of discrete objects and linear agencies.

It is in this way that the shift towards a new kind of diagnosis, with its concomitant reconceptualization of psychiatric 'material,' involves the work of disputing or disrupting the authoritative naming taxonomy of the wider discipline — what Plato would call the work of the dialectician. Situating their own field through a negative modalization of biological essentialism, transcultural psychiatrists lay the groundwork for a pluralistic diagnostic taxonomy that will proceed from the symmetrical foundations of their "ecosocial approach" (Gomez-Carrillo et al. 1). These claims to credit rest on a promise to deliver results that other research programs have failed to realize (Kirmayer & Crafa 10).[37] By expanding the parameters of diagnostic definition, these ecosocial literary objects are first made interesting (to the semiotic character of the reader) insofar as they are able to do the necessary work of resolving the conceptual poverty of conventional diagnostic *categories.* In Kirmayer's words, they account for "the emergent dynamics of complex networks that can help us go beyond the linear causal models that currently dominate both research and clinical thinking" (10). By integrating a critique of conventional diagnostic *instruments* as well — what Latour would call psychiatry's 'contexts of production' — this literary movement reinforces its claims to credit. Ultimately, it is through the failures of old tools in the clinic that the need for new names in the literature is made apparent and urgent (Kirmayer 3); these dialecticians promise not only new *kinds* of diagnoses, but new ways of *using* diagnosis altogether — again, not only a new ontology, but new methodologies as well.

For Kirmayer, the "additional [. . .] perspectives" of anthropology are what allow transcultural psychiatrists to "enlarge the clinical imagination" in practice (*CC* 13). The

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[37] Kirmayer makes use of familiar literary tactics to motivate this philosophical program: "There is an urgent need," he writes, "for an ontology that includes these larger processes to guide psychiatric research and practice" — folding four citations into the end of this sentence in a clear gesture of captation (Gomez-Carrillo et al 3).

perspectives of cultural affordance, for example, serve to orient the clinician towards a consideration of "ecologically meaningful action," by which the particularity of a patient's sociomaterial context takes precedence over (or at least exists alongside) the presumptive universality of their diagnostic status and physiological condition (*CMB* 9). The rearticulation of literary objects, in other words, leads back to a reconstitution of clinical attitudes and diagnostic strategies — a movement from thinking through the discrete mechanisms of neuroscience, to working with multilevel "loops that connect brain, body, and environment" (12). Diagnosis, in this scientific ontology, does not exist beyond or outside of the material that it claims to name (504). Instead, insofar as social processes and histories inscribe themselves in the flesh,[38] the taxonomies and moralities of psychiatric diagnosis continuously rewrite themselves into the loops of organism and environment that they are meant to represent. Indeed, patients themselves become agents here, involved in the dialectical process of *using* names and discourses to *reorganize* their own social contexts and cultural ecologies (Seligman et al. 11). A strong sensitivity to these social contexts is thus "essential," Kirmayer argues, "to the skills that allow experts to translate generic knowledge into effective action" (*CMB* 496). By ratifying these principles of symmetry, transcultural psychiatry tries to do justice to that ecological relation — which we Latourians might call hybridity — between diagnostic order and physiological reality.

Because methodology and ontology go hand-in-hand, any alternative approach to representing the mind and its disorders requires a new way of interacting with or intervening against them. For this task, transcultural psychiatry adopts "critical and social science perspectives," along with their "ethnographic" instruments, in order to reflect the fact that the

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[38] For an anthropological formulation of this claim, see Fassin's *L'éspace politique de la santé* (13-16).

clinic is "always part of a larger social system" (Kirmayer 11). In other words, the field constitutes itself through a thoroughgoing commitment to "methodological pluralism" (Kirmayer & Crafa 2) — a pluralism of perspectives which, as Latour would argue, is necessary to fill out the accounts with all the different kinds and scales of agency at play in a given scene or program of action.[39] To exemplify these aspects of the transcultural psychiatric skillset — its assimilation of ethnographic techniques of inscription, and its steadfast interdisciplinary sensibility — I will now turn to some of the constitutional literature of the CCS. These documents will introduce and explain one of the field's major methodological innovations: the Cultural Formulation Interview (CFI). This method of gathering and organizing cultural information as it relates to diagnostic reality will help us better understand the emergence of a new psychiatric substance and subject.

Part 2. From Biomedical to Anthropological Substance

In the first part of this section, we saw how transcultural psychiatrists at the CMHRU articulate their literary aesthetic by positioning themselves in relation to the existing credit cycles of psychiatric science. Their proposition of a new kind of scientific object — one grounded in a hybrid or ecological ontology, and constituted by a symmetrical and pluralistic methodology — is the result of their negative modalization of both conventional diagnostic taxonomies and techniques of inscription. By drawing attention to the asymmetrical *information* on which these taxonomies are based, they aim to persuade readers and resource-holders to invest credit in their own information, their new style of naming, and the skills of delegation that come along with it. Where, then, does this new information come from? What are the scenes of inscription in

[39] Fassin, for his part, emphasizes the multilevel and multicausal constitution of public health crises when he writes of "des multiples avatars des agents infectieux" to which a given condition might be ascribed (*SP* 123).

transcultural psychiatry? And how are they related to the clinic — that scene of intervention or point of leverage around which the whole discipline is organized?

To answer this question, we can begin by identifying just what kind of 'mass' it is that transcultural psychiatry claims to add to conventional information. In general, as the name suggests clearly enough, the transcultural subfield addresses itself to the "fact of cultural diversity" (*CC* 4). This fact poses a problem to health services insofar as "cultural processes shape the mechanisms of disease, the symptoms of illness, and subsequent ways of coping" (1). According to Kirmayer and his colleagues, such services might respond in one of two ways:

> [assimilating] patients into standard practice by normalizing and ignoring difference or acknowledging and responding to difference by developing more varied models and practices (4).

The transcultural effort exists under the rubric of the second response, as a move to construct better models and practices by producing more varied kinds of information. Describing the CCS at which I conducted my own participant observation, members of the CMHRU emphasize a *plurality of concerns* and a *sensitivity to context* as primary features of the information that it produces (9). The instrument involved in this production is the CFI: a clinical interview schedule that promises to construct diagnostic names and techniques which *better resist dissociation* by better representing those disorders that *otherwise resist intervention* (10).

In what follows, I will first consider the CFI as a *literary device* — an instrument that constructs this stronger information by producing *a certain kind of inscription*; then, I will consider it as a *technical device* — an instrument that produces its inscriptions by organizing *a certain kind of encounter*. The first part of this movement will involve considering *the semiotic quality* of the information: what the CFI stabilizes, how it mobilizes its users, and how it enables new combinations of entities and resources. The second part of the movement turns from the

literary fate of this information to *the scene of inscription* itself, asking how it is that the transcultural 'construction of sense' works to reorganize noisy material into orderly information in the first place. From our consideration of these initial clinical encounters, we will finally be able to articulate how, by finding the missing mass of conventional diagnostic accounting, ecosocial ontologies of distress *reconstitute the body* as a hybrid mediator of plural agencies.

Transcultural psychiatrists advocate for the credit-worthiness of the CFI by referring back to their ecological description of human distress. Narrative, they argue, is an important "causal mechanism in [. . .] psychopathology" — because it "represents a particular important bridge between individual psychological processes and the social world" (Kirmayer & Crafa 8). As an element of the "loops" that structure "adaptive systems" across the ecosocial gradient, narrative in fact mediates between the social and the material elements of mental disorder (8). Storytelling thus represents a *source of information* and *site of intervention* for clinicians — but only if they are equipped with the cultural competence and semiotic skills necessary to mobilize these stories, and to convert narrative into a *point of leverage*. In the CFI process, this conversion comes about through a "multidisciplinary collaboration" designed to "systematically elicit patients' accounts of their illness experience" (Dinh et al. 261; 262). By putting a diagnosis in "social context" through this systematic collaboration, users of the CFI gather the material necessary to represent symmetrically, making space for "the 'whole' person, not just the presenting problem" (262).

Where representation increases in symmetry, Latourians expect intervention to increase in hybridity. Transcultural psychiatrists seem to operate under the same assumption:

> This [new clinical information], in turn, opens up new lines of action, expanding the clinician's repertoire and introducing an element of pluralism into a monolithic medical system (*CC* 13).

By pluralizing the kinds of information by which a scene of disorder is described, transcultural clinicians pluralize the points of leverage through which they might act. A new political mobility comes along with this new kind of information: "enlarging the role of psychiatric expertise" to include "mediation [. . .] and advocacy," they move throughout the ecological scene, leveraging their authority in public "institutional settings" and "community agencies" to enact therapeutic order (12). Conventional psychiatric delegates are deployed in unconventional ways, as the team draws on "ethnographic literature" to reimagine the social networks and material forces that constitute a given situation of disorder (12). This supplementary information — what we call psychiatry's missing mass — helps translate diagnostic categories into relevant "cultural scripts," through a "hybridizing" process that enables a more effective association between the physician and their patient (14; 17). Assembling a diversity of perspectives and resources, the clinical team constructs a hybrid program of action, one that appeals to humans and nonhumans alike.

As transcultural psychiatrists continue to build literary accounts, clinical procedures, and diagnostic categories using the CFI, their disciplinary skillset should grow in strength. So, while there are fewer black boxes in transcultural psychiatry (fewer points of leverage that promise any 'definitive treatment'), they argue that there are more "diverse voices" (and so more ways to imagine what healing might actually look like) (13). Although their context-specific inscriptions may seem less stable and combinable than the statistical constructs of straight neuroscience, the symmetrical constitution of their information thus promises to afford greater mobility and utility. As a literary device, the CFI creates space for critical narrative information in the 'reification process' of diagnosis; as a technical or clinical device, it creates space for the interdisciplinary encounter in which this narrative information can be interpreted and operationalized. At the CCS,

the results of clinical CFI sessions move from noise towards order through the contributions of experts situated at various points on the ecosocial gradient; under this plurality of perspectives, the symmetrical account of a given case emerges through continuous conversational articulation. This activity of constructing sense — still an 'instrumental' process of inscription and refinement — draws its semiotic strength from the highly-situated cultural sensitivity of its participants.

Dinh, in her study of the CMHRU's CCS, identifies five stages in this process of translation and redefinition. The first stage, 'psychiatric history', involves the chairing psychiatrist's presentation of "diagnostic criteria" (Dinh et al. 265): here, the dialectical work of constructing a better account takes as its point of departure the conventional names of existing nosological taxonomies. In stage two, 'cultural formulation', the team introduces "a cultural explanatory model" or a specific "'idiom of distress'" relevant to the patient (266; 267): this is the missing mass which the team must translate into clinical relevance (or 'make matter') through the transcultural reconstruction of sense. At this point, any specific "power differentials" and "wider socio-political forces" that mediate the forms of physiological stress and psychological disorder in question are brought into play, as agents worthy of consideration (268). In the third stage, 'interdisciplinary assessment', the patient's specific sociocultural situation becomes "the point of convergence" between the "diverse [. . .] disciplinary perspectives" (269). Between the somatic orientation of medical expertise, and the symbolic orientation of anthropological awareness, the presenting problem begins to appear as the result of a specific context — a *particular form of cultural embodiment*. It is through this new representation that the fourth stage, 'interdisciplinary deliberation', opens up new interventions: if the problem in question involves a culturally-situated embodiment of distress, then solutions might involve the

culturally-specific affordances of the patient's unique ecosocial niche. In the final stage, 'closure', the team acknowledges the specific competences and contributions of its members.

What is new, in the end, with this interdisciplinary reconstitution of the patient? What does the inclusion of anthropological subjectivity do to psychiatric objectivity? What is so different about our bodies, that science needs a new approach to the treatment of physical material where we are concerned? Dinh describes the CFI as involving a "*humanistic thrust*" that shifts psychiatry away from the "'de-personalizing' aspect" of biomedicine (274; 272). The basis of this rehumanizing movement is an increasing representation of something called 'culture' in these accounts[40] — a force or structure that exists "not only in peoples' heads but also in the world" (Seligman et al. 8). Highly symmetrical, the definition of culture at work here involves "values and norms," but also "*actions* [. . .] and *discourse*, or the way in which language is mobilized and made to 'do' something in the world" (8). Linking these forms of social and technical agency *with* their anthropological theme of culture and *through* their medical concept of the body, transcultural psychiatrists claim a kind of radical mobility around the scene of diagnostic inscription — anything can be made relevant to the case at hand, a "potential for resilience" (Kirmayer 4), a valid concern worthy of psychiatric consideration.

For one way to interpret this extension of psychiatry's domain, we only have to return to Fassin's account of the diverse vectors of power through which conventional public health constitutes its subjects, and especially the intensification of its mechanisms around *les exilés* — often the very kinds of patients involved at the CCS. The CCS, in its coevolution with medical anthropology, emerges from a complicated history of encounters across difference:

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[40] For one of Kirmayer's attempts at an exhaustive definition of this slippery term, see *Cultural Consultation* (2-3).

> [the effort is] to understand and respond to human suffering in contexts that confound the alien qualities of psychopathology with the strangeness of the cultural 'other' (Kirmayer 2).

Latour's argument about the laboratory involves the claim that new facts, new kinds of objects, and new networks of association are disclosed when material is subject to extreme pressure in the experimental conditions of the instrument and its trial. In the situations described above by Kirmayer — much like those in Fassin's case studies — varied communities and unique bodies at the extremes of ecosocial precarity are thrown back against the bare fact of their difference. Under such extraordinary pressures, it should not be entirely surprising that new dimensions of embodiment might be disclosed. From there, the work becomes to articulate paths of resilience.

(II) The CESAR's Stress and Resilience Study

In the previous section, we saw how transcultural psychiatrists at the CMHRU play with the naming taxonomies of conventional medicine, reinscribing the physical material of the body through a sensitivity to its narrative or cultural dimensions. This disciplinary shift, born of a marriage between psychiatry and anthropology, may involve some radical claims, but it still aligns with the broad strokes of Latour's definition of science in action. In its production of order, transcultural psychiatry simply adds an ethnographic encounter to the clinical process of inscription. As a result, the field constructs sense through a particular kind of literary aesthetic — one that involves relational and cyclical entities rather than discrete, linear ones. The project of this discipline, then, is still to give an account of nature and to translate material into more objective terms — thereby generating some form of social credit and political leverage. What sets transcultural psychiatrists apart is their *ratification of the roles of culture* in this project, their acknowledgement of their own *political commitments*, and their discussion of some inherent

methodological limits in their discipline's symmetrical constitution. With this critical dimension of their project in mind, we are ready to turn to a treatment of the literature of the CESAR lab, and the Stress and Resilience Study (STARS), which forms the second half of my fieldwork.

In order to proceed with this turn, it will be useful to briefly recapitulate the relation between researchers and clinicians as established in the conceptual framework, and as gestured to above in Kirmayer's treatment of research-guiding manuals like the DSM and RDoC. Like laboratory researchers, clinicians produce inscriptions from (and ascribe names to) physical material. However, they are primarily concerned with the *use* of names, through their application to particular cases — either normatively, as in conventional psychiatry, or else critically, as in the transcultural clinical practice. Researchers, meanwhile, are concerned with the *construction* of names — and this work, too, can be conservative (through positive modalization), or disruptive (through negative modalization, and the proposition of alternatives). Unlike at the CCS, then — where clinicians are more interested in the *practical utility* than the *objective reality* of the diagnostic categories that they recite — scientists at the CESAR are primarily interested in reinforcing the *literary factuality* of the information, names, and objects that they produce. This laboratory process still represents an effort to support clinical work: after all, researchers want their names and facts to gain enough credit to move outward *into* society *through* the clinic; and the principle of science in action is that stronger facts can accomplish this outward movement by reliably affording mobility and leverage (cf. Gomez-Carrillo et al. 3). The salient difference is one of attitude: researchers want names to be *credible* in the literature, clinicians want them to be *useful* in society — but these goals clearly inform and sustain each other.

What we will see in this section, interestingly, is an incorporation of the "context-sensitive, integrative, idiographic approach" of transcultural psychiatry — which is, on the surface, more proper to the clinical encounter — directly into the laboratory research of the STARS project (6). In other words, I argue that, because scientists at the CESAR are committed to a *symmetrical approach to human bodies*, they also embrace the relational literary aesthetic of transcultural psychiatry. The adoption of this counter-disciplinary ontology at the CESAR — one which is grounded in contextual particularity and cultural variability — represents an important stage in transcultural psychiatry's emerging credit cycle. Research methodologies at the CESAR embody an anthropological sensitivity, so their efforts to name and gain leverage over stress — their particular object of research — are disruptive of conventional scientific taxonomies as a result. Insofar as the laboratory projects at the CESAR make room for the diverse voices of their research participants in their inscriptions, the information they produce is less mobile and less combinable. However, its increased symmetry may ultimately result in scientific objects and diagnostic names with greater utility for the inherently symmetrical space of the clinical encounter — and this, indeed, is the promise the CESAR makes in its claims for credit.

Part 1. From Biological to Biographical Stress

Before thinking through the possible clinical afterlife of the STARS project, it is first necessary to review its literary ground. On the CESAR website, the study defines itself in terms which are clearly hybrid, linking 'facteurs biologiques et socio-culturels':

> L'étude STARS vise à examiner les impacts du stress et de la stigmatisation sur la santé physique et mentale des individus. Plus précisément, nous cherchons à identifier les mécanismes par lesquels les facteurs de risque (par exemple, le stress chronique) et les facteurs de résilience (par

> exemple, le soutien social) influencent la santé et le bien-être des personnes issues des minorités sexuelles et de genre de différents âges et représentant différentes expériences de vie.[41]

By linking the study of stress — in both its physiological and psychological dimensions — with the culturally-specific concept of 'sexe*genre', the STARS project constitutes for itself a new object of scientific study: a form of stress which is mediated by both biological agents and biographical narratives. Such an object will have to exist in the space between the human and the nonhuman — in order to capture information and construct facts about it, scientists at the CESAR lab must assemble a research program that can both account for this kind of hybrid ontology, *and* provide the symmetrical methodologies necessary to assimilate 'natural' and 'cultural' material. Rob Juster, the director of the CESAR lab and architect of the STARS project, finds such a program in the disciplinary literature of allostasis and allostatic load.

As described in Chapter 2, the concept of allostasis, named by the neuroscientist Peter Sterling, first emerged in the stress science of the late 1980s. Focused on a new representation of the relation between organism and environment, the theory is fundamentally ecosocial or hybrid in nature. In a 2011 article summarizing research into the concept, Sterling articulates allostasis as the process by which the brain "mobilize[s] resources" from across the systems of the body, in response to social experiences (Sterling 1). Through its new object of allostatic stress, this systems-thinking approach to the body forges new associations between various literary cycles of active biological research (2). Allostatic theory defines stress as an effect of the organism's effort to adapt its somatic resources to a particular biosocial environment (5); health, in this framework, is a state in which the body is able to predictively activate physiological agencies and organize multilevel delegations in anticipation of changing environmental conditions (9).

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[41] See: https://www.cesarlab.ca/a-propos, and: https://www.cesarlab.ca/projets.

In 2002, Bruce McEwen and John Wingfield published "The concept of allostasis in biology and biomedicine" in *Hormones and Behavior*. This article reviews how the allostatic research program has built on its definition of stress to give name to the concept of allostatic load — the "cumulative cost to the body" of allostatic regulation (2). They define the form of allostatic load most relevant to human social environments as that which occurs when "sufficient or excess energy consumption [is] accompanied by social conflict and other kinds of social dysfunction" (2). McEwen, one of Juster's teachers and main literary allies, describes this association between the physiology of bodily energy and the sociology of environmental conflict in highly hybrid terms:"glucocorticosteroids and [the] activity of other mediators [. . .] such as the autonomic nervous system [. . .] wax and wane [with social stressors]" (2). At stake in this description is an effort to reconstitute or reinscribe *human stress* with the *nonhuman agency* of various substances and systems in the body, such that lived experience and social history may be interpreted through hormonal agents such as "perturbed cortisol" (3). In the research program that organizes itself around this symmetrical form of stress, biography and biology are made to speak for each other in the description of social threats to the physical body; as a result, the very substance of the body becomes a source of potential information about social dysfunction.

One interesting feature of this medical research, then, is its incorporation of public health discourses of environmental risk and measurement. McEwen argues that allostatic load may be scientifically defined and described through its "*secondary outcomes* that can be measured and are associated with increased risk for a disease," such as "metabolic imbalances" and hormonal disregulation (3). Of special relevance among these outcomes are the harmful delegations which sufferers of chronic distress choose to mediate or reorganize their experiences of their own body,

which McEwen groups under the concept of "substance abuse" (4), and which Latour might describe as 'deskilling' operations. The result of these new associations between physiological agents under the name of allostatic load is "a narrower and more precise definition of stress," one which "allows formulation of clear predictions that can be tested experimentally" (4). In this foundational allostatic literature, then, we find all the necessary elements of a new laboratory research program and credit cycle: a new definition (of stress), new scientific objects and facts (concerning stress as a function of social situations), and the promise of new leverage over society (through the authoritative representation of both behavioural and environmental risks to health, on the basis of experimentally-validated predictions). In order to extend this credit cycle, and support the movement of these facts into clinical settings, the research program of allostatic load requires the construction and positive modalization of credible black boxes.

In a 1993 paper published in *Archives of Internal Medicine*, McEwen and his coauthor Eliot Stellar construct an early argument in favour of the allostatic research program into stress. In the section 'Evidence for Effects of Stress on Health', they review the existing facts which link their object of study to important matters of public concern, defining it as a "risk factor" to diagnoses as broad as asthma, diabetes, heart disease, cancer, and autoimmunity (McEwen & Stellar 2097). Building on this literary scaffolding, they are able to motivate a "multilevel, interdisciplinary approach" into the "interactive [mechanisms and] effects of acute and chronic stress" (2097). The following section of the paper provides a litany of potential agents which might be linked under the allostatic program of study, associating behavioural habits with the well-defined neurochemical and endocrine mechanisms of existing medical science. The authors continue by articulating the emerging credit cycle at play in this new interdisciplinary literature:

> As more and more is learned about the specific biological mechanisms whereby physical and psychologic stress effect disease processes, *it becomes more and more important for physicians to take stress into account in diagnosis and treatment* [. . .] and more research is needed on the behavioral, genetic, and physiologic characteristics that indicate susceptibility and resiliency under stress. (2099, emphasis mine)

On the heels of this claim for credit in the diagnostic accounts of the clinic, the paper concludes with an intriguingly qualified promise about the literary future of the research program:

> Finally, [laboratory] and clinical research on the physiology and pathology of stress is moving steadily to identify regulatory mechanisms that participate in allostatic load [although] more *interdisciplinary research* is needed that recognizes the *full range of psychologic and biological factors* that intersect to enable an *environmental challenge* to precipitate a stress-related illness and allow resilient individuals to withstand such challenges. (2100, emphasis mine)

As in the case of the CCS and its CFI, an ecological or environmental ontology of the body here serves to ground and motivate an interdisciplinary inquiry into concerns of public health. The CCS organizes itself around the role of cultural variability in specific diagnostic cases; the CESAR studies the contextual variability of stress in the formation of pathology more generally. Here, McEwen opens the space for a new kind of black box in the literature — one which could link the existing literary entities of medical science with social and cultural factors of distress.

Last year, *Psychoneuroendocrinology*, an important mainstream journal in the field of biomedicine,[42] ran a special issue to mark the thirtieth anniversary of McEwen and Stellar's model of allostatic load. In the introduction to that issue, Juster — the director of the CESAR lab — argues that this research program is now beginning to turn the critical corner from theoretical elaboration to therapeutic application (Juster & Misiak 1). If practical interventions designed on the basis of this representational model gain traction and prove effective, the nature of the results produced by laboratories like the CESAR will dramatically change character, hardening from interesting theoretical claims to important medical facts. In his review of the "thousands of

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[42] At the risk of propagating over-investment in the imperfect 'Impact Factor' metric, Psychoneuroendocrinology returned a score of 4.7 in 2021, placing it in the top tenth of academic publications worldwide in terms of influence.

articles" informed by the allostatic load model, Juster identifies the maturation of a formal "index" of biomarkers, operationalized across "hundreds" of studies (1). The ongoing definition and refinement of this *allostatic load index* (ALI) promises the closure of this discipline's first major black box: an easily-recitable quantitative metric of stress, by which all of the relevant biological factors of allostatic load are made to *resist dissociation*. Through its ability to inform future research protocols and interpretations, the allostatic load index promises to fulfill Latour's principle holding that the fate of scientific facts rests in their *later use in the hands of others*.

The modality of the allostatic load index remains ambiguous — with a number of authors identifying inconsistencies in its construction across different studies, and heterogeneities in the experimental methodologies necessary to produce inscriptions of its composite biomarkers (cf. Juster et al. 2019; Edes & Crews 2016; Manigault et al. 2021). Nevertheless, the index remains at the heart of a growing literature that seeks to correlate in an increasingly strong manner physiological stress with psychosocial distress. In his review of the field, Juster identifies "more representation of diverse populations" as one of the most lively threads of this research program (Juster & Misiak 4). By interpreting the effect of various forms of "social injustice" on the ALI, such studies promise to furnish some information concerning the "complex" and "unique" resilience factors characteristic of a given subgroup (4); by affording a blackboxed method for translating "psychosocial and physiological indicators" into the same description of a problem, Juster also argues the ALI provides a "promising measure for routine use in clinical practice" (4).

In his positive modalization of these studies, Juster is quick to acknowledge the peculiar nature of their scientific information: due to the particularity of the social contexts in question, the "replication and generalizability" of the resulting inscriptions is limited (4). What we see here

is the same trade-off of leverage at work in the case of the CFI: symmetrical information, composed of both anthropological and biological inscriptions, is less stable, mobile, and combinable than that of conventional scientific research. The subfield's claim to credit thus rests on the same ontological argument of transcultural psychiatry more broadly: because the human body is a mediator of 'natural' and 'cultural' agencies, some kind of "ecological systems model" is required, in order to develop interventions that are adequate to the complexity of our situated embodiments (Juster et al. 14). In the clinic, physicians are more or less free to subscribe to this symmetrical ontology — there is no structural pressure, beyond the efficacy of their encounters with patients, to *prove* that this hybridity is 'real'. Life is different in the laboratory, however, where scientists are more subject to the competitive pressures of the literary credit cycle.

Proponents of the allostatic model at the CESAR must therefore find some literary device by which they might reliably associate the biological measures of the ALI with the sociological situations of their research participants. In the case of the STARS project, which uses the ALI to measure stress as a function of sex*gender, psychosocial 'inventories' are the necessary literary device; they provide the sociocultural mass that is missing from conventional stress science. Developed in the fields of sociology and psychology, the self-report inventory is a research tool designed to solicit structured information through a series of standardized questions; this information can in turn be used to construct profiles of a given group, or to compare individuals from within that group. Though I will present a deeper analysis of the STARS protocol in the following chapter, I at this point want to frame their use of sociological instruments as an important literary gesture — a positive modalization of one methodology by which sociologists define and give name to different identities and positionalities in a given cultural environment.

For one of the interdisciplinary credit cycles in which the STARS project participates through its use of self-report inventories, consider the *Handbook of Sexuality-Related Measures*, a sociological resource presented to all new members of the research team. The manual presents over two hundred distinct inventories for inscribing various dimensions of human sexuality — consisting of both quantitative and qualitative measures, it provides instructions for how to interpret data using analyses that range from the statistical to the symbolic. In the laboratory context, inventories join the long apparatus that converts experimental material from noise into order; by soliciting information on the sexual *performance* of research participants (in the Latourian sense of the word), inventories serve as the *trial* that stabilizes and names various kinds of sexual identity categories. In other words, these inventories become a part of the *instrument* by which the STARS produces inscriptions and constructs definitions concerning their new object — Queer stress can emerge from the laboratory as a harder and stronger fact, because this interdisciplinary protocol translates *sociocultural narratives of sexual variability* and *biological measurements of hormonal variability* into the same body of information.

Part 2. Sensitive Bodies, Sensitive Science

There are a few notable studies that have begun to contribute to the closure of the black boxes at play in the STARS project — those of specifically-situated forms of resilience, of hormonal physiology as a function of social position, and of the experimental protocol used in STARS to collect culturally-significant biological material. In 2021, Manigault et al. published a statistical analysis linking gender performance to cortisol measurements, using one of the *Handbook*'s inventories alongside the ALI. Their article cited two previous studies from the

CESAR as the only existing published research concerning the association between these objects of social and physical science (Manigault et al. 724). In the same year, Miller et al. reviewed eleven research publications concerning efforts to measure the impacts of social injustice through the ALI, concluding that there "is evidence that discrimination is associated with [allostatic load]" (Miller et al. 1). This third-type statement of fact speaks to the relative instability of the field's black boxes, and the paper's discussion section is full of first- and second-type statements concerning the research protocols of the studies in question (9-10). Such concessions regarding the quality of data in this field of research reflect similar statements made by members of the CMHRU — their frameworks for translating between the (asymmetrical) poles of conventional biomedical and sociological information require further solidification and refinement, and the credibility of mixed-methods or interdisciplinary research remains under construction.

However, one year later, in the hands of Dubois & Juster, the Miller review's basic suggestion of an association between discrimination and elevated allostatic load is taken up as strong enough to see that article *positively modalized* in the 'strengths' section of their article on allostatic load and gender variability. The Dubois & Juster article is significant for its use of "in-depth in-person interviews" based on an "ecological systems model" (Dubois & Juster 1). Ultimately interested in defining a "psychobiological link between lived experience and health," the study is one of the closest precursors to the ongoing STARS research, which also promises to associate "a range of contextual and experiential factors" — such as self-reported discrimination, sexual and gender identity, and various metrics of environmental or economic precarity — by "indexing allostatic load" in relation to them (1). When Dubois and Juster propose an ecological "model for understanding embodied experience and stress" in relation to sociological factors,

they articulate the potential of a new black box for symmetrical science — one which "can be used in [future] research" on health and resilience in Queer sociocultural environments (3). If the study of allostatic load is to move forward, this credit cycle must continue to build momentum.

Through their shared use of the ALI as a (more or less) objective metric for describing stress, their correlation of ALI values with statistical and thematic analyses of responses to sociological inventories, and their commitment to the symmetrical-ecological model of situated embodiment and stress, these papers all contribute to the increasing credibility of both allostatic research and transcultural psychiatry *within* the literary cycles of psychiatric science. For the time being, as in the Manigault and Miller articles, Dubois & Juster must acknowledge the difficulty of capturing symmetrical objects — such as socially-situated resilience to physiological stress — using existing research paradigms.[43] However, for these authors, defining the "underexplored" sociological dimensions of resilience is a critical scientific project (Dubois & Juster 1). This project, they argue, cannot be separated from the wider concerns of psychiatric research, and its urgency to public health justifies the methodological compromises that it requires. As proponents a relatively young research program, the laboratory scientists responsible for the construction of allostatic load leverage this politically-grounded claim to credit, so as to counteract the field's lack of fully blackboxed "consensus-based guidelines [. . .] for research and practice" (Juster & Misiak 4). Here, the scientists align their work with *external* political mobilizations in society — namely, the increasing intelligibility of Queer representation and rights-claims — to positively position and strategically reinforce their laboratory endeavours.

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[43] See also Flentje et al. (2019), page 19; Holz et al. (2020), page 392; and King et al (2020), page 1.

In a 2022 review written for *Biological Psychiatry*, Juster & Edmiston address fellow researchers in the field with a distinctly Latourian claim: "It is not possible," they write, "to depoliticize our work" (Edmiston & Juster 1254); the authors argue that the inclusion of new kinds of socially-situated information thus represents a critical opportunity to "improve neuroscience," rather than a compromise of methodological integrity (1251). For Latour, increased representation of nonhumans increases the political power of scientific actors: thinking through and with the agency of things enables new forms of mobility, and opens up new points of leverage in the face of social and technical problems. Edmiston & Juster, in their review, are attempting to make a similar point: by treating research participants from marginalized social positions as "reliable sources of knowledge," they argue that psychiatrists will access new forms of "expertise" (1255). This movement, from the "extractive model" of conventional research to the "partnership model" of symmetrical science, reflects the ontological and methodological shifts of transcultural psychiatry described above (1255): accessing the skills, expertise, and information of differently-situated forms of embodiment requires new tools and techniques of translation. It is on the basis of this ecosocial argument that psychiatrists — in both the clinic and the laboratory — justify, motivate, and build credit for their use of anthropological methods.

All that remains, for these counter-programs, is the work of building stronger alliances. Edmiston & Juster identify the ongoing project in psychiatry of reconstructing nosology as it relates to sexual performance and identity, discussing the diagnostic status of conditions such as homosexuality and gender variation in the DSM. They argue that these "shifts in terminology" represent the emergence of a distinct program of research in the field (1251). Where psychiatrists used to attribute pathology to these 'deviant' sexual identities themselves, the new namegivers of

Queer stress now ascribe the sources of disorder to external, sociological factors. This dialectical 'correction' of conventional nosology builds on the credit of "minority stress" theory — an interdisciplinary movement that is oriented towards the interpretation of stress as a result of marginalization (1251). By identifying with this growing counter-program, Juster situates the CESAR in relation to a wider movement in public discourse, and its growing influence in mainstream psychiatry. Through the strategic use of citation and captation, Juster positions his lab as a potential source of credible information for important sociopolitical projects to come.

In this section, I have described how the allostatic research program at the CESAR makes use of symmetrical representation in order to build literary credit, promising future forms of intervention which better respond to particular forms of situated embodiment. Drawing on literary discourses of contemporary public health — in which variable degrees of exposure to social stress *differentiate* bodies in terms of their vulnerability to disease and of death — scientists at the CESAR contribute to the ongoing reification, definition, and mobilization of different kinds of sexuality and gender identity *as objects of medical science*. By linking social categories to the already-blackboxed agency of specific physiological entities, the STARS research team promises increased representational fidelity to (and interventional leverage over) forms of vulnerability and precarity that have historically resisted capture by asymmetrical forms of clinical imagination. This project, which openly ratifies the political dimension that Latour identifies in all laboratory endeavours, seeks to leverage the objectivity of scientific information in service of critical clinical interventions into social structures or formations that are deleterious to the health and well-being of their communities of interest. Building on the concepts of the

biological milieu in McEwen's work and of the ecosocial niche in Kirmayer's, this current generation of physiological science seeks to develop a new point of leverage over stress and distress, by articulating in increasingly objective terms the unique positions and situations of these different kinds of bodies in their specific sociocultural contexts.

In the following chapter, I will demonstrate how these related literary programs inform their respective practices of science in action, by following their authors through the Montreal classrooms, laboratories, and clinics in which they continue to produce information and construct credit. As we move from the discursive themes presented above to a focus on the infrastructural and technological networks which operationalize and sustain them, our interest will remain in the conversion of physical material into scientific information. By following scientists through their daily activities in these three kinds of spaces, we will come to see with a greater degree of clarity the movement of that information outwards into society, where it ultimately comes to influence and reorganize bodily habits, technological delegations, and ecosocial environments. Of special interest in this movement will be those technological mediators of the alliances that exist between experts and their subjects, researchers and their participants, and physicians and their patients. In these scenes of science, those on either side of the encounter leverage the credibility of their own semiotic authority as a source of sociopolitical power and infrastructural mobility.

Chapter 6. Ethnographic Analysis

In the last chapter, I read some of the foundational literature of both transcultural psychiatry and allostatic stress research through the lens of my conceptual framework. I argued that this literature served to *contextualize* the activity of the university laboratory and the medical clinic, demonstrating both the *practical form* and the *theoretical content* of scientific argument as it occurs in research publications. I found that the literary activities of these related research programs construct similar accounts of the body as an ecosocial mediator — accounts which link shifting social categories or 'kinds' to stable biomedical entities. Through this effort to associate biography and biology more credibly, I have argued that transcultural researchers promise more effective interventions in scenes of disorder, mobilizing more allies and more resources in the name of resolving physiological stress and psychological distress. In this chapter, I will go into their spaces of work, and follow this science in action: first, in the production and presentation of facts, as it moves from material substance to semiotic information; and second, as it leverages its semiotic authority, redistributing resources for and around bodies under stress.

The narrative structure of this chapter will reflect my own trajectory through these field sites. First, I will describe the DSTP from a visitor's perspective — especially its Culture, Mind, and Brain workshop, where I first learned the basics of the emerging ecosocial philosophy that guides both laboratory and clinical efforts in transcultural psychiatry. I will argue that this kind of professional seminar is a space of *constructive proposition*: a meeting-place where researchers can work out how to join their names together, in order to build longer and stronger networks. Second, I will follow the substances of the body into and through the instrumental apparatus of the CESAR laboratory, tracing out the social and technical networks that are assembled by its

process of inscription and interpretation. Following Latour, I will define the laboratory as a space of *productive articulation*: a training-ground where facts can gather the momentum necessary to move out into the world, becoming influential enough to reorganize society. Third, I will tell the story of my involvement at the CCS, recounting the efforts we took to apply our ecosocial theory to specific embodied situations of distress. The consultation, it will become clear, is a space of *participatory organization*: in its dual process of representation and intervention, as diverse allies are asked to subscribe to the semiotic authority of scientific names, a peculiar kind of agency emerges. This agency will look familiar to the committed Latourian: the person in distress (by joining the therapeutic alliance, taking its name, and following its program of action) might transform their own stability, mobility, and combinability — and so find a new kind of strength.

Overall, this three-section story moves progressively 'outwards': from the conceptual object at the center of symmetrical psychiatry (the culturally-situated mind and brain); through the experimental networks that reify this object and modalize its credibility; and into the institutional networks of the clinic, where facts about the body and mind find the opportunity to refashion society in their image. Each section will take a similar structure. I will begin with a general narrative account of the field site, focusing on the *social procedures* and *technical conversations* that exemplify each site's embeddedness in the wider disciplinary processes of professional science. Next, I will draw out the salient *sociological structure* of the field site, articulating the complex alliances and interests at work in these scenes, and the different disciplinary trajectories that motivate their participants. Then, I will turn to the *technological structure* of each site, showing how, in Latour's words, "understanding *what* facts and machines are is the same task as understanding *who* the people are" (*SIA* 259).

(I) The Construction of Networks at the DSTP

Part 1. Field Story

On June 19th of 2023, I started my field work at McGill's Division of Social and Transcultural Psychiatry. Hopping off the Métro at McGill Station under a bright morning sky, I made the long and hot walk up the Peel street hill to the Ludmer building. The plain building — especially drab in the gothic shadow of the Ravenscrag manor next door — opened into an equally plain hallway, and an equally plain seminar room, right there on the main floor. A wide oblong table, a projector hookup with a tangled mass of wires falling out the side, and an all-too-familiar Owl device for hybrid conferencing in the middle of the room — it was an archetypal post-pandemic classroom, complete with muted and slightly awkward conversation amongst us early arrivers over our free coffee and snacks. This is the kind of space where scientific alliances are born, and at the low price of two hundred dollars, we had all secured our access. Just about thirty of us — eighteen researchers from across the biomedical disciplines, with nine presenters over a three-day schedule — had gathered here to talk about the cultural dimensions of the mind and brain. The purpose of this workshop, one of the more recent additions to the division's 'summer school' program, was to build "bridge concepts" between our approaches to research (I.18); through open conversation, organized around and prompted by the twelve sessions of presentations, we were going to figure out how to resolve the interdisciplinary tensions that stood in the way of deeper and more consistent collaborative inquiry into this shared interest.

The potential value of such a project to us participants might be easy to grasp: we could ask each other for help in clarifying research questions, for guidance in writing grants or

formulating claims fit to withstand the pressures of publication, and even for material support in our various laboratory endeavours (cf. I.20). From the perspective of the division, I have already identified one kind of value in hosting this workshop: like so many conference-spaces in the professional sciences, the CMB workshop was pay-to-play. In the first session, however, another, more interesting kind of value quickly became clear. Titled 'Co-Constructing Culture, Mind, and Brain,' and delivered by Laurence Kirmayer, the presentation consisted of a history of cultural psychiatry in relation to the mainstream discipline; a description of the field's emphasis on clinical contingency over biomedical consistency; and a swath of case studies bearing out the fundamental theoretical claims of the sub-discipline (I.10-23). In other words, this presentation served as a motivating argument in favour of the transcultural approach, preparing us participants for a kind of assumption into its research program (I.10). For a discipline like transcultural psychiatry, which still positions itself as, in part, alternative to the dominant literary regime of psychiatric factuality, this conscription of new allies is invaluable: if the workshop succeeds in its effort of getting us to speak in the idiomatic language of ecosocial philosophy, then the networks in which the division's facts might circulate become that much longer and stronger.

I have, at various points in this thesis, described the literary activity of science as being structured by a distinctive information economy — one in which the credibility of a given statement is a result not only of its conceptual consistency or theoretical validity, but also of its social currency. Events like the CMB workshop function to generate this social currency; the participants, that is, serve as a self-selecting readership against which scientists might test the legibility of their counter-statements. In our conversations during these sessions, we readily modalized the claims of each presenter, taking some up as high-type matters of fact, and

reformulating others as conjectural and speculative (cf. I.43, I.55). In this sense, the seminar room functions like a kind of semiotic laboratory, in which scientists are permitted to propose new associations, make mistakes, and adjust their strategies for moving outwards into society. Skillful advocates of a given science know not only how to articulate complicated ideas and fold convincing graphics into their presentations — they also understand how to take the temperature of a room, and shift their statements upwards or downwards in response. When, eight months later, I attended a lecture given by Laurence to a graduate class at the University of Montreal's faculty of medicine, his presentation of ecosocial philosophy was more tentative: anchored firmly to published literature, his argument in favour of the field was less based on its conceptual vibrancy than on its correspondence to experimentally-validated psychiatric findings (II.57-64).

One especially salient feature of these discursive spaces is the way in which they are fundamentally structured by a clear *social-semiotic reciprocity*. This reciprocal structure of the seminar room is clearly visible in two kinds of relationships: the *vertical* relationship between critic and presenter, and the *horizontal* one between different presenters. Both relationships involve an exchange of what I will call *social-semiotic solidity*: in the first case, listeners gain literary competency (and admittance into an existing alliance) by accepting the authority of a presenter as credible (cf. I.22); the second kind of exchange takes place in the common situation where presenters, after being invited to speak in one space, will then invite their hosts to speak in their own spaces. This increases both parties' domains of authority — Laurence's guest lecture at the University of Montreal, in a course taught by two CMB presenters, is just one example of this horizontal reciprocity. These exchanges of solidity — essentially a social strategy of mutual benefit — define the basic structure of the alliances that form in the classroom; and it is these

relationships that generate and secure the necessary stability and mobility for statements of fact. The more we might learn to 'speak for' each other's facts, the stronger we both become. In both cases, the information economy of science grows through a process of *cross-pollination* and *semiotic mutation*: as statements and facts circulate out from their sites of inscription and scenes of production, they must continue to 'make sense' to increasingly distant critics.

Of course, there is a harsh limit to the spirit of mutual aid in the sciences, and it is set by the competitive allocation of scarce resources, according to the stratified structure of the credit cycle. In my fieldwork at the DSTP, this came out most clearly in an interview with one associate professor, who had presented at another of the summer school's workshops. They described the horizontal exchange among researchers in a more pragmatic tone: the interplay of ideas in professional science, they suggested, does not always take the form of some flowery meadow, alive with the free movement of ideas from one node to the next; rather, it is often subject to a kind of strong conceptual gravity, whereby ideas on the fringes are increasingly warped towards their local centers. Things might look different at the DSTP — with its insistence on pluralism, and its own position on the margins of psychiatry — but some degree of homogenization, or of regression to the norm, is still at work (I.77). At the division, one mechanism of alignment is the bi-annual departmental meeting at which research priorities are set and reviewed (I.80). Not only a strategy of creative mutation, but also a herd mentality, in this survival of the fittest.

Part 2. Social Mapping

In understanding the social side of the DSTP, there are a few different scenes to consider. On a broad level, anyone formally associated with the department is bound together in a literary

alliance, sharing the same interests with respect to the credit cycle's process of stratification. The rising tide of credit lifts all boats, so to speak, as it is accumulated by the division's total output of publications. As their own field competes for recognition, the DSTP's members strategically position themselves in relation to other trends and movements in psychiatric and medical science. In one of my conversations with Laurence, for example, he spoke of departmental preparations for an important conference in the emerging field of Global Mental Health (GMH) — which, being adjacent to the domain of transcultural psychiatry, is at once an ally and a competitor (III.1, cf. II.4). Specific inter-institutional alliances also give shape to activities at the division: Laurence opened the CMB workshop by acknowledging the financial support of the Foundation for Psychocultural Research (FPR) in establishing the summer school, and in circulating its texts. The interests of those in charge of the field's major institutions of credit — the CIHR in Canada, or the NIMH in the USA — exert a strong ambient pressure on researchers; and the lower one sits in the institutional and literary structures of stratification, the more intensely this pressure is felt (I.79). For junior researchers, whose professional viability is directly tied to their ability to secure grants, carving out a unique niche comes at its own risk.

Within the division, distinct research groups form and collaborate more or less organically — one researcher at the division spoke of these "crew[s]" as being neatly associated with the literary interests of the most senior faculty members (I.79). One such group at the contemporary DSTP is organized around the field of computational psychiatry — highly favoured by the positivist intellectual regime of mainstream psychiatry, some researchers at the division have begun to think through how to translate cultural difference into computational models (cf. Constant et al. 2018, Vessière et al. 2020). Many of the more anthropologically-

minded researchers at the division (though by no means all) have strong feelings about this sharp turn towards the quantitative (I.79; I.82; III.3). In this instance, it is evident that the fault lines between social alliances in science result from allegiances to different methods as much as they are defined by one's support for any specific set of claims or positive content. At events like the bi-annual meeting, tensions between these alliances might sharpen, as the need to clarify a research program generates pressure to reconcile differences or make compromises (I.80). Both tactics, then, have their place in the literary landscape and its competition for credit — not only mutual aid, but also tribal exclusivity. Some representational models survive, others must be cast aside; and for researchers who have spent years elaborating these literary devices, any sacrifice comes with a real emotional cost (cf. I.32).

We are quite far, now, from the conventional imagination of science as a rational, unfeeling pursuit of impersonal truths. The texture of scientific space at the level of university departments is richly personal, alive with social strategy, and charged with economic pressure.[44] Hiring decisions are the source of bitter disputes and disappointments (I.77); the adjudication of tenure gives rise to broken promises and betrayals (III.10); and grant writing seasons bear witness to an "overwhelming" affective temporality of intense stress (I.79). It is no wonder, then, that social networks promising a clear trajectory through this fray are so commonplace, and so attractive. Institutions like the FPR — or the DSTP itself, for that matter — which are "dedicated to training a younger generation of scholars" (I.12), trade off on the competitive nature of the sciences to carry out what we have learned to describe as a process of *translation*. By convincing young scholars to make a detour through their walls, these institutions are able to invent new

[44] Scientific taxonomies, in other words, are built in spaces structured by locally-specific social conventions. Our philosophical shlemiel, Cratylus, would not be thrilled to acknowledge this denaturalization of scientific method.

goals (i.e., 'describe the brain in cultural terms'), invent new groups (i.e., the *Culture, Mind, and Brain Network*), and thereby become indispensable allies in their navigation of the credit cycle.

Laurence, for his part, insists that the purpose of the DSTP is not to create a restrictive 'school' or research program (III.3). Rather, he frames the division as participating in a more collaborative effort to "create an infrastructure" for culturally-situated, mixed-methods research into psychiatric problems (I.57). The result of this endeavour is still a social "network" (III.3) — but one which simply facilitates, rather than fully determines, the movement and exchange of resources, researchers, ideas, and techniques across contexts that remain locally-specific at every point. Thinking in Latourian terms, this is a project of translation less in the sense of *defining* interests than in the sense of *combining* them — an effort of hybridization, bespoke invention, and mutual instruction. If there are, as I argue above, two opposed tactics of surviving the credit cycle of science, it is clear which one this division strives for in spirit. Given the way the wind is blowing in the sciences — what, with McGill's nearly hundred-million dollar grant promising "big data analysis to reveal the fundamental mechanisms underlying normal brain function and brain disorders"[45] — the viability of such a commitment certainly remains to be seen. The fate of the alliances at the DSTP, meanwhile, depends on the continued loyalty of its members to this social network, their continued recitation of its credibility, and their continued use of its methods.

Part 3. Technical Mapping

If the project, then, of the DSTP is to create an 'infrastructure' for research, beyond these tenuous social connections, of what technological elements does that infrastructure consist?

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[45] See 'https://www.mcgill.ca/hbhl/about'

Unlike the laboratory and clinic, both of which involve clear and well-defined technologies of inscription and intervention, the classroom is primarily and fundamentally a discursive space. In other words, while those other spaces are organized around a technical practice of working with scientifically-captured materials (whether those be pharmaceutical agents in the clinic, or chemical reagents in the lab), the central activity of the classroom is conversational in nature — namely, teaching students how and what to think. Nevertheless, this discursive activity is not without its own technical dimension, and a suite of technologies proper to it. Where technicians in the lab and clinic transform substance into information through their expert use of *inscription devices*, the namegivers of the classroom make use of what I will call *instruction devices*. Inscription devices start with information and move it towards new names; instruction devices start with names and use them to train new dialecticians — future scientists or collaborators — in how to work with scientific information, or in how to make sense of experimental material.

In Chapter 3, I followed Latour in making a distinction between internal and external literature in the scientific laboratory. A similar distinction, I will argue, is at work in the instruction devices of the classroom, which can be classified according to a set of more or less formal literary techniques. The internal instruction devices include presentations given by senior researchers, but also the materials to which their lectures are tethered — syllabuses, websites, and handouts. In other words, any discrete resource arranged around the scene of instruction, insofar as it contributes to the stable transmission of information, might count as a technical device in the context of the classroom.[46] Importantly, in the contemporary classroom, it is not

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[46] I argue that this secondary material — even the 'external' scientific articles referenced in lectures or provided as readings — counts as 'internal' literature in the context of the classroom, because it was never opened up and studied directly. Rather, it stood on its own as an ambient support or prop for the authority of the presenter.

possible to disarticulate these semiotic resources from the host of technological mediators by which they are structured and communicated (I.13) — virtual resources such as filesharing or teleconferencing services; technical architectures such as the PowerPoint software; technological networks of electrification; and the whole digital infrastructure of the internet and its attendant hardware. These technical elements of the classroom are not arbitrary or matter-of-fact; rather, the choices that instructors make (which services they use, what style of lecture they perform) mark them as participants in highly specific technical traditions and technological networks.[47]

It is with its external instruction devices, however, that the DSTP most meaningfully contributes to the literary activity of the credit cycle. Among these devices are the textbooks, manuals, and guides which emerge out of these seminar-style conferences, and come to shape transcultural psychiatric practice in Montreal and beyond. For one paradigmatic example, consider *Cultural Consultation* — edited by three members of the DSTP, it describes the summer school as one of its formative sites (*CC* 344). The social structure of reciprocity is clearly at work in the formation of these devices: when Juan Mezzich — a transcultural psychiatrist cited extensively in *Cultural Consultation* — edited an internationally-oriented textbook of his own (*Person Centered Psychiatry*, 2017), he invited Laurence to contribute to two chapters. Here, we can see how understanding the technical structure of this field requires an awareness of its social structure. According to the latest available data, these technical resources have been virtually accessed over one-hundred-thousand times (III.2); as they travel through more and greater networks, they open up newly possible alliances for those who remain in place at the division.

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[47] Consider, for one very mundane example, the kind of subtle friction involved in being a 'Zoom' person forced to join a 'Teams' meeting, or being a 'OneDrive' person asked to collaborate on a 'Google Drive' project.

(II) The Production of Facts at the CESAR

Part 1. Field Story

As soon as disciplinary or inter-disciplinary networks are in place, they need facts to circulate through them; this continual generation of credit and credibility is what binds the allies together over time. At the CMB workshop, Laurence and Rob formalized their interest in each other's research programs and literary products; by bringing two student-researchers along to learn at the DSTP, Rob further helped to build out a robust network of translation between transcultural and allostatic literatures. Once Rob and his collaborators return to their own site, the focus shifts back to producing new information, such that future statements of allostatic fact might, in leveraging that new network, enjoy increased legibility and visibility. In this part of my ethnography, I will turn from the *social architecture* of scientific alliances to a treatment of the *semiotic material* which structures and sustains them — its origins in the murky substances of the human body, its transit through the fragile apparatus of the laboratory, and its eventual transformation into something clear and strong enough to inform a shared regime of factuality.

For all its climactic grandeur in the realm of concepts, this story begins on a muggy August afternoon on rue Sainte-Catherine, where, under a somewhat insipid soundtrack of pop music, two rows of tented booths lined the pedestrianized street from Berri to Beaudry. This was the setting of my first in-person encounter with the STARS research team, at their post on the route of Montreal's 'TD Pride' festival, in the city's Gay Village. Here, they were hard at work in the recruitment of participants for their study (I.84). Their presence at the festival belies a few important aspects of symmetrical laboratory research. First of all, there is the notable fact that, before any participant can bring their potentially-meaningful substance to the laboratory, they

have to ‘prepare’ the sample, by moving through society. In other words, there is a prologue to the standardized trajectory of somatic material *through* the laboratory, one which consists of a human body’s unique trajectory *outside* of the laboratory. The very ‘non-scientific’ activities of our participants — as they encounter stress, and build resilience — are what set the stage for all our later activities of reification and interpretation. The first act in this laboratory drama is here, at the scene of enrolment, where members of the research team deploy tactics of “community outreach” (and financial persuasion) to solicit these walking archives of allostatic information to “contribute to science,” and make a detour through the walls of our center of calculation (I.110).

A second notable fact about this particular scene of enrolment concerns the CESAR lab’s strategic efforts in social positioning — not just as a site of *Queer research*, but also as a *Queer site* of research. By participating directly in a community event; by signifying competence through specific stylistic and linguistic choices (such as the use of Queer-coded facial jewelry and pride flags, or the use of culturally-specific terminology and the ‘vocal fry’) (I.84); by prominently featuring their commitment to honouring “lived experiences,” and to treating Queer stress not as an individual pathology, but rather as one symptom of social injustice (I.84) — all of these positioning tactics contribute to the success of the CESAR’s efforts in aligning their own interests with those of the Queer community. What results is a mass recruitment of new allies, as members of the community ‘out there’ are translated into the laboratory network as participants.

If the initial enrolment of these participants is the opening scene of the STARS’ first act, its closing scene would be the screening process, by which eligible participants are identified, named, and scheduled for their laboratory trials. This process, which takes place over the phone, is the first step in producing refined scientific information: by subjecting participants to a series

of sociologically-oriented demographic questions, we acquire the information we need to classify them within our experimental taxonomy of sexuality and gender (I.75). Those who fail to fit into relevant terminological categories are excluded, or set aside for future dialectical consideration.

The next act of the laboratory drama may seem relatively insignificant, but it is in fact the stage of laboratory research to which the most collective time and attention is dedicated: the review and preparation of laboratory documents and procedures. Updating calendars (I.125); ensuring consistency between protocol manuals (2.9); printing, arranging, and filing documents for each experimental encounter (I.128)— these administrative tasks dominate the daily life of technicians at the CESAR. All this activity demonstrates the importance of internal literature to successful research: if the movement of these documents is not stably and reliably coordinated, no credible information will ever be able to exit the laboratory, and all the work of experimental inscription will never have the chance to disappear behind the facts that it is intended to produce.

Only once the work of naming and scheduling participants is complete — and only if the work of preparing the internal literature has been carried off well — can the first 'trial' of the STARS project take place. This trial consists of three main scenes: first, the administration of a multi-inventory questionnaire, designed to solicit the 'cultural' side of STARS' symmetrical information; second, the use of the Trier Social Stress Test (TSST), a "validated psychosocial stressor" designed to induce a physiological stress response (I.105); third, on either side of the TSST, the collection of saliva samples from participants. Ultimately, it is the variation of hormonal levels across these samples that will indicate the 'performance' of the experimental object — that is, the human body — under the conditions of the laboratory trial; and it is the interpretation of this information that will produce new facts about stress, allostasis, and

Queerness. Once the saliva has been collected, the act of inscription — of transforming it into meaningful data — depends on its rapid stabilization in one of the CESAR's industrial freezers, because the physiological substance begins to decay as soon as it leaves the body (I.105). Here, the preparatory work of internal literacy contributes to the technical skill of researchers: each vial is manually labelled ahead of time, to enable its proper storage and future identification (II.37).

This third scene of the trial — the collection of physiological substances — moves out beyond the laboratory as well. In order to produce stronger facts about the stress patterns of its participants, the STARS project also requires data about hormonal fluctuation over the course of multiple days (II.10). In this part of the research process, participants are sent home with more pre-labelled vials, and detailed instructions on how to produce and store the samples. This activity represents one form of what Latour would call the *extension of laboratory conditions into society*: in order to generate the strong information we need, the STARS team now has to train its participants to *become increasingly reliable allies*. Overcoming the many difficulties of this task ultimately required enlisting the help of further allies still, in the form of a technological device which recorded the times at which our human participants accessed their mobile sampling kits (II.27). Our delegation of responsibility *away* from participants and *towards* this device clearly demonstrates the utility of hybrid alliances in the work of laboratory extension.

The final act of the STARS experiment is the process of salivary cortisol analysis — the moment of inscription, at which physiological substance is converted into statistical information. The analysis involves four main scenes: preparation of the substance; operation of the inscription device; production of measurements; and management of inscriptions (cf. II.32-37). In the first scene, the laboratory technician retrieves samples from the freezers, according to a work

schedule provided by research directors. The use of auxiliary instruments (such as centrifuges and agitators) refines the substance, rendering it suitable for analysis; the saliva is then loaded into plates which are coated with a solution of rabbit cortisol antibodies. In the second scene, the salivary substance — itself the product of *the body's* performance in the TSST trial — is now subjected to a further trial of *its own* performance, in which it is made to 'compete' against synthetic enzymes: both substances bind to the plate's antibodies, according to their respective concentrations. Following an incubation period, the technician washes the plates of any unbound material, and adds a further reagent to induce a colour-change in each plate. The resulting colour reflects the amount of salivary cortisol that 'outcompeted' the enzymes. It is this variable intensity of colour that is measured in the third scene: the well is passed through an attached device called a spectrophotometer — quite literally a black box on the work bench — which produces a quantitative inscription of the whole series of trials. Finally, the technician reviews all the data digitally — plotting it onto a graph, excluding any outlying artefacts, and comparing each set of results against a 'control curve' to determine the actual cortisol levels in the saliva.

What has taken place here? From this inscription, as a reification of the moment at which a given person underwent our experimental trial, a clear line can be traced — one which leads from hundreds of dispersed bodies out in Montreal society, and moves through their mouths into thousands of small plastic test tubes; it leads from this watery, indistinct mass of saliva through the brightly-lit environment of the laboratory; the material becomes increasingly isolated, as it is refined, rinsed, and redistributed; the nonhuman agents within the substance — the ones we want to hear from — are continually stabilized, moved around, and recombined. At the end of this long arc, the material disappears completely, replaced by information which is said to represent

not only the qualities of a physiological substance, but also the quality of those bodies and lives which produced it — how stressful they are, how they organize resistance against harm. The laboratory choreography that gives shape to this trajectory is not simply of our own making: in order to *describe* stress and *ascribe* its causes, technicians must *subscribe* to the authority of their blackboxed instruments, following *prescribed* programs of action; they are *conscripted* by these devices — not just as autonomous agents, but also as subservient allies. The social authority of any claims they make about stress rests on this basic and mutual exchange that takes place at the scene of inscription — yet another structure of *semiotic reciprocity*, under which one set of actors, by contributing to the solid objectivity of the other, intensifies its own credibility.

In the coming chapter, I will return to this kind of newborn information, and follow it on its movement into the literature, back into the credit cycles of fact construction. I will think through the internal deliberations in which researchers transcribe it into its literary state, as well as the external conversations in which differently-positioned readers or users either affirm or contest its authority. Finally, I will review its symmetrical constitution, and consider some of the implications of these arguments about stress — especially insofar as they represent a peculiar understanding of the body as a kind of sensitive instrument, one which might both *take measurements of* and *make changes to* society. For now, suffice it to say that this technical information is of a thoroughly social nature: supported by the social position of a Queer laboratory, structured by the social authority of scientific instruments, and destined for the social networks of literary alliances. This being the case, it will now be useful to turn to these relations, and map out the interests and infrastructures at work in the CESAR to a greater degree of clarity.

Part 2. Social Mapping

If the activities described in the previous paragraphs suggest some of the technical elements of producing credible facts at the CESAR, the connections that I will describe in this part demonstrate the social elements of constructing and leveraging scientific credit. The social alliances that constitute a research laboratory exist on a continuous spectrum, between those that are more 'internal' to the institutional structure of professional science, and those that lead 'outside' the lab and connect it to wider society. I described the importance of the CESAR's external links to Queer society, referred to by the STARS team simply as "la communauté" (II.30), in the previous part of this section. The key institutional alliance, meanwhile, is to the Signature Biobank — this institute is the home of the assay instrument[48] used in the salivary cortisol analysis, the freezers used to store samples, and large databases of quantitative information accumulated by past studies (I.130). Through the Biobank, the CESAR is linked to two other research centers in the building: the *Centre d'étude sur le stress human* (CESH), and the *Centre d'étude sur le trauma* (CELT). Rob's former PhD supervisor, Sonia Lupien, directs the CESH — his lab's alliance with the Biobank, critical to the technical strength of his work, is thus structured by a social connection (I.67). Both aspects of this alliance are reciprocal: the more studies to which the Biobank contributes, the more gravity it enjoys as an ongoing center of calculation; similarly, the more credit accumulated by Rob's texts, the stronger Sonia's network becomes, and all the allies enjoy an increased visibility and circulation of resources (I.110).

And indeed, the CESAR's social network continues to grow: what began as a two-person research team has expanded to accommodate twenty-five student researchers by its fourth full

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[48] 'Assay' is a technical term referring to any test to find and measure the specific amount of a given substance.

year (II.52). As all of these members make their detour through the CESAR, a now-familiar reciprocal exchange takes place: they *contribute* to the credibility of the centre's studies; and the centre *attributes* credit to them, in the form of named authorships, letters of reference, and (in some cases) bursaries or salaries. Besides this internal distribution of credit, what pressures and tactics of translation keep the interests of the research team in alignment? To the extent that Juster affords his student researchers significant agency in designing studies (I.67, II.51), projects like the STARS represent the collective invention of new groups and new goals. This stratified process of alignment also takes place at a series of events much like the DSTP's bi-annual meeting: reading groups in which, under Juster's direction, the whole CESAR team convenes to interpret relevant literature (I.87); presentation rehearsals, where researchers test out different modalizations of their own statements before publication (II.54); and inter-lab meetings between the CESAR, the CESH, and the CELT, where each group shares findings, discusses the allocation of resources, and decides on new directions for their shared research efforts (II.21-26).

Beyond the lab, the ascendancy of specific research objects in the literature — such as the 'blood-brain barrier', which Rob identified as a special interest of the *Journal of Psychosomatic Medicine* (I.67) — might alter the priorities and methods of a given study: in order to link the findings of the STARS to this newly socially-salient object, Rob shifted resources to carry out a more comprehensive analysis of participant blood (I.111).[49] A month after the STARS meeting at which he proposed these methodological changes, Rob was fully enlisted as an ally of the blood-brain barrier and its research program; at a conference hosted by Signature, he spoke to his gathered allies at length of the virtues and promises of pursuing information along that path

[49] This exemplifies one kind of agency that Latourian scholars learn to attribute to nonhumans: as an object of study becomes the *shared concern* of various interpreters, it can be said to *assemble* and thus *act upon* that network.

(I.129). The social strategics of laboratory life are clear here: in order to gain access to a given literary alliance, the CESAR has to invest in the objects which that alliance holds in good credit.

I began this section by suggesting that the first use of socially-constructed scientific alliances is for the traffic of new information. Just as social spaces like the conference room serve as vectors by which scientific objects like the blood-brain barrier are *imported* into the CESAR laboratory, they are also a crucial vector in the the *outward* movement of CESAR-produced facts and information. In other words, this *initial* movement of network-construction — the circulation of new objects — may be seen as the *final* stage of fact-production. In order to preserve the stability of this cycle, Rob was constantly engaged in maintaining and making use of the socioliterary networks that extended outwards from his lab: over the course of my nine-month ethnography of the CESAR, he attended academic conferences in Los Angeles, Frankfurt, and Toronto, in addition to the many that took place in Montreal and online (II.1). This process of taking the CESAR's spit on tour — bringing the information it generates into new spaces, in the articulation of new claims — is crucial to the position of the lab within the scientific structure of stratification (II.52). In Frankfurt, for example, Rob met with members from the lab of Clemens Kirschbaum, designer of the TSST, and a key figure in the biopsychosocial stress literature. By linking the allostatic model to the existing credibility of the TSST, Rob sets up the conditions for a powerful exchange of semiotic solidity: Kirschbaum, after all, is much more likely to support the circulation of results that are based on his own methodological instrument.

One final way to observe the social dynamics of these alliances is through the informal conversations — as commonplace at the laboratory as any other workplace — through which colleagues speculate and reflect on their trajectories through the professional discipline. At the

CESAR, these conversations intensify in the grant-writing seasons, and senior researchers share their skills, advice, and experiences with younger technicians hoping to secure crucial funding and legitimacy (cf. I.109, II.10). The STARS project team consists primarily of student researchers, and has no staff members on site full-time. Ensuring that enough technicians would be at the laboratory to move participants through the experimental protocol was thus a constant concern (cf. I.121); in the grant-writing seasons, when researchers must dedicate additional time and attention to making claims for credit on their own behalf, arranging this more menial laboratory work becomes a source of heightened frustration (I.125). A kind of reciprocal gift-politics of workload-sharing emerges under such conditions (I.129): senior researchers promise editorial assistance on applications,[50] and junior researchers are expected to carry out the lion's share of the hands-on experimental labour in return (II.1). Through these social processes, laboratory life rolls on. Each actor who passes through the lab takes their own place and assumes their own role in the unfolding drama of scientific research.

Part 3. Technical Mapping

In Latourian terms, the inscription device at the center of the STARS is the competitive cortisol enzyme-linked immunosorbent assay (or ELISA), nicknamed 'Plato' by the lab technicians (because it handles plates). The ELISA bioassay, to put it mildly, is an extremely complex mechanical device (and Plato is a particularly sensitive model), and its function involves incredibly opaque principles of biochemistry (II.35). Suffice it to say, no one at the

[50] With respect to his own security of credit, Juster spoke openly of the importance of "doing it strategically" (II.52). In one case, the differing eligibility criteria for holding future research chairs became a factor in his grant-writing decisions; in another situation, Juster advised one of his senior researchers to think about "the normal path for a biostatistician" in their decision about a job offer (II.21).

CESAR lab can readily give a full and clear account of exactly how the thing works, and there are only a few technicians in the building who can reliably get it to produce coherent results (II.34). Nevertheless, it is only when these scientists stand next to their machine and point to the production of data on its screen that the hard facticity of this information can be 'guaranteed'. This hybrid of machine, computer, and man is — at last — the foundational scene of our science.

These 'black boxes' are an achievement representing the past settlement of major controversies in science;[51] once stabilized, they can be taken up in the production of credible facts. Until someone comes along to challenge the authority of this hybrid instrument — a challenge which would ultimately involve reopening those long-settled principles of hormonal chemistry — Plato will go on functioning as one of the countless 'black boxes' that enable the continual and predictable generation of scientific information. If, as has been the case with the ELISA, they work as predicted and promised, they will remain more or less safe from any attempt to destabilize them, and the old controversies will not have to be reopened.

But these expensive devices do not come to the laboratory from nowhere. Behind any scene of inscription are the long and strong social networks that hold the black boxes in place, organized by the promise of future credit. In the case of the CESAR and its affiliate laboratories, the directors had to leverage their scientific authority to convince funding agencies and institutional bodies (such as the university, the Canadian Tri-Council, and the CIUSSS) to finance, house, electrify, and otherwise support these processes of instrumental research (I.112).

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[51] In the case of the ELISA, for example, long and arduous debates as to whether it was possible to detect concentrations of biological materials through the activity of synthetic enzymes had to be settled before any institution of science would invest in the development of such technology. Once these disputes were settled into credible black boxes of theory, it took ten years for the practical black boxes to be stabilized (See Lequin 2005).

Without some social credibility to begin with, there would be no way to equip the laboratories and recruit the technicians that are necessary to the ongoing solidification of scientific authority.

It is time for a brief review, then, of the heterogenous technologies at play in the credit cycles of the CESAR and the STARS. What nonhuman agents are recruited by the lab, brought in to stabilize this precarious transformation of disorderly material into credible information about Queer stress and Queer life? Facing society, the lab deploys agents of authority and persuasion: we observe the use of university letterhead in the solicitation of participants (I.84), or the promise of financial reward in the organization of professional trajectories that brings student researchers to the workbench. Social technologies such as digital calendars (I.88), online filesharing services and archives (II.28), and communication platforms coordinate and give shape to these relationships. The 'validated' surveys of the STARS occupy a similar liminal status between the social and the technical (II.1). On the more material side, these blackboxed inventories must be administered through laboratory computers in each testing room; the TSST, for its part, only works if the microphones and speakers in the *salle d'expérience* function properly (II.28). Further along the laboratory's assembly line, the practical durability of plastic sampling kits will link up with Signature's formidable array of industrial freezers (costing thousands of dollars a month to operate and secure against malfunction) (I.109). Plato and the spectrophotometer are strong devices, but they can 'raise the world' not an inch without the Biobank's centrifuge (II.33); and in order to secure the constant flow of coated plates required to handle the saliva, dozens of rabbits need to be continually held in place at an industrial facility, where their cortisol antibodies are converted into useful nonhuman reagents (II.37). Obviously, the list doesn't end there either, as all of these auxiliary devices have their own networks too.

Latour argues that the distinctive qualities of scientific information are its stability, mobility, and combinability; it is possible to read in these very terms the role of technological allies in the process of fact production — they *stabilize* physiological material, they condition its *mobility* through the lab, and they compose the instrumental structures in which it is *combined* and transformed. By bringing matter closer, in this way, to the condition of information, the eventual conversion of flesh into fact becomes possible. CESAR's technical networks, which join biological instruments to anthropological ones, enable 'cultured' material — gay spit, so to speak — to remain symmetrical as it leaves its sites of inscription behind, and so becomes 'real'.

From an ethnographic point of view, it is not possible to give a full account of scientific credit cycles without continually crossing this great divide between sociology and technology. This being the case, it is perhaps not unsurprising that participant-observers of science have been hesitant to accept the claims of old-school scientists that their own research — with its famous universal method — somehow functions non-dialectically, exists apart from cultural forces and conventions, and works on an isolated nature that comes out of the labs as if from nowhere. Instead, our rearticulation of laboratory work seems to confirm a long-held anthropological suspicion that cultural and natural environments are, in fact, somehow one and the same. The CESAR lab, by openly accepting this hybridity of the social and the material, moves closer to a practice of science for which symmetrical anthropologists have advocated since Latour.

So far in this chapter, I've used Latourian rules of method to follow the *representational* side of science in action — its *production* of technical facts, and its concomitant *construction* of social networks around those facts. I've followed the still-unfinished processes by which scientists at the CESAR attempt to blackbox allostatic load and Queer stress (Rule 1), and have

identified some of the literary alliances in which their claims will be taken up and transformed (Rule 2). I have suggested that nature and society — or 'stress' and 'Queerness' — are concepts which will only emerge out of the stabilization of these literary processes (Rule 3), and have traced out the list of heterogenous actors involved in the settlement of their controversies (Rule 4). Finally, I have reviewed some of the various strategies scientists use to extend their laboratories and lengthen the networks in which their facts might circulate (Rules 5 and 6).

Now, as I transition to an ethnography of the transcultural clinic, my focus will turn more to the *interventional* side of science in action: it is in this kind of space, where our new facts and techniques are put into use, that their form and their fate will be conclusively determined (First Principle). In the clinic, the scientists will 'speak in the name of new allies that they have shaped and enrolled' — leveraging laboratory resources to build stronger associations, and 'tip the balance of force in their favour' (Second and Third Principles). By finding out which networks become the longest, we will see which facts turn out the strongest (Fourth Principle); we will also see, in cases where oppositional networks come into contact, the social physics which cause some facts to be displaced, while others are hardened (Fifth Principle). In tracing this movement of resources — the history of their 'mobility, faithfulness, combination, and cohesion' — we will arrive at a solid history of transcultural psychiatry in Montreal (Sixth Principle). As this science moves into society, it will come to structure a new kind of encounter with the body. Its initial scenes of production, which began to disappear with the transformation of material into fact, remain blackboxed; what is at stake in the clinic, then, is something like a scene of *deployment*. Certain bodies, in other words, in being asked to *internalize* scientific information, function as instruments for the *extension* of science, and so for the *reorganization* of both society and nature.

(III) The Organization of Names at the CCS

Part 1. Field Story

In Chapter 5, I followed Dinh et al. (2012) in describing the five stages of case conferences at the CCS: (1) psychiatric history, (2) cultural formulation, (3) interdisciplinary assessment, (4) interdisciplinary deliberation, and (5) closure. In this part, I will supplement Dinh's description by identifying three basic activities that take place across these stages: *symmetrical interpretation*, *hybrid networking*, and *strategic naming*. These three activities constitute the technical practice of transcultural psychiatry in the clinical context of the CCS: proceeding directly from a hybrid representation of the body and its pathologies, they contribute to a kind of social reorganization or reproduction that aligns with the ecosocial theory of stress and medicine. Before getting into the weeds of this argument, I will begin with a brief narrative account of five emblematic case conferences in which I participated.

In July of 2023, just before before participating in the CMB workshop at McGill, I was invited by Eric Jarvis to observe a session of the CCS. The conference team that day consisted of Eric (the chairing psychiatrist), a peer-support worker from the ICFP, a graduate student in the social sciences from McGill, a resident in psychiatry at the JGH, and the referring physician — a coordinator of Montreal's *Clinique des demandeurs d'asile et réfugiés* (CDAR). The conference concerned a South American man who — having been incarcerated in his home country, and tortured over his alleged participation in a terror organization — had fled to Canada, and awaited adjudication of his asylum claim (I.1-9). At the time of our conference, he was in recovery from a severe workplace injury, and held a formal diagnostic status of PTSD. We reviewed his

migration story, consulted the documents in support of his diagnosis, considered his symptoms and medications, and discussed the political conditions and colonial history of his country of origin. In light of this far-reaching conversation, the team quickly established a primary care objective: to support his asylum claim, so as to address the "deterioration of [his] mental status" before the threat of deportation (I.4). At work in this conference, then, is a *symmetrical storytelling*, whereby a migrant's *somatic stress* is ascribed in part to their *political status*.[52]

Three months later, by early September, I had observed two similar case conferences, and began to participate more actively in their interdisciplinary conversations. My fourth conference at the CCS had a slightly different composition. This time, the patient in question had themselves requested referral to the CCS, as they sought support in contesting a court-ordered prescription of anti-psychotic medication. The conference team consisted of Eric and his psychiatry resident; we were joined by the patient's family doctor, and two physicians from *Doctors Without Borders* (MSF), both trained in medical anthropology. Our discussion involved the typical themes of a CCS conference — on the social side: family dynamics, work and labour, religious affiliations, linguistic status, education; on a more technical register: medical conditions, physical symptoms, and pharmaceutical delegates (I.100-104). Of special concern in this case, however, were specific social and political economies of race: the patient, a Black Quebecois man, had been officially named as a psychiatric subject in the course of a judicial trial, at which he had not been able to afford his own legal representation (I.100). At work here was an accounting of the patient's *social position* as a factor in his *delegatory agency*. He was resisting institutional impositions (by the hospital and the court) of a diagnostic status; our goal was to reorganize those institutional

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[52] What is important to note here is the way in which transcultural storytelling thus allows for a *political* intervention into *somatic* disorder; our act of 'relief' is still directed *to* the body, but goes *through* society.

relations — to intervene in the "therapeutic alliance" between doctor and patient (I.102) — by instructing the physician in a sociopolitically-aware form of "cultural humility" (I.104).

Two distinct conferences from the Winter of 2023 demonstrate the CCS' use of specific cultural concepts as heuristic tools in the interpretation of psychiatric problems. The first case involved the mother of a migrant family from the South Asian subcontinent; the second case concerned a permanent resident in Canada, whose family remained in their country of origin in Southeast Asia. Eric described both women as "deactivated" (II.17; II.46) — suffering from depressive symptoms as a result of the complicated cultural dimensions of their situations in life. The mother of the first family had been victim to state-sanctioned sexual violence in her country of origin; described as "constantly being pulled back by what happened to her" (II.15), she was also subject to the "unbearable anxiety" of having to narrate these experiences at an upcoming asylum hearing (cf. I.70). Given the taboo status of sexual trauma in the South Asian cultural context, we knew this would be an almost-impossible task — only a further infliction, by state institutions, of stress and harm. Our goal was to supplement her restricted agency in this hearing, by defining her "impaired testimony" as "medically-indicated" (II.18) — a legally-admissible sequela of PTSD.[53] In the second of these cases, our intervention was targeted less towards the courtroom than the therapeutic alliance: in our discussion, we approached the nurse's depression through the articulation of four cultural concepts or idioms of familial obligation, which had become salient in light of her inability to return to her family and help during the pandemic (II.45). This anthropological perspective on her depression allowed the referring care-provider to

[53] This intervention can be read as part of a highly-localized clinical credit cycle: when the subjects at the center of our therapeutic alliances are deemed "just not credible" as a result of their fragmented testimony, we need to find ways to positively modalize their statements before the court (II.3). The PTSD diagnosis performs this function.

reimagine the nurse's medical condition as culturally-inflected (II.47); any treatment that followed would have to be informed by the social constitution of her stress and distress.

The final case that I'll discuss in this movement took place at the end of my fieldwork, in the Fall of 2024. As a bookend to my involvement at the CCS, its details are similar to the first conference that I attended. In this case, the person under discussion was another male migrant — this time from the Caribbean, but also a victim of organized violence — awaiting the adjudication of their asylum claim (III.6-9). What differentiated him from the South American subject of the first conference was his ambiguous diagnostic status: while the first patient had a well-documented and consistent presentation of PTSD, this person's symptoms were nonspecific and unpredictable. In Eric's words, there was "a strange thing going on" in this case (III.6).

The CCS engaged with these unusual clinical facts — with this body that resists assimilation or conversion to diagnostic order — through a kind of strategic and pluralistic namegiving. Resolved as they are to solicit a clear and coherent narrative of events that are inherently disruptive and deeply disorienting, the asylum courts need one kind of name: a PTSD diagnosis, with its socially-authorized explanation of fragmentary narratives, would clearly be "helpful for his application" (III.9). But "less so for him," Eric figures, as long as he resists our psychological interpretations of his symptoms, and continues to pursue a neurological diagnosis (III.9). The subject in question, then — this person who finds themselves in a complicated social and political predicament — needs another kind of name, another kind of explanatory niche: a way of narrating and representing his distress which, by acting as a "face-saving maneuver", might help to ameliorate it (III.8). We thus instruct the referring team to engage constructively with the patient's claims of epilepsy, in order to "help him creatively adjust" to his disorderly

symptoms (III.8). The diagnosis of his epilepsy might be dubious, but the reality of his distress certainly is not: here, where there are "cross-cutting agendas" of fact and narrative, this peculiar disorder might "be epilepsy at the same time at PTSD, at the same time as something else" (III.9). Both kinds of CCS interventions are at work here: one faces the courts which, as representatives of society, demand "simple diagnoses" and "straightforward accounts" of migrant bodies (III.9); another faces the clinics which, as the site of a therapeutic alliance, must make room for all the hybridities and complexities of situated embodiments.

What is at stake in this difference of narrative forms? One way to interpret what we see here is the coming-into-contact of two counter-networks, two opposed regimes of facts and concerns. On the one hand, there is the federal court — its vigilance against false asylum claims operates on the basis of a juridical grammar of accountability: you must 'prove' that you are a 'real' refugee in order to gain access to its network of resources. On the other hand, there is the transcultural clinic: its primary interest is not in the 'reality' of one version of events over another — or even, to some extent, the 'reality' of one diagnostic status over another — but rather the symmetrical dimensions of a undeniably real and embodied predicament.[54] The first priority of the clinic is continuity of care; what stands in the way of this, in many CCS cases, is the threat of deportation. The first priority of the court is the determination of facts; from this perspective, the spectres of fabricated narratives or artificial symptoms are obviously problematic. Latour's Fifth Principle describes the situation well:

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[54] Eric describes the salient difference of the transcultural approach in these terms: "[The CCS goes] to the people on the ground, [to listen to] what they mean by distress, or depression — like, what do they mean by that? How does it fit into their worldview, their cosmology, their belief systems; what are the homegrown treatments or interventions people might use? And then we're interested in knowing it — that's how we work on it. It's a different kind of an approach, it's more anthropological" (II.4).

> Irrationality is always an accusation made by someone building a network over someone else who stands in the way; thus, there is no Great Divide between minds, but only shorter or longer networks; harder facts are not the rule but the exception, since they are only needed in a very few cases to displace others on a large scale out of their usual ways (*SIA* 259).

In what remains of this chapter, it won't be my business to litigate any such accusations. For what it's worth, I think that the courts put migrants and their families in unjust situations, exposing them at the height of their vulnerability to the "idiosyncratic power" of unpredictable judges (III.3);[55] I think that the clinic, insofar as it intervenes in these ethically-dubious power dynamics, does interesting and important work. What I'll describe, instead, are the sociotechnical networks that structure the CCS; the strategies and agencies involved in the extension of these networks; and the kinds of order, sensitivity, or meaning that they try to construct.

Part 2. Technical Mapping

Because, like the DSTP, the CCS is a primarily discursive space, my review of its technical network will be brief, and descriptive in nature. For this reason, and since it will expediently set the stage for my review of the more complicated social constitution of the CCS, I will carry this part out first. Once again, I'll make an *ad hoc* distinction between the inside and the outside of a scientific field site, and identify a difference between *internal* clinical technologies and *external* ones. For the most part, the technologies I have in mind here are different kinds of formal documents. On the internal side, there are medical and legal reports — which outline *objective* facts about the patient in question — and the report produced by the Cultural Formulation Interview (CFI) — which contains a more *subjective* set of facts. On the external side, the most important kind of technical device involved at the CCS are the letters of

[55] According to Eric's estimations, the approval rate for some of these judges is above three quarters; for others, it is below one third (II.8). Some judges are receptive to our recommendations; others are strongly oppositional (cf. II.3).

support that Eric often submits to the immigration courts *via* an asylum claimant's legal counsel. Finally, there are a few important infrastructural networks to consider.

The medical charts which we review during the first stage of the case conferences are, along with the CFI reports, the main form of internal literature that circulates in the clinic. In other words, it is on these "working documents" that the stress and distress of the patient in question is initially inscribed (I.100); and it is through our use of these technical documents that the disorder at hand is converted into transcultural information — rendered less noisy, and so more orderly. In the case of refugee patients, court documents contribute importantly to this process of reification. Most often, these are "basis of claim" documents, which outline the biographical details of a given asylum claim (II.17); or else, the highly sought-after 'brown paper,' which confirms an approved asylum claim (I.96). Both give shape to the clinic's activity.

Like most medical charts, the document summarizing a given CFI encounter enters the space of the CCS in the form of an "evolving report" (II.3) — these technologies of order remain the site of continual reinscription and reconstruction until they must leave the clinical network. While medical and legal documents, by concealing the role of the author in their production, typically remain circumscribed in the realm of objectivity, the CFI report is more reflexive and relational. These documents often explicitly inscribe the clinician's own interpretation of an interviewee's affective state (I.70); they are emotionally dynamic, and essentially narrative in nature (II.18). Eric describes the interview process and product in these terms:

> [The interviews] are so dependent on a relationship. They're so embodied, I would say. They're real, they're physical; you have to see people sweating, you have to see people at unease, or restless, walking around — how do they breathe? It's all really relevant. I need those cues, because I often don't know the culture very well, or their language, even. So I really depend on those physical signs of what's happening (III.5).

This description clarifies the ways in which the clinician's body is enrolled as a technical ally in the production of clinical order — a kind of sensitive instrument of ethnographic inscription. These documents, which "add in a little cultural material" to the scene of interpretation (III.5), are what enable the CCS to produce symmetrical accounts of a given problem.

The last kind of technical document to treat in this section is the Letter of Support. These letters, as I'll continue to argue below, function as an important lever of intervention for the CCS. Consistent with the ecosocial philosophy which holds that scenes of personal disorder reflect kinds of disorder or injustice on a wider social and political scale, the Letter of Support is a useful tool by which transcultural allies can 'tip the balance of forces' in favour of their network or position. These letters emerge out of Dinh's fourth stage of the CCS conference: the interdisciplinary deliberation, in which questions of diagnosis and treatment, refracted through a "contextual and relational focus," are no longer constrained by the rigid and individualizing categories of conventional nosology (Dinh et al. 270). Instead, diagnosis is taken up as a strategic vector of intervention that is at once technically authoritative and socially persuasive.

Finally, the material infrastructures of the CCS merit a quick and dirty review. First of all, like my other field sites, the CCS is impossible to imagine apart from the virtual mediators — teleconferencing and filesharing services — that stand between its participants (cf. I.1). The CCS also exercises medical authority over a host of nonhuman delegates, such as pills (I.1)[56] or service dogs (I.3). We also consider the material and economic conditions of a given case as important sources of stress, and thus as possible sites of intervention: one example of this is

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[56] Eric has an idiosyncratic way of talking about the utility of pharmaceutical agents. Although he certainly speaks of their efficacy in high-type statements, his relationship to their utility is often qualified. In some cases, he argues that pills are "not appropriate" (II.18); at other times, he speaks of their agency in a casual tone that belies a sensitivity to the reductive accounting practices of pharmacology. "It's not particularly a molecule I like

Eric's high-type claim that "nothing is more discouraging than a pest-filled environment" (II.43). It is not only the judges, then, over which our network of allies wants to gain some leverage; the agency of Parc-Extension bedbugs must also be understood and accounted for. We find ourselves caught up with strange bedfellows, indeed, in this clinical work of ecosocial reorganization.

Part 3. Social Mapping

In my social mapping of the CCS, I will describe the three technical activities defined in a preliminary manner at the beginning of this section. First, I will discuss *symmetrical interpretation*, as a practice of listening which gives shape to the social alliances in and around the case conference. Then, I will consider how the trajectories of actors both inside and outside the clinic are altered by the activity of *hybrid networking*. Finally, I will treat *strategic naming*, in relation to the various social and political interests at work in the consultation process. By following this three-part movement of clinical action, the sense in which ecosocial factuality might be said to *externalize* or *reinscribe* itself into society will continue to become clearer.

The first two stages of a CCS case conference — what Dinh describes as the presentation of a patient's psychiatric history and cultural formulation — correspond most closely to the activity of *symmetrical interpretation*. In this process, sources of biomedical and biographical information are gathered and assimilated by the consulting team (cf. I.1, I.93). While biomedical facts are sometimes presented in higher-type statements than sociocultural ones (cf. I.5), both kinds of information contribute in important ways to our *internal representations* of the case — that is to say, the *culturally-situated story* that we collectively build of a particular psychiatric problem. Increasing the variety of credible explanatory factors allows for more allies to become

interested in a given case[57] — this is because, as I've argued above, different representations lead to different interventions. Describing a psychiatric problem in pharmaceutical terms opens the CCS network to medical doctors (I.6); the same problem framed in spiritual terms invites local religious groups into the scene (I.70); describing it in terms of gender or sexuality might connect us to allies like *Clinique Mauve* (I.118); identifying the labour conditions at play connects us to Quebec's *Commission des normes, de l'équité, de la santé et de la sécurité du travail* (CNESST) (I.4). The specific semiotic material of this pluralistic story thus defines the basic dimensions and extent of the relational network that links together the diverse organizations involved at the CCS.

As the case conference transitions to its third stage — and shifts from the work of representation to that of intervention — these social alliances are brought into play. It is possible to distinguish four broad categories of allies: the referring team, the consultation team, the CCS' partner organizations, and the patients' other social supports (family, community, legal, *et cetera*). The referring teams I observed during my ethnography of the CCS had fairly diverse compositions: they might involve family doctors (cf. I.100); specialists in migrant health (cf. I.1); counsellors (cf. I.115); social workers (cf. I.92); or occupational therapists (II.44), for just some examples. The consultation teams consisted most commonly of Eric, a peer-support worker from the ICFP, and a medical anthropologist from *Doctors Without Borders*; we were regularly joined by students of social science (cf. I.1) and other physicians (cf. II.38). The most complex alliances involved in the conference were those with partner organizations in the fields of social

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[57] The most commonly utilized of these allied organizations were Montreal's CDAR, Quebec's *Programme régional d'accueil et d'intégration des demandeurs d'asile* (PRAIDA), the *RIVO-Résilience* network for victims of organized violence, and the *Soutien d'intensité variable* (SIV) program offered by Montreal's *Centres locals de services communautaires* (CLSCs). *Clinique Mauve* was an invaluable resource when gender or sexuality was an element of the psychiatric problem or social predicament in question.

services and mental health. Conference participants used their knowledge of different eligibility criteria and availability levels to make decisions about what referral might best benefit the patient in question.[58] The CCS enlists these social organizations to help mobilize other actors around the patient, so as to solidify and extend their network of support.

This is the activity of *hybrid networking*: as the conference moves away from its discussion of the psychiatric documents and the formulation interview — away, that is, from the scenes of inscription at which the patient was initially enrolled in the clinical process — it moves outwards into society, leveraging the semiotic material of its symmetrical stories in order to redistribute resources around a scene of disorder. Eric describes this process as using a "factual, medical position" to "activate" social and material structures of "resilience" (I.8). This is the critical gesture at the center of transcultural psychiatry's movement from theory into practice: by using these stories and situations to identify and activate "missing social resources" (I.102), the disordered body in question becomes a kind of *critical ecosocial instrument* in the diagnosis and treatment *of their own environment*. An instrument in both senses of the word, importantly: when joined to the clinical network of the CCS, these sensitive bodies function as *inscription devices*, producing information about the 'disorder' of their environments; the critical interpretation that results from this clinical encounter enables them to become *levers of sociomaterial justice*, moving allied organizations into their environments in the fulfillment of a clinical imperative.

An important aspect of the information constructed at the CCS is thus its affective gravity: part of what draws the interested allies to a given case, in other words, is their

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[58] In one case, for example, this stage of interdisciplinary assessment saw the conference team strategically prioritize a diagnosis of depression over one of adjustment disorder, in light of the fact that the JGH's *Mood and Anxiety Clinic* only accepts referrals for patients with certain diagnostic statuses (II.47).

investment in what Fassin would call the 'moral economy' of the psychiatric problem in question. This moral dimension structures the CCS' *external representations* of a given case — most notably in the Letters of Support, which the conference often discusses in its fourth stage, interdisciplinary deliberation. They are a key mechanism by which the CCS exercises its *strategic naming*: in one letter concerning a family claiming asylum from their country of origin in South Asia, Jarvis diagnosed the mother with PTSD (I.70);[59] the letter went on to identify the son as Queer, and, after describing the sexual politics of the country of origin, predicted the rejection of their claim as posing a "plausible [. . .] risk to life and limb" (I.70; I.77). In another letter to the court, Jarvis described the insufficient "state of mental health care" in the same country, thus suggesting the immorality of deporting refugees who suffer from disorders such as "treatment-resistant PTSD" (II.18; I.70). Linking socially-authoritative medical concepts like continuity of care to a contextually-informed moral economy of illness, the CCS strategically deploys diagnostic names in an effort to keep migrants attached to its networks.

With this account of clinical action, we have arrived at another scene of *social-semiotic reciprocity.* By participating in the clinical encounter, for example, patients might receive a new kind of *solidity* — the formulation interview gives them a chance to articulate their problems and make sense of new solutions (cf. I.94), and orderly diagnostic names might grant them access to longer and stronger networks of care. Latour's constructivist principle, then, applies to clinical diagnoses in much the the same way as it did to scientific facts: their 'reality' is not a consequence of their correctness according to nature, so much as it is a result of *how they are*

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[59] Eric described the strategic utility of PTSD in these terms: "If I can kind of shift the mirrors a little bit, and PTSD appears there, maybe I'll focus on that particular lens for a letter, because I can see that's going to be very helpful" (III.5). This exemplifies the *dialectical, use-oriented* side of namegiving, and the role of *social currency* within it.

taken up by later users — such as patients, judges, politicians, and administrators. The CCS, in turn, enjoys its own solidification with each encounter — it continues to build out its network of allies, and thereby strengthens the arguments it can make in favour of its own credibility.[60]

In this sense, it is also possible to understand the CCS' sociotechnical activities — interpreting, networking, and naming — as part of an effort to bring its participants closer to the condition of scientific information: diagnostic interpretation enhances their social and semiotic *stability*, by giving them names that are more legible to more users*;* successful Letters of Support, by intervening in the moral status of their 'disordered' habits and stories, significantly effect their legal and political *mobility* in Canada; and the tactics of hybrid networking, by altering their trajectory through clinical and paraclinical sites of care, determine their technical *combinability* with other allies, resources, and delegates of the CCS network. For this essay's final chapter, my aim will be to interpret this feature of symmetrical science in action, in which the initial laboratory conversion of *somatic material into scientific information* seems to result in a later clinical transformation: that of *human bodies into credible vectors of scientific authority*. By thinking through these two related ways in which transcultural psychiatrists — in a reciprocal collaboration with their patients and participants — come to treat the body as an instrument, we might finally arrive at an speculative understanding of ecosocially-responsible medicine.

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[60] Articles like Dinh's for example, function to convert these encounters into literary credit. In my interview with Eric, it should be noted, he acknowledged that the CCS "should produce more reports on outcome" (III.4). The CCS does host, every year, a conference of the *International Consortium for Cultural Consultation* (II.49-50), which functions much like the literary conferences I described in my ethnography of the DSTP and CESAR.

Chapter 7. Interpretations

In *Representing and Intervening* (1983), the philosopher of science Ian Hacking introduces the old question in science studies — familiar in shape to any reader of Plato's *Cratylus* — concerning the nature of scientific names. He asks whether, or to what extent, the entities described in different scientific theories are actually held by scientists to be *real*, or whether they are simply *useful* explanatory fictions. Following his summary of these long and sundry debates about representational reality, Hacking argues that science studies needs to complement its accounts of epistemology with a new focus; namely, on how reality is also constituted by scenes and spaces of practical intervention. In other words, we know something is real when we learn how to work with it, make use of it, or build a reliable program of action around it (*RI* 131). An important thread in Hacking's later writing revisits this relationship between representation and intervention: this is his concept of the *looping effect*, which describes the ways in which certain practices of representation can themselves intervene in social reality.

In "Making Up People" (2006), he writes that human sciences — especially medicine — "create kinds of people that [. . .] did not exist before," through the articulation of diagnostic entities (1). Hacking goes on to write of the "niches" that these representational entities afford, especially to those who lack a sense of agency (3) — it is this ecology of the looping effect to which both Birk and Fassin refer in their social studies of the clinic (cf. Birk 202; *SP* 110). For Hacking, meanwhile, the importance of this theory is that it gives us a way to settle the question of diagnostic nature: the names of the clinic *become real* to the extent that they *enable useful* interventions in society. People in distress use these names to orient themselves; as more people subscribe to their way of making sense, the diagnostic entities assume more reality.

In this final chapter, I will fit Hacking's heuristic of representation and intervention into my conceptual frame, as a kind of interpretive lens. His theory of the diagnostic niche is clearly consistent with the symmetrical account of clinical medicine as I have developed it in this essay; his theory of the looping effect, for its part, offers us a new way to think through the cyclical structure of scientific literacy in the context of the human sciences. Latour and Fassin gave us the tools to *contextualize* transcultural psychiatry in action; Hacking will provide the resources for us to *analyze* this activity in terms of the *kinds of niches it works to construct*. This language of the niche helps to build out Latour's account of the body as a mediator: whereas Latour writes that "to have a body is to learn to be affected [. . .] by other entities, humans or non-humans" (*TB* 205), Hacking would remind us that we are always already *invited* by medical science to learn our bodies in specific ways. By thinking through the activities of the scientists at my field sites as part of this looping effect, we will be able to understand their treatment of the body as an effort to *present new options* for kinds of people who exist outside of a well-defined niche. It is through this analysis that we might interpret the quality of transcultural psychiatry in Montreal.

As I transition to a discussion of my findings at these field sites, let's keep this set of relationships in mind: an information economy of science — structured by an ongoing credit cycle, and consisting of a series of reciprocal exchanges — which leads to the transformation of physical bodies into social instruments, marked by their different kinds of stability, mobility, and combinability. These sociomaterial differences, always semiotically-laden and morally-charged, are both the consequence of past therapeutic alliances and the cause of future psychiatric action. The diagnostic names that define and determine the trajectories of these niches emerge as the result of an ongoing collaboration between the different participants in the scientific encounter.

Part 1. The Human Kinds of Transcultural Psychiatry

Let's begin by fleshing out Hacking's account of the body as an object of science. In his essay, he lists ten "engines of making up people" (5). The first seven relate to the *scientific construction* of a given kind; the final three are features of its *social reproduction*. In the first three engines — *count*, *quantify*, and *create norms* — we can see the influence of contemporary public health's statistical literacy (5-6). These engines determine which bodies count, how the material of those bodies will be translated into information, and to what extent they are to be considered as disordered. The next two engines — *correlate* and *medicalize* — demonstrate the vectors of power which Fassin identified in the space of public health (6). In the process of correlation, the bodies of a given kind are made to *resist dissociation* from each other, such that they are *differentiated* from other kinds of bodies; in medicalization, scientists determine how the disorders in question are to be *managed* by science, and thus how the bodies in question are to be *treated* by doctors. The latter engines — *biologization*, *normalization*, *bureaucratization*, and *reclaiming* — describe the outward movement of public health from the clinic into society (7). First, the new niche is naturalized, as the cause of its pathology is shifted away from moral or cultural origins, and ascribed to more neutral nonhuman agents; next, a social and political network organizes itself around the niche, connecting it to other institutional resources; finally, members of the niche organize themselves under the sign of their new name — making claims on their own behalf, they resist or otherwise participate in their ongoing scientific construction.

All of Hacking's engines are in play across my three field sites. At the CESAR, these looping effects determine the construction of the Queer person as a medically-relevant 'kind' of

body, and of stress as a medically-relevant 'system' of the body; at the CCS, normalization and bureaucratization help to explain the collective settlement of diagnoses, and the proposition of a treatment program fit to the conditions of migrant life; and at the DSTP, educators are well aware of this phenomenon: highly reflexive about diagnostic realism, they ultimately defer to "pragmatic decisions about where clinical leverage can be found" in a gesture that is similar to Hacking's elevation of technical practice over abstract theory (Kirmayer & Crafa 10). As a field which ratifies its own cultural situatedness in this way, transcultural psychiatry is especially open to the role of patients in articulating their own niches, norms, and needs.

What has already become clear in our study of the laboratory and clinic are the many ways in which these scientific encounters are never entirely successful in their efforts at producing order, constructing sense, or articulating reality. As Birk argues in his interpretation of Latour, the normative dimensions of symmetrical diagnosis "do not determine the subject, but prescribe certain ways of being — ways of being that might be resisted" (198). Or, in Latour's own words, there "might be an enormous gap between the prescribed user and the user-in-the-flesh" (*MM* 161). The psychiatric subject thus starts as a semiotic character as much as the reader of an article or the user of a manual, and, for Latour, "nothing in a given scene can prevent the inscribed [character] from behaving differently from what was expected" (161). In the clinical and laboratory scenes of an ecosocial medicine, the literary entities of biological theory and psychiatric diagnosis must encounter the sociomaterial environments and situated embodiments that they are meant to capture or represent. For the coming parts of this chapter, we will consider how the scientists of transcultural psychiatry in Montreal continue to make sense of bodies, niches, and human kinds that are always surprising in their resistance to order.

Part 2. Bodies That Might Matter

In the last chapter, I described the technical activity at the DSTP as one of *instruction*, which makes use of identifiable social networks and technical devices in order to reproduce and solidify scientific facts. I also defined its seminars as spaces of *constructive proposition*, in which differently-positioned participants could articulate their own objects of research in the locally-idiomatic terms of the transcultural literature. I identified one of the most notable features of the ecosocial literature as its commitment to explanatory pluralism; this pluralistic approach makes these seminar spaces quite welcoming to proponents of any alternative psychiatric fact: to solidify their own object of study's relevance to the division's research program, they only have to fold it into one of the "different kinds of processes [. . .] in which brain and culture are [there said to be] coconstructing each other" (I.16).This activity of collective translation was a clear priority of the CMB workshop; Laurence was consistently willing to integrate or converse with different explanatory models from the ecosocial perspective (cf. I.11). However, he often shifted statements to a higher type *after* reframing them in ecosocial literary idioms (cf. I.13, I.52). Rearticulating the propositions of workshop participants in the language of interacting feedback loops (cf. I.38, II.57), Laurence leveraged his positional authority in the classroom, thereby instructing participants on how to make better sense of their own objects of research. The result: an extended domain of *objectivity* for the ecosocial representational model or ontology.

By instructing its participants in how to use ecosocial objectivity as a method of interpretation, this kind of workshop gives shape to a new collective *literacy*. The process of instruction extends the stability and mobility of ecosocial fact in two ways: first, it produces a

newly-trained readership for the distinct literature of the division, lengthening the reach and strengthening the impact of disciplinary journals such as *Transcultural Psychiatry*; second, it enables workshop participants to refer back to the ecosocial model when interpreting facts from other scientific disciplines. Both enhance the positive modality of ecosocial naming practices, as the citation of literature from within the discipline becomes increasingly likely in new contexts (cf. I.24). This being the case, it should be no surprise that one of Kirmayer's main activities at the CMB workshop involved introducing us to transcultural literature — the seminar's syllabus consisted of no less than seventy-eight readings, and he consistently made reference to further texts in his responses to questions and other presentations. Taken together, this careful attention to textual interpretation prepared participants to 'speak for' the field, make references to its foundational literature, and positively modalize any future articles in support of it.

By contributing to the initial objectivity and extended legibility of ecosocial fact, the classrooms of transcultural psychiatry also give shape to the field's power to construct niches. Latour argues that the aim of science is to move outwards into society: this third contribution thus represents a mature stage of the field's credit cycle, in which its facts begin to inform the behaviours and norms of its allies. One primary mechanism of this outward movement is related to the fact that, according to Kirmayer, psychiatry consists of "giving an object to people" — an explanatory model by which they might organize and interpret their distress (I.22). To the extent that, in any eventual clinical encounter, physicians serve as educators of their patients, the *instructive* activity of the CMB workshop will reproduce itself in the outward trajectory of its participants, as they return to their own clinical practices (I.65). By strengthening the literary credit and technical currency of ecosocial fact, the DSTP strengthens the diagnostic niches which

it eventually hopes to open in transcultural laboratories and clinics. As educational spaces of transcultural psychiatry contribute to an interdisciplinary incorporation of ecosocial techniques and technologies, the result is a *medicalization* and *normalization* of new human kinds.

Before moving on to the laboratory and the clinic themselves — those sites at which the body and its substances are directly transformed into scientific information — it will be useful to remain a while longer in these classrooms, and to consider just how it is that their symmetrical accounts of stress and distress alter the status of the body in science. In this closing movement, I will argue that the instruction of ecosocial philosophy does not simply afford its practitioners a new sensitivity to embodied predicaments: in so doing, it establishes the body as the potential site of a new kind of scientific authority. By proposing an account of the body as 'coconstructed' by culture, the symmetrical literature of transcultural psychiatry sets out a conceptual framework in which the ostensibly stable physical structures of life sciences are reinscribed as local "emergent properties" (I.42). It is with this reinscription that a new medical authority over the body takes shape: because of the multifactorial complexity of ecosocial accounting, transcultural psychiatry necessarily involves "tolerating uncertainty" (II.62). The delegations and disorders of a given body might not fully 'make sense', because they are contingent on a kind of cultural variability that cannot fully be captured by conventional instruments of science (I.39).

The conventional scientific endeavour — constructing sense by producing order — takes on a new dimension in this practice: the information required to assemble credible facts must come not only from the *nonhuman substance* of the body, but from its *human testimony* as well. By treating this ethnographic dimension of fact-construction as "a path to competence [and] a path to equity" (III.2), instructors at the division make a critical intervention in the moral and

semiotic economies of mental disorder, disrupting the conventional normativity of psychiatric diagnosis (I.44). In other words, by teaching its participants to pursue cultural competence as a path to symmetrical expertise, events like the CMB workshop propose to reorganize the structure of scientific authority in a fundamental way, calling for the ratification and inclusion of the tenth engine of articulating human kinds. According to the transcultural approach, psychiatric 'order' cannot just come from above, because this 'order' is what continuously emerges at every point of contact between unique bodies and the cultural imperatives which give them shape. When it comes to situated human bodies, order is thus something that we only ever determine after the fact — to paraphrase Latour, its qualities are a consequence, not a cause, of collective action. In a psychiatric practice that values cultural sensitivity, authority does not finally appear at the end of the clinical interview, as the result of some settlement of controversies — rather, authority might emerge *in the process of having the conversation*, as participants in the clinical or laboratory encounter find new ways to make sense of themselves and their environments.

There is one important feature of this new scientific encounter to emphasize at this point. So far in this thesis, I've argued that the symmetrical science of transcultural psychiatry leads to a twofold *instrumentalization* of the patient's or participant's body: first, as a *sensitive archive* of environmental stress; and second, as a *critical lever* of environmental reorganization. I've also made the argument that, in ecosocial medicine, this point of clinical intervention is neither merely social nor merely somatic — such hybrid approaches, in which technoscientific devices are deployed alongside psychosocial insights, represent an alignment with the Latourian principle that "there is no sense in defining the body directly" (*TB* 206). Ecosocially-situated bodies are instead defined by their relations with elements spanning the cultural and the material;

we might say that, in order to help build out better niches for their patients and participants, transcultural scientists seek to become "sensitive to what these other elements are" (206). It is thus possible to make the further argument that, in the encounters of a symmetrical medicine, the *body of the scientist* must also function as a kind of double-instrument. In other words, in order to activate the semiotic leverage of their counterparts, the researcher or clinician must first learn from them how to interpret and critique their sociomaterial situations. It is in this encounter, and its reciprocal or mutual instrumentalization, that symmetrical information derives its authority.

The CESAR laboratory, then, is one space in which transcultural scientists are at work acquiring this new kind of sensitivity: the STARS project, for example, enacts the symmetrical gesture by which research subjects are invited to contribute their perspective to the construction of their own scientific names. Through ongoing consultations with participants, the STARS team continually adjusted the kinds of information behind the black boxes with which they proposed to capture Queer stress: in one case, over a series of meetings towards the end of the project's pilot phase, STARS experimenters debated the addition of a new sociological inventory to their laboratory apparatus (I.126); the basis of this discussion was the need to capture more information regarding the *experiential* or *narrative* dimensions of their participants' sexuality (I.67). From our engagements with the Queer community, we knew that the conventional or surface-level identity-categories of sexual orientation were no longer adequate to describe the differences between specific 'kinds' of Queerness; a more richly-textured testimony about how sexual desire and performance contributed to each participant's resilience against stress was therefore necessary (II.1). In this instance, the process of *reclaiming* is imported directly into that

of *quantification* and *normalization*: because, as one researcher put it, "sexuality [seems to be] such an important part of Queer socialization," our instruments for measuring Queer stress could not leave out that kind of narrative information (II.2). In other words, we found that the Queer body — and its ecosocial niche — consists, in part, of a sexuality that often troubles or resists conventional categories of gender and identity; good engines for making up Queerness as a human kind thus need to find some way to account for that new social economy of desire.

Importantly, the introduction of this new information required the modification of the STARS' statistical objectivity: any such kind of intersectional analysis requires the integration of multi-variable techniques of analysis and interpretation (II.54). As Rob put it in one of the CESAR's lab-wide reading groups, journals still expect "clean curves," even as the information at the heart of a given research project become more complex (I.91). Thanks to the disciplinary pressures of the literature, the goal of the STARS project remains — for all its anthropological sensitivities — the construction of a *statistical character* capable of representing Queer stress in Montreal. The STARS authors argue that this kind of figure, who speaks in the language of the ALI, can best describe the degree to which the Queer community is "stressée à mort" (I.127). For Rob, "the cortisol will tell us" what we need to know about the stress and resilience of our participants (I.114); the STARS team counts on their statistical construction of Queer stress, in other words, as the most persuasive voice in which to speak to and enrol the semiotic character of their eventual readers (II.22). In our symmetrical approach to the literary process of science,

an understanding of the ecological niche of Queerness emerges through a translation between the cultural narratives of our participants and the statistical testimony of their hormones.[61]

In this literary production of a statistical voice for Queer stress, we can see how the niche-construction of the STARS project is still informed by elements of contemporary public health. First, the scientists assemble diverse forms of information, so as to give name to a crisis facing the community — one which is described as a differential exposure to the risk of death, and ascribed to the material effects of social stigma (I.127). The pursuit of resilience factors in this study intends to generate counter-leverage over this stress, and also represents an effort to demonstrate the utility of the ALI in designing medical interventions (I.129). The now-articulate material of the body becomes a semiotic resource in our effort to identify and control stress (I.99); through such biologization, no trace of stress is ever lost — as long as the blackboxed substances can be found in the body, their testimony can be included in the account (II.37; II.54). Future scientists of allostatic load might then take up our results, as they continue to work out what kinds of sociomaterial environments are required to support resilient Queer communities.

It is in this eventual work of medically-informed social engineering that the semiotic transformation of somatic material reaches its conclusion — what Hacking would call the engine of *normalization*. Queer bodies, quantified and correlated through the instrumental nexus of the ALI, are now endowed with the capacity to identify the stress factors of their environment;

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[61] This translation is not without its troubles. While the STARS program includes a prominent qualitative branch, the nature of statistical science is to move away from groundedness in the particular, towards a kind of profile of the average within a certain category: a biographical fact about one bisexual man might be anthropologically interesting; a biological fact about bisexual men in general is scientifically important. This is a tension in the information ecology of the sociocultural approaches to medicine that, in my opinion, is yet to be fully resolved. For the time being, the body is the instrumental nexus which links nature to culture: under this peculiar scientific gaze, the activity of hormonal mediators becomes the sensitive register on which something as evanescent and immaterial as sexual fantasy is said to inscribe itself. Always refracted through the prism of social experience, this statistical analysis facilitates a *new differentiation* of Queer bodies, and thus a *rematerialization* of their norms and margins.

allostatic researchers, with their ability to read and interpret the information of the body, endow themselves with the leverage to critique and intervene in these environments. This is the special work of scientific laboratories: through the positioning tactics and engines of public health, even a substance as conspicuously neutral as saliva (cf. Douglas, 1966; Latour, 1988) can become a vibrant and authoritative source of meaning. Projects like the STARS, with their symmetrical sensitivity, draw on this powerful statistical testimony of the body, without — as Latour argues is typically the case — leaving the original scene of the scientific encounter behind. Instead, by accounting for and learning from the ethnographic testimony of their research participants, they infuse this somatic material with a locally-and-culturally-variable flavour.[62] Blending public health and critical inquiry, such technoscientists endeavour to convert the hybrid body of their research participants into an Archimedean lever: an instrument strong enough to move the world, precisely because of its groundedness at a highly specific point of intervention.

That activity of the laboratory — which in the last chapter I defined, in Latourian terms, as *productive articulation* — appears here through Hacking's lens as a style of namegiving that, by *solidifying new kinds of people*, intervenes in the very substance it is meant to represent. What I mean to indicate by this is the way in which human flesh and society are both *rematerialized* by this process of niche-construction: from an anthropological point of view, it appears that spit wields an incredible power, manifest in its ability to arrange all of these various actors into highly-disciplined networks of protocol and practice. This is the trust we hold in the promise of

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[62] There is, of course, something paradoxical about the 'normalization' of a human kind that refers to and defines itself as 'Queer'. As a Latourian interpreter of science, I might hazard the prediction that this semiotic tension will eventually become a point of reclamation, at which participants in the CESAR project would resist or reject efforts to name them scientifically. Researchers like Juster, for their part, seek to resolve this tension through their involvement and inclusion of community members as "reliable sources of knowledge" (Edmiston & Juster 1254).

scientific knowledge — the lengths to which we are willing to go to separate information from noise, and to move to order from disorder. Under this lens, the CESAR lab itself comes to appear as a strange kind of instrument, one capable of changing social sensibilities by making spit newly sensable and legible. What makes the STARS project and others like it so bold (or even radical) is their willingness to qualify — in a double sense of the word — the quantitative information that has for so long served as the end goal and initial justification of these kinds of networks.

The STARS protocol qualifies its biometric data through the operational premise that biology is not enough on its own. According to those at the CESAR, biology needs to be made qualitative: biographical information and cultural context, which cannot be stabilized in the same way as physical values can, are necessary for a full interpretation of the facts. This premise disrupts the most impressive feature of information, which is its ability to leave behind the social histories that contributed to its production. A cortisol concentration of 15 mcg/dL is evidently just that; you can take that figure anywhere. Queer cortisol, however, will be wrapped up in intractable webs of life experiences, and anchored firmly to the cultural contexts and senses in which those experiences are possible and intelligible. It is the argument of symmetrical that nature and culture — or quantitative material histories and qualitative social histories — must be thought together, and then brought together in the construction of responsible facts. Through this participant-oriented commitment to responsibility, I argue that the science in action at labs like the CESAR might bring a new kind of quality to the powerful information economy of health.

A similar commitment is at place at the CCS — this is why, in the previous chapter, I named its technical activity as *participatory reorganizing*. In the case studies I presented there, it

was possible to see the ways in which the moral and social discourses of health enabled clinical interventions into the political economies of resource-distribution around the body. In such activities, the CCS inverts conventional moralities: in the case of court-ordered pharmaceutical treatment, for example, pathology is juridically ascribed to the chemical agency of the brain — a form of disorder that is then used to explain that person's 'immoral' habits and behaviours (I.102). A critical symmetrical medicine rereads this scene: the 'illness' here is not in the person (or their body), but rather in the social response to their cultural difference (I.92). This sensitivity to the specific dimensions of "Black health" demonstrates the extent to which transcultural psychiatry does not operate around some universal concept of the healthy body (I.101). The utility of symmetrical storytelling to this kind of environmental medicine lies in its capacity to disclose the forms of counter-power or resilience that already exist in these marginalized positions; by *denaturalizing* the sociomaterial structures that produce differential precarity, such storytelling authorizes a further *rematerialization* of the resilient niche.

The transcultural psychiatrist, in other words, works to become an *ally of the resistance* that is *already taking place* at scenes of psychiatric disorder. On the one hand, they contest the conventional diagnostic practices that name some forms of coping as orderly while relegating others to the margins; on the other hand, they contribute to the social legibility of the claims to legitimacy and consistency that structure culturally-situated forms of resilience. Wielding the semiotic authority of technoscience, they attempt to brush biomedical literacy against the grain, rendering the making-up of people more amenable to *reclamation*. Ultimately, the goal is to alter the relevant information economy of illness such that these counter-processes of representation and intervention have a fighting chance in the competition for environmental stability and

material resources. If Latour and Hacking are correct, then once these strong counter-networks are in place, it won't take long for their names to become real.

For the time being, however, the *bureaucratization* of transcultural psychiatry's human kinds is far from complete. One clear example of this concerns the conditions required to solicit symmetrical information in the first place: our ethnographic interviews and interdisciplinary consultations — which are, after all, required to translate between biography and biomedicine — demand up to five hours of clinical attention for the formulation of a single case (III.4). According to Eric, scaling up this practice would far exceed the conventional or existing capacity of Montreal's health care network (III.4). This is why the mutual instrumentalization that takes place in symmetrical psychiatry is so critical: by demonstrating this structural inadequacy, participants in the CCS interviews turn the diagnostic encounter against itself, bringing clinical attention to the disorders and immoralities of the sociopolitical systems in which it is situated (II.7). In other words, participatory namegiving has the capacity to give rise to a critical sensitivity to our broken moral economies of distress; this being the case, it is possible to interpret the clinical encounter as involving yet another form of reciprocal exchange: just as we propose new names and instruct new techniques for resolving the distress of our participants, they afford us the opportunity to demand better environmental conditions for own work.

Part 3. The Middle Voice of Transcultural Psychiatry

At the beginning of my presentation of this essay's conceptual framework, I promised to think through the positive dimensions of scientific authority. In what remains of this last chapter, I will finally arrive at some speculative propositions for a critical analysis of ecosocial medicine,

and the kind of scientific responsibility it works to construct. As I argued in my conceptual framework, Latour's later writings betray a high degree of ethical sensitivity, and he suggests a number of criteria for evaluating the ethical 'quality' of a given style or piece of scientific work. Fassin's work, grounded as it is in a tradition of critical anthropology, is inherently ethically-charged, and orients itself towards the 'problematization' of public health as a scientific field and governmental effort. By reading them together, I will be able to identify a number of ethical qualities and quandaries in this psychiatric practice of representation and intervention.

Some of Latour's most direct writing on evaluating the quality of a given form of inquiry comes in the 2004 article, "How to Talk About the Body?" In it, he identifies the need for a "an alternative normative political epistemology" for the sciences (214); to this end, he argues for the prioritization of *propositions of fact* over *statements of fact* (206). The scientific proposition is primarily concerned with increasing degrees of articulation; it exemplifies a more ethical science because it allows for more kinds of accounting and explanation to coexist. Propositions multiply, statements delimit. Borrowing from the prior work of Isabelle Stengers and Vinciane Despret, Latour proposes eight qualities for such a science: (1) *knowing is rare* and cannot be the outcome of a general method; (2) its propositions have to be *interesting*; (3) its laboratories must take *risks*; (4) its inquiries must invite *resistance*; (5) its hybrids must have the chance to *ask their own questions*; (6) it must embody a willingness to adopt or adapt to *new repertoires of action*; (7); it must not eliminate *alternative forms of articulation*; (8) its scientists must take *versions of reality* from outside the laboratory very seriously. Transcultural psychiatry in Montreal — with its principle of cultural humility, its promotion of narrative agency, and its openness to alternative positionalities — clearly satisfies a number of these Latourian criteria.

Elsewhere in "How to Talk About the Body?" Latour observes the surprising responses of cultural subjects and natural objects to the laboratory. He argues that "humans have a great tendency, when faced with scientific authority, [. . .] to behave like obedient objects" or to "transmit objectivation obediently" — things, on the other hand, "obstinately object to being studied" (217). The scientific laboratory, in our cultural context, enjoys an existing credibility — an inherited semiotic authority that extends well beyond concrete networks of scientific practice, and into wider society. In other words, when asked questions by scientists, people usually answer *the way they think they are supposed to*; things usually do not.[63] Instruments like the CFI as it is used at the CCS, or the sociological inventories of the ALI as they are deployed by the CESAR, take the risk of inviting alternative propositions of fact into the clinic and lab. By opening this kind of space for their participants, they make it easier for these communities of study to *resist* the ways in which they have been objectified, or rendered abject, by the looping effects of the old and broken information economies of technoscience or public health. Given that reality, or so Latour argues, is whatever resists, then the creation of this space is no minor feat.

Fassin writes of the political space of public health as "un éspace défini par les rapports entre le corps physique et le corps social" (*EP* 35). To the extent that our different environmental positions afford us different kinds of agency and sensitivity in these power relations, symmetrical sciences do "not expect accounts [of the body] to converge into one single version that will close the discussion" (*TB* 211). By ratifying the material reality and medical salience of both cultural diversity and political economy, they satisfy Latour's call for "a different normative definition of what it is to speak scientifically about the body" (206). As an ecosocial medicine — one with the

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[63] As Rob put it during one of the lab reading groups: "Cortisol will never do exactly what you expect it to" (I.91).

environment-oriented features of contemporary public health — transcultural psychiatry affords its practitioners authority not just over the body and its "psychological 'interiors,'" but also over the "socio-material arrangements of the world" that structure them (Birk 204). The diagnostic niches which it works to construct are thus "entangled with subjectivity," while also being "tied up with materiality" (194). These materially-structured forms of subjectivity remain "an ongoing accomplishment by many different actors" (198) — ultimately, their stability or collapse is only the result of how the looping effects and credit cycles of medicine play out in society.

For one specific example of ethical intervention into a credit cycle, consider the goal of the STARS project: the active sampling and testing of a 'statistically meaningful' population of the Montreal queer community, in order to create the semiotic character of an 'average hormonal profile' (I.105). The STARS project argues that such a statistical figure can speak to the conditions of an (already-organized) political group, on their behalf, and in the powerful ethical language of risks to their health. At the same time, by embracing and learning from the political claims of its community of study, the CESAR takes the risk of adopting a new repertoire of scientific activism. In so doing, the lab positions its synthetic Queer body to be returned to and enlisted by the community, as an important semiotic ally in their ongoing struggle for stronger civic rights and better medical care. For Latour, such alliances between scientific research and political organization represent a mutual translation of interests — in other words, the politicization and 'scientification' of the body are only inverse results of the same kind of effort: to become more interesting to more people. After all, even the powerful semiotic character of the statistically-objectified body is powerless if it cannot capture and mobilize the interest of the

reader. If the reader — whether they're another scientist, a doctor, or a concerned legislator — doesn't care, then they're not an ally; and if no readers care, then the fact isn't going anywhere.

Transcultural psychiatry in Montreal — in both its laboratory and clinical practices — takes as its point of departure the proposal that interest, or care, is best built around solidarity with its communities of study. This commitment involves certain sacrifices with respect to the pressures and rewards of the disciplinary credit cycle: the symmetrical methods and hybrid objects of ecosocial science, after all, do not yet enjoy the same literary currency or social authority of their more conventional counterparts. Nonetheless, through its ongoing rearticulation of stress and resilience, this transcultural medicine continues to construct the foundations of a new and radical form of scientific namegiving. In its emerging language of the situated body, communities on the margins of common sensitivities find a new way to represent their social and material problems. With this middle voice, symmetrical science promises a new kind of agency to those who struggle to make sense of themselves amidst all the noise and disorder of life.

In Latour's words, under the gaze of a symmetrical medicine, the human body does not appear as a "robot animated by neurons, impulses, [and] selfish genes" (*NM* 136); rather, it looks like "the puzzle of the folded body" — a puzzle that is at once sociological and technological — containing "so much diversity, so many microbes, so many organs, all folded in such a way" that the many agencies and delegates seem to act as one (*TB* 227). Indeed, Latour argues that human subjectivity is "at the intersection between *mastery* (of the object) and *being mastered* (by the object)" (Birk 193). If this is right, then any middle-voiced concept of pathology needs to involve the act of delegation as the scene at which we experience disorder; middle-voiced therapeutic care, too, follows from this premise, that we reorganize our habits of delegation

around the prescriptions inscribed in a given psychiatric diagnosis. At stake in diagnostic naming, when viewed symmetrically, is thus not just the definition of specific subjective conditions, but the production and construction of objective sociomaterial life trajectories.

My interest with this ethnographic paper was to answer a simple enough question about science in action: namely, how does physical material become scientific information? This is, after all, the fundamental process of all laboratory and clinical projects: some kind of material or substance is brought in, it undergoes a process of technical manipulation, and then the results of this encounter are subject to analysis and interpretation. From this interpretive stage come the scientific facts and theories we use to predict, explain, and control natural events. The story, then, seems fairly straightforward. In practice, of course, things are rather more complicated. The material never quite acts in the ways we want it to. The results usually involve a lot of massaging, as outliers are excluded for any number of reasons. Some names are made to matter more than others. Some disappear entirely. We never quite understand how or even whether the instruments themselves are working properly. Nonetheless, scientific action continues to authorize itself, producing results and enabling mechanical feats with remarkable precision, efficiency, and reliability. In this thesis, by applying a critical framework to the subfield of transcultural psychiatry in Montreal, I have attempted to articulate some of the deeper and more complicated story behind this continual transformation of substance into science, and to give some representation to whatever it is that gets lost in that translation.

Conclusion

In this thesis, I have worked to describe the role and status of the body in the scientific field of transcultural psychiatry. I have found that the body is, at the hands of transcultural psychiatrists, instrumentalized in two ways: first, as a source of information; and second, as a site of intervention. In the distinct literary and technical culture of this psychiatric subfield, these conversions of the body are organized around the concept of the ecosocial niche: the body of a given person, in other words, is *situated* within its particular and unique environment — a life context which can, the literature argues, only be imagined and defined in symmetrical terms. In transcultural psychiatry, there is thus no question of 'the body' as such, but rather of bodies in the plural — each one existing as an ongoing process of *rematerialization* in relation to the factors, features, actors, and pressures of its sociocultural and biological milieu. By offering a form of *literacy* with respect to these environmentally-attenuated systems of the body, transcultural psychiatry offers its practitioners a *semiotic* and *technical* point of leverage, in the effort to reorganize both these environments and the bodies within them. It is in this way that the *representational* and the *interventional* sides of transcultural psychiatry are bound together: the construction of sense and the production of order are both authorized by the same scientific culture — a culture which is, I have argued, fundamentally structured by literary concerns.

One especially salient literary idiom at my sites of field work is that of stress: in the literature of ecosocial science, stress has begun to emerge as an increasingly compelling object of interest. By drawing on the public-health-style discourse of risk and environmental exposure, subfields such as allostatic load research have persuasively motivated their laboratory and statistical endeavours to construct black boxes around the social and somatic phenomenon of

stress. Stress also exerts a strong technical gravity in the clinical practice of transcultural psychiatry: it is, after all, on the basis of stress-based diagnostic categories such as post-traumatic stress disorder that transcultural psychiatrists often find the most narrative leverage in their efforts to stabilize, mobilize, and combine patients within their networks of care.

For what remains of this conclusion, I will provide a brief review of my arguments about stress and the body as I have presented them in this thesis. In so doing, I will foreground the specific decisions about giving name to stress made by scientists at my field sites. It is through these naming practices that the literary culture of science provides its 'users' — both clinicians and patients, researchers and participants — with distinct objects of mutual interest. This process of generating shared interest in a shared language is the basic mechanism of the information economy of science: by structuring and sustaining this kind of social-semiotic currency, the naming game of transcultural psychiatry grounds and establishes the playing field of those encounters of reciprocal exchange that I discuss above, at which the *authority* of science and the *order* or *sense* of embodied stress give rise to each other. In this sense, the *legibility* of scientific fact, the *intelligibility* of psychiatric distress, the *inhabitability* of the disordered body, and the *materiality* of the ecosocial niche all emerge from the same historical process of scientific action.

In my introduction, I began by articulating the Latourian concept of symmetrical inquiry. Latour's account of how to improve the social study of science, I argued, is also relevant to the counter-disciplinary arguments formulated by transcultural psychiatrists, from Wittkower and Fried to Kleinman and Kirmayer. Both groups of cultural critics, that is, argue that the material and the social dimensions of a given problem must be thought through concurrently and

interactively. The peculiar methodologies of laboratory ethnography (in which the material agency of the technical apparatus must be described in order to understand the social agency of scientific fact) thus bear a close resemblance to the methodologies of cultural psychiatry (in which the material agency of the biomedicalized body is read through its social and semiotic positionality). I argued that the disciplinary history of transcultural psychiatry, which blends the more deductive naming practices of anthropology with the more inductive ones of biological science, uniquely positions the field to respond to this Latourian call for symmetry.

In my first chapter, I drew out more clearly the kinds of conceptual concerns at play in the giving and using of names, by presenting a figurative solution to the question of natural language from classical philosophy. Plato's drama of the namegiver and the dialectician draws out some of the most salient concerns of scientific literacy: how to establish a stable linguistic taxonomy, how to give names that permit for the clear division and instruction of a shared world, and how the shared use of those names can either solidify or disrupt the conceptual taxonomies to which they belong. I ended the chapter with a meditation on the disciplinary name for transcultural psychiatry itself, demonstrating that the names which we use to navigate scientific space are not simply neutral vehicles for information, but contain their own dense webs of poetic and cultural associations. Just as we, as readers and writers, can become attached to one literary idiom over another for reasons of aesthetic judgement, the names of science are also subject to the vicissitudes and vagaries of taste and style — factors which, despite their apparent distance from 'objective reality', nonetheless become the subject of intense controversy in science.

In the second chapter, I defined some of the key conceptual terms of my own thinking in this project, and discussed their foundations in the relevant anthropological and scientific

literature. I began by contextualizing the disciplinary history of transcultural psychiatry and allostatic load, and gestured towards the inherent critical charge of these biomedical fields: by seeking to represent the plural mechanisms by which social order differentially inscribes itself on the bodies and minds of differently-situated communities and patients, both of these practices lay the groundwork for pluralistic interventions into sites or scenes of disorder. This basic premise of symmetrical medicine, I argued, is an important feature of its approach to social justice through medical equity. In the second part of the chapter, I discuss the conceptual terms at play in my analysis of how these fields transform the body through their naming practices — first, those terms which define the conceptual limits of the human body as an object of scientific inquiry; second, those terms which define stress as a matter of scientific concern with respect to the body; and third, terms from the field of science studies which structure my anthropological analysis of these scientific practices. Taken together, this set of terms allows for an ethnography that follows scientific action from its origin in literacy to its operationalization in technique.

In the final part of the second chapter, I define my three main sites of field work, and describe their relationship to each other through the interdisciplinary social networks of transcultural psychiatry in Montreal. At this point, I begin to make my argument that this form of science is a fundamentally collective practice, involving the ongoing construction of institutional infrastructures, economies, and conventions. By drawing attention to transcultural psychiatry's clear acknowledgement and ratification of this activity of social construction — as, in fact, a structural feature of scientific knowledge — I suggest that it takes important strides towards establishing cultural sensitivity and anthropological awareness within its own discipline. It is on the basis of this claim that I later undertake my critical evaluation of its practices.

The third chapter is the longest of my thesis, and it sets out in greater detail the conceptual framework of my ethnography. The chapter consists of a presentation of two distinct anthropologies of science: first, Bruno Latour's ethnographic description and philosophical interpretation of contemporary laboratory science; second, Didier Fassin's critical account of contemporary public health. In broad terms, the goal of this chapter is to articulate an historical context of biomedicine, within which it is then possible to situate and analyze the particular features of transcultural psychiatry. Of special interest in Latour and Fassin's frameworks are their attention to the use of names as a vector of scientific authority: according to both authors, the literary and technical name of a given scientific object is anthropologically significant, insofar as it becomes the subject of disciplinary controversy, and also serves as the point of contact between science and society. Once a name is given at one site of scientific production, it must be taken up by others as an organizing principle of further research: its status and credibility is only ever the result of this later use and redefinition. As the name continues to acquire disciplinary gravity and authority, it moves outwards into society — a movement which can either consist of tacit reception or troubled resistance. By rearticulating the power dynamics between physician and patient, or researcher and participant, transcultural psychiatry promises to transform the conventional mechanisms of this process of giving and using scientific names.

This first section of this chapter outlines the Latourian framework for ethnographic science studies: it emphasizes how science constructs order from noise, through an aesthetic process which transforms perceptions into clear and distinct facts. Latour argues that scientific knowledge relies on a socially-mediated literacy, in which instruments convert physical material into literary inscriptions, and these inscriptions, shaped by disciplinary pressures, form the

foundation of future facts. Science functions according to a kind of information economy in which instruments act as a mint, inscriptions as currency, facts as products, and literature as the marketplace — creating competition for resources and attention. The disciplinary divisions between the sciences are, in part, the result of this economic process. Latour also critiques the conventional separation of nature and culture in scientific literacy, asserting that knowledge production is inherently hybrid, as laboratory instruments mediate between human and nonhuman agents. The authority of scientific facts, he contends, arises not from their inherent objectivity, but from scientists' ability to cross the divide between nature and culture, refining their work through sociotechnical negotiation until their facts gain disciplinary credibility.

This framework for thinking through scientific action leaves us with a few important conclusions: first, that the work of science involves a collective process of constructing coherent names out of heterogenous sources of information; second, that the social authority of scientific actors is intimately bound up with the material activity of objects in their experimental trials; and third, that the blackboxed instrument is what sets the stage for that central scene of the scientific drama, in which these forms of human and nonhuman agency give shape to each other. All three of these arguments about scientific authority remain relevant to Didier Fassin's account of public health and the clinic. For Fassin, medical spaces are inherently political in nature: disorders and diseases of the body reflect social reality, insofar as they manifest differently in bodies according to their unique social status and position. The clinic, as that space in which these hybrid disorders are given their scientific names, functions like a kind of scientific instrument: physical material enters, scientists produce and assemble inscriptions out of it, and some new kind of object emerges, with a disciplinary definition and a technical program of action inscribed within it.

According to Fassin, in contemporary medicine, one increasingly prevalent kind of object is the public health crisis. In public health, the domain of concern is not just the individual body, but the wider sociomaterial environment; this symmetrical field is saturated with moral and political forces, and the dominant literary idiom is that of risk to the body and to society. By translating the environment into the language of risk, public health extends the mobility and the authority of medical scientists. Calculated and constructed in terms of statistical probability, public health crises exist in a literary form of accounting that has its own specific blind spots and zones of intensification. Much like in Latour's account of conventional science, Fassin argues that this literary fashion draws its authority from its ability to leave local context behind: by moving from physical bodies-in-the-clinic to a semiotic body-in-the-literature, public health works to construct diagnostic categories with broad social authority. For Fassin, the result of this constructive process is that the clinic becomes a site at which disordered bodies are continually rematerialized: in order to leverage the social authority of a diagnostic name, patients must assent to the way in which it makes sense of them. Part of what interests Fassin in these clinical encounters are the instances in which this diagnostic sense fails to materialize — especially when medical actors resist participating in these programs of action. By asserting the particularity of their unique situations, patients and their physicians can disrupt the semiotic economy of a given diagnosis. In my analysis of transcultural psychiatry, I draw on Fassin's account of the clinic and of the public health crisis to contextualize this different form of participatory namegiving.

My fourth chapter presents the methodology of my ethnographic research, and describes some of the peculiar ethical quandaries involved in researching researchers. I discuss the rules of method and principles of inquiry provided by Latour in *Science in Action*, and review the

procedures of laboratory ethnography as he presents them in *Laboratory Life*. I outline the processes of note-taking, participation, and textual analysis that constitute my fieldwork; I also define my main objects of study as the technical activities and conversations of my participants — and not their personal sentiments or arguments about science. I identify my primary anthropological interest as consisting of the literary and technical transformations of the body at the hands of these scientists, and the names which they construct around this instrumentalization of somatic material. Finally, I argue that these technical processes of inscribing and naming the body must be understood in the context of the disciplinary literature that informs them; it is on this basis that I motivate my fifth chapter, which consists of an extensive document analysis of the fields of transcultural psychiatry and allostatic load research.

My analysis of the transcultural psychiatry literature, in the first section of the chapter, establishes two main points: first, that the ecosocial ontology which informs the contemporary field is essentially symmetrical in nature; second, that this ontological commitment enables a clinical methodology which importantly redefines the diagnostic categories of public health and biomedical science. The argument is simple enough in structure: because transcultural psychiatry abides by a symmetrical form of literary representation, its technical instruments are organized around interventions which are also symmetrical in nature; culturally-specific problems demand culturally-sensitive solutions. At stake in transcultural psychiatry is not just the treatment of disordered bodies, but rather the treatment of disordered sociomaterial environments.

My review of the allostatic literature results in a similar argument. I identify the hybrid accounting at the heart of this research program, which treats the biological mechanisms of stress as directly related to the social order and its pressures. I discuss the historical context of this

subfield of neurobiology, in which researchers are still attempting to construct instrumental black boxes capable of persuasively representing ecological stressors on the body. The most prominent of these black boxes, the allostatic load index, promises to answer the public-health question of environmental risk, by comprehensively capturing the nonhuman agents of physiological stress, and measuring their effects on the body as a system. In transculturally-informed allostatic load research, the cultural variability of social life is just one more factor to integrate into accounting for stress. Because every body inhabits a unique cultural position and situation, the technical apparatus of allostatic load research needs to include some instruments capable of producing cultural information. Sociological inventories and ethnographic interviews, by enabling this ordering or sense-making of narrative information, contribute to the methodologically-symmetrical project involved in constructing a science of something like Queer stress.

In the sixth chapter, I provide an ethnographic analysis of my three field sites. Following the Latourian method, I first present an introductory story of my participant observation, before undertaking a more structured analysis of the social and technical dimensions of the classroom, laboratory, and clinic. Following ecosocial facts outwards into society, I argue that the spaces of transcultural psychiatry consist of activities which I define as constructive proposition, productive articulation, and participatory organization. Taken together, these three kinds of collective action describe the efforts of symmetrical scientists in their effort to rematerialize stress, by reorganizing the uniquely-situated bodies and environments in which they encounter it.

The final chapter of my thesis consists of my interpretation of these ethnographic findings. Bringing in Ian Hacking's concept of the looping effect to refine my account of the diagnostic niche, I argue that the names of transcultural psychiatry acquire more authority and

reality as they become more socially useful. With this proposition of the diagnostic niche — as a collective and ongoing construction of psychiatric literacy — the trajectory of my argument about naming the body reaches its conclusion. Beginning with an instrumentalization of the volatile and troubled material of the distressed body, the information economy of this science gives rise to a series of reciprocal social relations, which in turn structure the technical activities and semiotic objects of the classroom, laboratory, and clinic. Refined by the economic and disciplinary pressures of these spaces, its names gain enough strength to leave their context of production behind, moving outwards into society as credible and objective facts. As social vectors of scientific authority, these diagnoses there organize a new kind of encounter between physician and patient, whereby the disordered body is once again instrumentalized as a point of leverage against environmental stress. The agency of the body and the features of its environment are both rematerialized in this process of participatory organization: authorized by the diagnostic name, its users are able to construct alliances, shift resources, and redistribute the burdens of competence around the situated embodiment of stress in question.

In a symmetrical science such as transcultural psychiatry, factual authority is opened up to participants on both sides of the encounter. Patients and research subjects — with their situated knowledges and unique embodiments — become active participants in the scientific information economy. By contributing their particularities, their differences, and their stories to this ongoing effort of constructing cultural sensitivity, they introduce a principle of irreduction to the facts at play in this credit cycle. Such facts, it may seem, are less strong than those of our conventional sciences. As names given to use in the world, I would argue, these peculiar literary entities are more lively, more dynamic, and more interesting than those that have come before.

At this point, I only have one more trick to play. I'd like to conclude this thesis by returning to visit our old friend Plato, and his imaginary friend, the *nomothétēs* — the giver of names, or of laws. In the first chapter of this essay, I discussed Plato's characterization of this figure's work as a two-part effort: first, they apprehend the pattern of natural reality; then, they externalize that pattern in the representative material of their choosing. In the case of the namegiver, this material is a linguistic taxonomy of identities and divisions, which can be put to use in skillful philosophical dialectic. In the case of the lawgiver, the material in question is that of a legislative code, which is put to use by the citizens and judges of a republic, in their ongoing enactment of political life. In both cases, the principle and end of these actions is to participate in what Plato calls the "divine" pursuit of wisdom (*Letters* 340c) — to imitate the life of the gods, by more fully embodying our proper nature as political and philosophical creatures.

But the project never seems to go very well for long. Plato's beloved Attic dialect became the origin of rhetorical sophistry and professional deception (cf. *Gorgias*); the Athenian democracy of Pericles and Thucydides gave way to the disastrous Sicilian expedition and the rule of the Thirty Tyrants (cf. *Parmenides*). How does Plato account for this failure of philosophy to bring its users closer to some stable reality, once and for all? He provides an interesting and useful hint in the strange and wonderful *Timaeus* dialogue, which takes place one day after the famous events of the *Republic* dialogue, in which Socrates finally provided his most comprehensive description of a true philosophical politics. In the *Timaeus*, Socrates asks the titular character to relate his memory of an ancient Egyptian account of the history of the universe, from its origin up to the appearance of the first human.

In this story, Timaeus describes the cosmogonic activities of the semi-divine demiurge, a craftsman-figure who stood between material reality and the world of the gods. Much like Plato's lawgiver, the demiurge was tasked with ordering physical material in accordance with divine nature (29a). The demiurge began by creating the five elements of classical Greek physics, and establishing physical principles such as time, motion, identity, and difference (35a-39e). It all goes more or less swimmingly, up until the completion of the human soul and body (40d-47e). As soon as Timaeus reaches this point in the story, he introduces the principle of "the Straying Cause" (48a) — a force of disruption, agitation, or volatility inherent to physical matter itself. The world, in other words, is "of mixed birth" (48a). All our best efforts here below will never fully live up to the lawgiver's intellectual vision, because there is some principle of necessity, proper to the material of the body, whose "nature it is to set things adrift" (48b).

It is something very much like this Straying Cause that our friends in transcultural psychiatry seem to encounter in their ongoing efforts to diagnose the disorders of the mind and body. Even our best names for these conditions, it seems, will never quite stay in place; under new cultural situations, the body sets itself adrift in unexpected and unpredictable ways. The very effort of naming, as Hacking would remind us, continually undermines itself: as soon as a new diagnostic niche emerges, it gives rise to new ways of life. Medical thus literacy constructs its black boxes around that material whose intrinsic nature it is to "drift continually," as its own names rematerialize the boundaries of illness and wellness (52e). Vibrant matter, volatile flesh, or straying cause: it is this endlessly-interesting matter of concern that governs the game of science in action — always giving rise to new facts and factions, always refashioning itself under the sign of new disciplines, always authorizing new realities and constructing new encounters.

Thank you for reading.

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Appendix. Methodological Structures

(1) Rules of Method and Principles (from *Science in Action*)

Rule 1.
We study science *in action* and not ready made science or technology; to do so, we either arrive before the facts and machines are blackboxed or we follow the controversies that reopen them. (Introduction)

Rule 2.
To determine the objectivity or subjectivity of a claim, the efficiency or perfection of a mechanism, we do not look for their *intrinsic* qualities but at all the transformations they undergo *later* in the hands of others. (Chapter l)

Rule 3.
Since the settlement of a controversy is the *cause* of Nature's representation, not its consequence, we can never use this consequence, Nature, to explain how and why a controversy has been settled. (Chapter 2)

Rule 4.
Since the settlement of a controversy is the *cause* of Society's stability, we cannot use Society to explain how and why a controversy has been settled. We should consider symmetrically the efforts to enrol human and non-human resources. (Chapter 3)

Rule 5.
We have to be as *undecided* as the various actors we follow as to what technoscience is made of; every time an inside/outside divide is built, we should study the two sides simultaneously and make the list, no matter how long and heterogeneous, of those who do the work. (Chapter 4)

Rule 6.
Confronted with the accusation of irrationality, we look neither at what rule of logic has been broken, nor at what structure of society could explain the distortion, but to the angle and direction of the observer's *displacement*, and to the *length* of the network thus being built. (Chapter 5)

Rule 7.
Before attributing any special quality to the mind or to the method of people, let us examine first the many ways through which inscriptions are gathered, combined, tied together and sent back. Only if there is something unexplained once the networks have been studied shall we start to speak of cognitive factors. (Chapter 6)

First principle.
The fate of facts and machines is in later users' hands; their qualities are thus a consequence, not a cause, of a collective action. (Chapter 1)

Second principle.
Scientists and engineers speak in the name of new allies that they have shaped and enrolled; representatives among other representatives, they add these unexpected resources to tip the balance of force in their favour. (Chapter 2)

Third principle.
We are never confronted with science, technology and society, but with a gamut of weaker and stronger *associations*; thus understanding *what* facts and machines are is the same task as understanding *who* the people are. (Chapter 3)

Fourth principle.
The more science and technology have an esoteric content the further they extend outside; thus, 'science and technology' is only a subset of technoscience. (Chapter 4)

Fifth principle.
Irrationality is always an accusation made by someone building a network over someone else who stands in the way; thus, there is no Great Divide between minds, but only shorter and longer networks; harder facts are not the rule but the exception, since they are needed only in a very few cases to displace others on a large scale out of their usual ways. (Chapter 5)

Sixth principle. History of technoscience is in a large part the history of the resources scattered along networks to accelerate the mobility, faithfulness, combination and cohesion of traces that make action at a distance possible. (Chapter 6)

(2) Modality and Statement Types (from *Science in Action* and *Laboratory Life*)

Positive Modality: sentences that lead a statement away from its conditions of production, making it solid enough to render some other consequences necessary.
Negative Modality: sentences that lead a statement in the other direction towards its conditions of production and that explain in detail why it is solid or weak instead of using it to render some other consequences more necessary.

Type 5. Simple, taken-for-granted facts
Type 4. Facts which include the explicit technical information
Type 3. Modalized or qualified facts
Type 2. Claims which include the explicit circumstances of their production
Type 1. Conjectures or speculations

(3) Translation Types and Tactics (from *Science in Action*)

Translation 1. *I want what you want.* Your interest will be my interest, for now.
Translation 2. *I want it, why don't you?* My interest should be your interest.
Translation 3. *If you just make a short detour…* Our separate interests can be achieved together.
Translation 4. *Reshuffling interests and goals*
    Tactic 1. *Displacing goals*. If you pursue my interest instead, we will get to yours.
    Tactic 2. *Inventing new goals*. Your interest is actually different than what you thought.
    Tactic 3. *Inventing new groups*. Our interests are actually shared, let's organize.
    Tactic 4. *Rendering the detour invisible*. You always had to come this way.
    Tactic 5. *Winning trials of attribution*. I deserve credit for achieving your interest.
Translation 5. *Becoming indispensable*. My interest will be your interest, for now.

(4) Laboratory Positioning Moves (from "Give me a Laboratory…")

Move 1. *Capturing others' interests*. Translating your problems into my terms.
Move 2. *Moving the leverage point*. Finding some element of the problem to isolate in the lab.
Move 3. *Moving the world with the lever*. Reorganizing society around the new scientific object.

(5) Methods of Description (from "Where are the Missing Masses?")

Description. What is going on at the scene of action?
Transcription. What is a better way to organize the scene?
Inscription. What is the activity of each participant in the scene?
Prescription. What activities are all of the participants required to perform?
Subscription. What conditions are required of all the participants for them to join?
Circumscription. What are the limits of the scene and who is excluded?
Ascription. What agency is responsible for the activity in the scene?
Conscription. How does the scene solicit its actors?

(7) Qualities of Good Science (from "How to Talk About the Body?")

Quality 1. Knowing is rare and cannot be the outcome of a general method
Quality 2. Propositions have to be interesting
Quality 3. Laboratories must take risks
Quality 4. Inquiries must invite resistance
Quality 5. Participants or reagents must have the chance to ask their own questions
Quality 6. Neither distance nor empathy are good scientific attitudes
Quality 7. Science does not eliminate alternative forms of articulation
Quality 8. Scientists must take versions of reality from outside the laboratory very seriously

(6) Engines of Making up People (from "Making up People")

Engines of Discovery
1. *Count*. How many people are of this kind?
2. *Quantify*. What are the objective conditions of belonging to this kind?
3. *Create norms*. What is the relation of this kind to normalcy?
4. *Correlate*. What are the associated conditions of belonging to this kind?
5. *Medicalize*. How are the people in this kind to be managed by doctors?
6. *Biologize*. What are the biological origins of this medical condition?
7. *Genetecize*. What are the genetic origins of this biological condition?

Engines of Practice, Administration, and Resistance
8. *Normalize*. How do we make these people more normal?
9. *Bureaucratize*. How are the people in this kind to be managed by administrators?
10. *Reclaim our identity*. How do people of this kind resist those who manage them?

(8) Axes and Effects of Public Health (from *Les mondes de la santé publique*)

First Axis. *The inscription of the social order in the body*: regulation of inequalities in health
Second Axis. *The power to heal*: differentiation of medicine as a field
Third Axis. *The collective management of illness*: administration of the government of life

(9) Vectors of Power in Spaces of Health (from *L'éspace politique de la santé*)

First Vector. *The diagnostic gesture*: ways that researchers and physicians make up kinds.
Second Vector. *The relieving act*: ways that physicians and administrators organize care.
Third Vector. *The legislative decision*: ways that administrators and researchers sustain networks.

(10) Stages of the CFI Consultation (from "Influence of the DSM-IV")

First Stage. *Psychiatric History*: the conventional biomedical formulation of the case.
Second Stage. *Cultural Formulation*: the presentation of the CFI findings.
Third Stage. *Interdisciplinary Assessment*: the interaction of different perspectives on the case.
Fourth Stage. *Interdisciplinary Deliberation*: the discussion of possible treatment options.
Fifth Stage. *Closure*: the acknowledgement of competencies and contributions.

Methodological Protocols

Participant Observation

Note-taking of activities

- Define the scenes of inscription and the processes of blackboxing
- Interpret the experimental or clinical protocol in terms of positioning moves

Note-taking of discussions

- Describe the content of statements; code the internal themes
- Analyze the themes in terms of modalization and translation

Interviews (See Appendix 2, 'Interview Guide')

Document Analysis

- Comprehend the technical information
- Identify the names and modalities
- Analyze the citational chains and research program