Living and Working with the New Medical Technologies

Intersections of Inquiry

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Contents

vii

Part III Technologies and bodies: the extended networks of biomedicine

1   Introduction

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Use of the term "intersections" in the title of this book undoubtedly evokes impressions of postmodernist rhetoric, yet it is also a term long used by symbolic interactionist sociologists to analyze "intersecting" lines of action and social worlds.[1] This interplay of postmodernity and tradition is precisely one of the "intersections" we undertake to explore in this volume. Further intersections arise between the various analytical approaches exemplified by the authors represented in this book, and also between the human actors, the tools, the entities and the bodies that are constitutive of the new medical technologies. How these intersections relate to each other, in other words, how new biomedical objects and subjects call for new kinds of analyses, is one of the issues raised by the present collection of articles.

As indicated by the book's title, one can work with the new medical technologies, and we all live, directly or indirectly, with them. Some of the contributors tend to focus on the "working" side of this equation, others on its "living" side, while all struggle, more or less openly, to bring these two sides together. The authors display many differences in their choice of topics and approach (two not entirely independent elements). However, they share an understanding of "body politics" that, instead of rejecting or accepting recurring dichotomies such as that between Nature and Culture, looks at how dichotomies are produced. Yet, rather than focusing on either differences or commonalities, it seems more interesting to us to look at intersections, that is, temporary convergences that can lead to advances on some particular problem, with no pretence of providing a comprehensive world-view or a theoretical manifesto.[2]

The book was born then from an attempt at a dialogue across disciplinary fields that took place at a three-day conference held in July 1996 at Cambridge University, England. The fields represented were (medical) anthropology and science studies, although the latter can hardly be considered a disciplinary field but, rather, a loose connection of scholars focusing on a problem area. Reference to a "dialogue across

disciplinary fields'' is not meant to imply that organizers and partici-
pants took for granted disciplinary demarcations, nor that most of them
claimed allegiance to a clear-cut disciplinary identity, though this is evi-
dently the case for some contributors. Rather, the conference could be
said to lie at the intersection of two domains that, in spite of their inter-
est in the same substantive area, differ in the ways they conceive of their
analytical objects. This neat divide into two camps does indeed capture
some of the dynamic of the meeting, but it also oversimplifies it, as it
soon became apparent to all participants that ''science studies'' does not
present a united front but, rather, a diversity of approaches mobilizing
sociology, epistemology, history, and anthropology itself. Similarly, an
anthropology of medicine, while the culture concept is usually retained
as one of its central concepts, nevertheless draws on insights obtained
from history, sociology, and so on.

Conception of the conference can be traced back to ongoing dis-
cussions over the last few years among the three book editors, two
anthropologists and a sociologist who, while belonging to the same aca-
demic department, operate in different, though intersecting, disciplinary
and professional networks. By resorting to a somewhat trite metaphor,
one could argue that the conference and book grew out of the need, on
the editors' part, to explore and clarify some of the perceived relations
and tensions, both within and between their department microcosm and
their professional macrocosms. Yet, we would resist such a description,
for the problem, as we frame it, is less one of drawing parallels between
different ''levels'' of generality, than one of understanding the pro-
duction of generalities out of local situations.[3]

As the conference progressed, discussions started to crystallize around
a few intersections including agreements on what the disagreements are.
Simultaneously, attempts at increasing the generality of the discussion
by mobilizing a disciplinary rhetoric were replaced by more concrete
references to a diversity of audiences, the actual or ideal readers of our
work. To talk of audiences has the advantage of stressing the agency of
researchers, their active shaping of discourses to interest colleagues, the
subjects they study, funding agencies and so on. To talk of audiences
also immediately evokes a potential multiplicity and heterogeneity of
individuals and groups. In our case, they ranged from academic col-
leagues across various social science fields, to the researchers and clin-
icians in the biomedical field who were the objects of, but also often the
collaborators in, our inquiries, to the patients who redefine their identit-
ies and properties by interacting with the new technologies of the body.

It seems fair to argue that at the conference the (real or perceived)
challenge came from science studies – a relative newcomer to the aca-

demic world – and it crystallized around the notion of ''black box.'' Briefly put, from the point of view of science studies, mainstream work in well-established disciplines such as anthropology or sociology, even when purportedly dealing with medical or scientific matters, generally ignores the production of clinical and laboratory objects and procedures, thus treating them as ''black boxes.'' The ''opening of black boxes'' is often seen as (one of) the specific task(s) of science studies, with the proviso that there is no agreement on what would constitute a proper ''opening'' of the boxes. The counter-argument raised by several anthropologists at the conference was that a focus on the opening of the black boxes leads to a neglect of the ''broader context'' within which scientific and technological objects evolve in practice, and translates into little interest in their fate once they leave the scientific laboratory – a point to which we will subsequently return.

Numerous articles could be cited to validate both sides of this argument, but the contributors to this book make it clear that such a dichotomous, oppositional setting apart of medical anthropology and science studies does not apply here. On the contrary, most of the authors, precisely by focusing their interpretations on intersections among the activities of a range of actors and objects, overcome, to a degree, the dilemma posed by black-box or broader context. This is not to suggest, however, that some kind of rapprochement was achieved; on the contrary, as the essays make clear, deep epistemological differences remain among the authors of these essays.

Problematics of Medical Anthropology

Over the last twenty-five years, medical anthropology – as a field clearly defined by distinctive professional associations, journals, symposia, programs of graduate training, and so on – has emerged from insignificance to become one of the most conspicuous anthropological specialties. During this period of expansion, medical anthropology has likewise grown more variegated, both as to its subjects – international health, psychiatry, indigenous systems of medical practice, new biomedical technologies, and so on – and its perspectives and epistemologies. Until the 1960s, anthropologists interested in medicine tended to accept the goals set by Western medical institutions or, alternatively, to follow an ethnographic tradition that located sickness and healing as subjects within the circle of witchcraft, sorcery, magic and religion. Today anthropologists can choose from an assortment of orientations – from anthropologies of suffering, experience and the body; from anthropologies that identify themselves with the interests of Western medical

institutions; and from anthropologies that seek to demonstrate how values, interests and bias are integral to all medical knowledge and practice. Because of these developments and the divisions to which they have given birth, one should not assume that writer and reader share a consensus regarding the meaning of this term "medical anthropology."

Despite these various orientations, the term "medical anthropology" is sometimes understood to imply that anthropologists interested in sickness, healing, bodily consciousness and similar subjects, constitute an autonomous subdiscipline, and that they rely on methodologies and theories that are particular to medicine and substantially different from those employed by social and cultural anthropologists. We reject this proposition, at least as far as it refers to the kind of work represented in this volume; "medical" ideas, practices and responses ought to be approached in the same way, *mutatis mutandi*, that anthropologists concern themselves with other domains of life.

The anthropology of medicine is free to follow many paths. One path is *interrogatory*. It seeks solutions to the puzzles stemming from people's claims about events and objects in the material world – their ideas about witches, surrogate motherhood, and pathogenic (traumatic) memories, for example. It rejects what can be called the "idea of cognitive generosity" – the notion that inconsistencies and contradictions in people's accounts of the world are merely apparent, rather than real (Lukes 1967; Sperber 1985: chapter 2). The starting point of the interrogatory path is the assumption that goal-oriented human behavior is guided by reason (rationality). Evidence to the contrary can be interpreted as the failure of the observer (anthropologist) to completely understand the perceptions, beliefs and desires of the individual actors. But what exactly does "reason" mean in these circumstances? At the very least, it would imply an ability to apprehend contradictions, inconsistencies and emotionally charged input to debate. But the idea that we all, anthropologists and the people whom anthropologists study, can spontaneously agree on what constitutes a contradiction or an inconsistency is precisely the problem. Of course, there is a wide range of perceptions on which anthropologists will generally agree with our informants. For example, both of us will be able to pick out individuals afflicted with severe psychoses as different from other people. But spontaneous agreement is missing in many instances, including the puzzling events that we frequently encounter while studying the diagnosis and treatment of sickness in other societies. And the reason is clear: the conditions that determine whether a belief or a claim is a contradiction and/or an "inconsistency" are cultural and not universally evident.

How do we account for the orderliness of people's medical behaviour

unless we assume that they are rational? And if we cannot claim a shared rationality with our informants (minimally the ability to agree on what constitutes a contradiction), then how can we claim to understand and interpret their life worlds and their cultures? The solution is not to renounce reason, but rather to *relocate it* – moving it out of the minds and brains of individuals and into the social institutions, technologies and practices through which individuals interact with one another and with other elements of the material world. Instead of using this term "reason," which brings with it unwarranted assumptions about the tendencies and powers of the human mind, it would be clearer if we substituted for it another term, *styles of reasoning*, borrowed from the philosopher Ian Hacking (1992). Where "reason" suggests something unitary, universal and constant, styles of reasoning suggests variation – that is, ways of making facts and meanings that change from society to society, and from venue to venue within a given society.

This version of an anthropology of medicine has many historical sources, but the most obvious place to begin is with J. G. Frazer's familiar observations on magic, science and religion. Frazer identified religion with the propitiation and supplication of natural and supernatural powers, and he identified magic with the coercion of these same powers. In Frazer's version, magic resembles science more closely than it resembles religion. To grasp the significance of his point, one must understand his ideas about reasoning. Like his great predecessor, the empiricist philosopher David Hume, Frazer believed that human cognition is guided by principles of *association*. Mind is initially a blank slate upon which experience leaves its impressions. In the course of further experiences, these impressions are associated into complex concepts, through relations based on resemblance and contiguity.

According to Frazer, beliefs and practices connected with magic are based on two kinds of associations: resemblance or "sympathy," where like is seen to produce like, and contiguity or "contagion," in which things once in contact continue to influence one another after the contact is ended. Scientific reasoning is based on similar associations but, in this case, they are constrained and controlled. In contrast, the cognitive processes underlying magic are characteristically confused. Nonetheless, magic and science are connected genealogically: science is the product of a progressive refinement in thinking rather than a discontinuity.

Frazer's account is individualistic and cognitive. In it, systems of beliefs and practices are displaced by the perceptions and mental operations of solitary thinkers; psychology is treated as the queen of the human sciences (see Ackerman 1987: 40, 51, 157–8). In other words, he is writing about reason rather than styles of reasoning.

E. E. Evans-Pritchard's famous monograph on magic and medicine, *Witchcraft, Oracles and Magic Among the Azande* (1937), departs radically from Frazer's work. The Azande book is the product of intensive ethnographic research conducted in the language of the local people, while Frazer's research consisted of collecting vignettes from unsystematic and fragmentary literary accounts. During his lifetime, Frazer's work was described as being "comparative"; today we are inclined to see it as being merely "anecdotal."

Evans-Pritchard's investigations of sickness and witchcraft led him to the oracles through which the Azande construct their disease etiologies, manage their sickness episodes, choose their interventions and identify targets for vengeance magic. By Western standards, the oracles appear to be "so much nonsense," and Evans-Pritchard makes this his starting point. Evans-Pritchard's achievement was to put the apparent nonsense into a context, that is, a *system* of ideas, perceptions, technologies and social relations. And, following this, he proceeded to demonstrate the system's underlying coherence.

In the context of his book, "coherence" refers to two things. First, it refers to a formal property of the system, its capacity to reproduce itself over time. For example, the Zande system is organized in such a way that oracles provide an endless supply of "witches," thus perpetuating the Zande inclination to consult witch-catching oracles when someone falls sick or suffers misfortune. Second, coherence implies the existence of a thinking subject (the typical Zande) and a standard against which the subject's words (and the thoughts and perceptions that they ostensibly mirror) and behavior can be measured. In other words, the system is coherent in the sense that its elements meet a standard that has been set for them by Evans-Pritchard and by his ideal reader.

Beneath the surface of this text, Evans-Pritchard is engaged in conversation with Westerners who would question the rationality of the Azande. His book is simultaneously an ethnography and an interrogation in which the Azande are made to account for logical inconsistencies and contradictions. This represents a mode of discourse that is not only foreign to Frazer's evolutionary associationism, but is also hostile to the principle of cognitive generosity.

Sixty years after its publication, *Witchcraft, Oracles and Magic Among the Azande* remains a monument in the history of the anthropology of medicine. Contemporary readers may find parts of it rather old fashioned, specifically where Evans-Pritchard introduces science into his discourse. "[W]e need to judge to whom we can appeal for a decision when a question arises whether a notion shall be classed as mystical." This judge or standard is science – according to Evans-Pritchard, a

development of common sense that is distinguished from the latter by its superior techniques of observation and reasoning, most notably its fidelity to "experiment and rules of Logic" (Evans-Pritchard 1937: 12). Some of these same readers might object to the way in which Evans-Pritchard characterizes science, as a highly refined form of rule-governed rationality. Science, they will want to argue, is a powerful and successful system of reasoning, a historically determined assembly of objects, technologies, social relations and language games. Indeed some readers will want to go even further in this respect, to ask whether it is possible to employ the term "science" in any useful sense other than the plural; not science, but rather the "sciences," each deploying its particular system of reasoning. And which of these systems did Evans-Pritchard have in mind as his standard: medical science, biological science, psychiatric science, particle physics?

If we dispense with the idea of a unitary rule-governed science and a correspondence theory of knowledge, is it possible to continue to interrogate cultures in the way that Evans-Pritchard interrogated Zande oracles? This is a serious question, because its answer will determine not only how we will interpret *Witchcraft, Oracles and Magic Among the Azande* from this point on, but likewise how we will study and describe our common subject, whether it be medicine, sickness or madness.

From Representations to Practices

The "science question," that is, the question of whether or not science should be considered as a socio-cultural endeavor and analyzed as such, has not only been a key issue to anthropologists interested in investigating biomedicine, but has also been instrumental in the development of the field known as "science studies." And here is another intersection. Evans-Pritchard's book that, as argued in the previous section, should be considered "a monument in the history of the anthropology of medicine," also appears to be a persistent trope in many early science studies texts, in particular those originating from the Edinburgh-based "strong program."[4] The choice of the Azande's poison oracle as a key example in Barnes's and Bloor's manifestos for a new sociology of science was of course not an innocent one, since that same example had been mobilized by philosophers engaged in normative analyses of science. In the particular reading of the strong programers, the poison oracle thus became at the same time a resource against and a terrain on which to confront attempts at a demarcationist philosophy of science, one that is aimed at developing logical, a priori criteria, such as "falsifiability" and a universal understanding of "rationality" for establishing a distinction

between scientific and non- (or pseudo-) scientific knowledge. According to strong programers, social, rather than logical processes, grounded all sort of beliefs, including those labeled as scientific, and thus a sociology, rather than a philosophy of science, was called for to account for the content of science.[5]

Yet, in spite of the excitement that it generated and still seems to generate in some quarters, it soon became apparent that the claim that scientific knowledge is socially produced is a far less interesting one than attempts to investigate *how* that same scientific knowledge is (re)produced. And, following up on this insight, it also became evident that the socio-cultural reductionism advocated by strong programers was a mirror image of the logical or technological reductionism advocated by their philosophical counterparts and, as such, untenable. As a way out of this dilemma, one had to shift the focus of inquiry from knowledge to practices, thus completing the ethnographic turn by looking at the ''material culture'' of science, at how scientists perform things, rather than at the frozen products of those performances. Interestingly enough, after Evans-Pritchard, it was yet another scholar of the 1930s, Ludwik Fleck, who can retrospectively be said to have first shown how to go about doing precisely these kinds of analyses.

Fleck, the author of a masterful account of the development of the Wassermann reaction to diagnose syphilis that antedates the publication of Evans-Pritchard by two years (1979[1935]), is presently credited with having written the very first monograph dealing with the content of scientific knowledge from a thoroughly sociological point of view. Fleck's contribution, however, initially attracted far less attention than Evans-Pritchard, and even today Fleck is often perceived as a mere ''precursor'' of Kuhn. This is how, for instance, Barnes (1982) presented Fleck's work.[6] Yet, another reading of Fleck is possible (not to speak of another reading of Kuhn![7]), one that chooses to emphasize not so much Fleck's claim that facts about syphilis were actively shaped by thought-styles or paradigm-bound collectivities, but, rather, his detailed description of how ''practice,'' understood as a series of ongoing interactions between researchers, tools, instruments, resources and other elements of biomedical networks, accounted for how the Wassermann test was developed and accepted as a reliable diagnostic procedure.

So here, it would seem, we could find another intersection between anthropology and science studies, insofar as the ethnographic analysis of the material culture of collectivities is something science studies practitioners share with anthropologists. Yet things are not so simple, since ''practice,'' as seen by the latter, is intimately linked with or shaped by culture, while for at least some of the former, a practice-oriented or

''performative'' approach (such as the one exemplified in Mol's chapter in the present volume) represents an alternative to cultural analysis. Traces of this opposition are easy to find in the present book, but it should quickly be added that the demarcation line between these different approaches does not run smoothly between anthropology and science studies; rather, it cuts across each field, especially in the case of science studies, a state of affairs once again readily detectable in this collection of essays.

The papers in Part II of the present volume point to the existence of these intersecting, yet diverging paths. Ilana Löwy's chapter clearly lies on the ''culturalist'' and ''contextualist'' side of the debate, insofar as its twin goals are to show how clinical trials for anti-HIV drugs were shaped by a long-established, pre-existing culture of clinical experimentation evolving out of cancer research – an argument that profoundly qualifies claims about the alleged decisive role played by the intervention of AIDS activists (Epstein 1996) – and to argue instead that a specific political and economic context constrained their design and performance. In contrast, Annemarie Mol adopts a ''performative'' approach that replaces the dichotomy between ''disease'' and ''illness'' (the medical and the social) with an ethnographic understanding of the material realities (the plural is important) of diseases. As a result, the picture of a culturally and cognitively homogeneous biomedical model facing the lifeworld of patients – or the constraining powers of context-defining institutions – is replaced by the pragmatic intersection and juxtaposition of ''differences'' (Berg and Mol 1998) that proliferate on both sides of the alleged divide. The chapter by Peter Keating and Alberto Cambrosio also questions socially or culturally determinist accounts by examining competing classifications of nosological entities. Rather than reducing differences to the incommensurability of social worlds or professional segments, the authors stress the formal and informal regulatory activities that allow a variety of perspectives and practices to co-exist and unforeseen options to emerge. Regulation, in this sense, is not restricted to the organization of consensus. By making hierarchies and choices explicit and consistent across laboratories, it leads to the production of new, unexpected events and highlights the emergent qualities of any resulting scheme. Finally, Allan Young's chapter examines the multiple styles of reasoning – epidemiological, statistical, clinical and experimental – that intersect in psychiatric science and clinical practice through technologies of traumatic remembering and forgetting. The chapter traces the transformation of these technologies over the past half-century, in relation to the biologization of mental illness (and developments in psychopharmacology, neuroscience, and imaging

technologies), the rise of population-based epidemiological research, and the creation of the National Institute of Mental Health.

It can indeed be argued that a source of disagreement between the various authors represented in this book relates to the question of whether or not a notion such as that of ''context'' (be it cultural, social or whatever) should be invoked to account for the topic under investigation. The answer to this question depends, in part, on what one means by the term ''context.'' For instance, the term is often used to argue that in analyzing biomedical innovations one should look not only at the biomedical setting from which they emerged but also at their subsequent fate in relation to patients, medical insurance companies, the popular press, and so on. In spite of possible disagreements on how this is actually to be done, this interpretation of the ''context'' clause is not *per se* very controversial, for all can agree that a detailed ethnographic analysis of new medical technologies should include the extended network that is co-substantive with their definition. However, talk about context can also be interpreted to mean that a priori-defined cultural, social, economic and political factors should be included in the analysis of scientific and clinical work. Yet, whether scientific or clinical laboratories can be equated with some sort of subsystem contained in a larger social setting and open to outside cultural and social influences is an open issue. To think in this way, to mobilize this kind of ''container metaphor'' (Lakoff and Johnson 1980), is to engage in a profoundly asymmetrical endeavor, treating scientific and medical notions and practices as open to investigation while taking for granted sociological and anthropological understandings of society and culture. To the criticism that one should not treat scientific and clinical research as if they were taking place in a social and cultural void, it could be countered that the point is not about whether biomedical practices take place within or outside a socio-cultural space but, rather, to ask which tools should be used to account for those very practices. Rather than using society to explain nature (or vice versa) one should investigate the co-production of nature and society.[8]

In this last respect, Hans-Jörg Rheinberger (1997: 37) espouses a clear-cut stance, when he claims that ''[i]t is not, in the end, the scientific or the broader culture that determines ''from outside'' what it means to be a laboratory [. . .]. It is ''inside'' the laboratory that those master signifiers are generated and regenerated that ultimately gain the power of determining what it means to be a scientific – or a broader – culture.'' If this is so, one can indeed be led to argue, as Rheinberger does in the first chapter of the present collection, that the advent of genetic engineering and its application to medicine corresponds not

simply to a radical transformation of biomedical practices and representations, but also, more generally, to a collapse of ontological distinctions between nature and society, one that will require, among other things, a profound redefinition of the tasks of the social sciences. Paul Rabinow's chapter contains a sharp criticism of Rheinberger's argument, and of the germane argument by Latour (1993), that he characterizes as epochal and metaphysical. Rabinow urges us to desist from these kinds of totalizing categories and to focus on a more restrained research strategy, one intent on developing a series of limited concepts that will simultaneously avoid pseudo-entities such as ''culture'' and ''science'' and allow for the naming of things that had previously to be left unnamed.

Technology and Human Subjects

The constitution and transformation of physical bodies and individual identities through technological practices is a concern of several of the anthropological contributors to this volume. It is at sites of practice, particularly policy-making arenas and clinical settings, where most attention has been paid to the way in which, following Foucault, biopower is performed. Certain feminist anthropologists have taken up the problem of the microphysics of power where Foucault left off and, going beyond the original argument about subjugated knowledge and the work of repression, have made the accounts and experiences of those on whom technologies are practiced central to their investigations. In science studies it was recognized that non-human actors have agency and are not merely passive entities, they are part of the production of knowledge and practice. Similarly, whereas earlier theorizing about relationships of power, hierarchies and repression tended to constitute those on whom power was enacted as passive recipients, in recent years an emphasis on individual agency – including a range of responses to new technologies, from a wholehearted embrace of them, to a pragmatic acceptance or rejection, to an ironical distancing – has replaced the former picture of a technological manipulation of subjects (Ginsberg and Rapp 1995; Lock and Kaufert 1998). In part because the subjects of technologies are themselves situated at intersections – of the medical world, individual interest, and family obligations, to name a few – their co-production of technological practice makes for an important part of the analysis. To this it should be added that while the transition from the embrace of medical technologies to a distancing has characterized work in both the anthropology of medicine and in science studies, several workers in the latter field (some represented in this book) have, by now, opted for a different approach. Instead of aiming at a critical

sociology of medical technologies, one, that is, that provides implicit or explicit criticism of those technologies, these writers' goal is to produce a *sociology of criticism* – a symmetrical analysis of the resources mobilized by scientists, clinicians and lay people to assess and use those same technologies.[9]

When analyzing medical, as opposed to other forms of technologies, another site of agency must, of course, be kept in mind, namely that of the material body. In keeping with a commitment to an approach that acknowledges the co-production of nature and society, technologies of the body are not, therefore, conceptualized as things-in-themselves by the book's contributors. Medical technologies are independent neither of the agency of scientists and medical practitioners, nor of the individuals on whom the technologies are practiced. Further, medical practice cannot be conceived independently of the material body of the patients. Here, however, different approaches can be, once again, adopted. Some scholars maintain that while the body has indeed been neglected by historians and sociologists of medicine and has thus to be "put back in place," that body has to be conceived of as a material, ahistorical, biological entity in which cultural practices can be grounded, but that is not in itself cultural. Other scholars, on the opposite end of the spectrum, see the body as yet another socio-cultural or discursive construct. Yet other scholars, think that both alternatives have to be rejected and that this can effectively be done by looking at the practices through which bodies are performed.[10] It is at these complex intersections that the majority of the authors of this book have worked to reveal how we all live and work with the new medical technologies.

Rayna Rapp highlights the gap between epidemiological description, clinical services, and individual and family understandings of illness, and argues that technologies of diagnosis, therapies of intervention, and systems of support are all enacted and interpreted in this gap. However, her essay which focuses on Down syndrome, more than any other in this collection, is concerned with how families perceive and talk about this gap. Like Kaufert, Rapp is sensitive to the unstable, expansive area of expert knowledge. She shows how identities of families with Down syndrome children are "resculpted" as a result of their exposure to support groups and the medical world, akin to what Rabinow (1992) has termed as "biosociality." However, she argues throughout her essay for "doubled discourses," in which scientific discourse is contested from various domains of popular knowledge and is dispersed unevenly into the lives of those directly affected by this disorder.

Joseph Dumit also analyzes how new biomedical technologies are articulated with the production of new biosocial identities, but, in his

case, by focusing on patients suffering from ''new socio-medical disorders,'' i.e. ill-defined, overlapping syndromes such as Chronic Fatigue Syndrome and Attention Deficit Disorder. The existence of these disorders is highly contested (for instance by the companies that some patients hold responsible for the onset of their disease) and, in an interesting twist, while social constructivist arguments are used by corporate lawyers and company experts to deny the reality of the disorders, new biomedical technologies such as brain imaging are seen by patients as a major tool for validating their claims. The production of new nosological entities thus takes place in a virtual space governed by controversy and inhabited by the courts and the media (including the Internet) as well as by doctors and patients, and the resulting realities are temporally and locally contingent.

In her chapter, Patricia Kaufert discusses the creation of clinics for the screening of two ''hidden'' diseases – cervical and breast cancer. Following Foucault's lead, Kaufert understands the idea of screening for disease as reflecting a particular view of health and disease, one in which the body, in particular the female body, must be thought of in need of constant monitoring and surveillance. Kaufert contests the idea of a truth in numbers, and shows convincingly how interpretations by epidemiologists and radiologists are made on the basis of vastly different perceptions of the meanings of numbers. Similarly to Rapp's doubled discourse, Kaufert writes about ''two conversations'' that take place simultaneously, but ''in different tones and languages.'' The first, about risk, cost-containment, survival times and so on, is rational. The second deals with emotion, faith, morality, fear and death. These two conversations can never be reconciled, and neither can one ever replace the other.

The chapter by Margaret Lock is concerned with the reconfiguration of the margins between culture and nature, in this instance life and death, as a result of intensive care technologies developed over the past thirty years. Her comparative ethnographic work in Japan and North America shows how in these two locations meanings attributed to braindead bodies are not the same, with significantly different outcomes for clinical practices, including the organ transplant enterprise in these two settings. Lock does not argue for a straightforward contrast between Japan and North America based on cultural difference, but presents a complex argument in which heterogeneity and disputes in both settings are recognized.

Veena Das traces the multiple genealogies – the set of practices – that result in the object of study for her essay, namely the transplant world in India. Das shows how the political representation of individuals is

co-produced with scientific knowledge, and she gives emphasis to how international discourse, with its rhetoric of a ''shortage of organs'' is reinterpreted in the ''local'' setting of India. On the basis of ethnographic data from India, in which it is clear that structural violence is implicated in the unequal distribution of and access to organs, Das is able to generate a critique of a bioethics grounded in the ideas of autonomy and individual rights.

The chapters presented here are the product of a challenge to communicate successfully across disciplinary boundaries. The reader may judge whether or not the authors have risen to the occasion.

NOTES

The Cambridge conference took place under the aegis of the Committee on Culture, Health, and Human Development of the Social Sciences Research Council, New York. In particular we wish to thank the Committee Program Director, Frank Kessel PhD, for his unwavering support and personal contribution to the conference. We are also very grateful to Diane Colbert for carrying out the arrangements for the conference on our behalf. The SSRC also provided funds to support this endeavor, as did the Wenner Gren Foundation for Anthropological Research, New York.

1. See, e.g., Strauss 1993, esp. pp. 39–40 and 233.
2. On this topic, see Rheinberger's notion of ''concatenation'' in Rheinberger 1997: 21–23.
3. For a fascinating discussion of ''increases in generality,'' a central topic of the ''new'' French sociology, see Boltanski and Thévenot 1991, and Boltanski 1990a, esp. Section III.
4. See, for instance, Barnes 1974 and Bloor 1976; see also Collins and Pinch 1982 as well as Law and Lodge 1984. By breaking the hegemony of US-based functionalist approaches over the sociology of science, the ''strong program'' effectively led to the establishment of the science studies field. For a stimulating analysis of ritualistic accounts of the demise of the ''old'' sociology of science and the rise of a ''new'' one, see Lynch 1993: chapters 2 and 3.
5. For early attempts to apply this approach to medicine, see Wright and Treacher 1982.
6. For those who appreciate Canguilhem's warnings against the ''myth of precursors'' in the history of science, it is particularly ironic to see Fleck labeled as a ''precursor'' of Kuhn; see Canguilhem 1968.
7. For a discussion of two possible readings of Kuhn's work, one that matches Barnes's reading and one that focuses on practice, see Rouse 1987: chapter 2.
8. For more on this issue see, for instance, the debate between, on the one side, Collins and Yearly and, on the other side, Callon and Latour in Pickering 1992.
9. On this issue see Boltanski 1990b.

10. These issues were the focus of a recent conference on ''Theorizing Bodies
in Medical Practices'' held in September 1998 in Paris and organized by
the Centre de Sociologie de l'Innovation (CSI) of the Ecole Nationale
Supérieure des Mines and by The Netherlands Graduate School of Science,
Technology and Modern Culture (WTMC).

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