# chapter 6 **doing theory**

It can be done. It is possible to write an ethnography of *disease*. This book shows that this is the case. It has presented a patchwork image of atherosclerosis of the leg arteries: a single disease that in practice appears to be more than one—without being fragmented into many. Thus, a body may be multiple without shifting into pluralism. So instead of tracing paradigmatic gaps, this ethnography-of-a-disease became a study into the coexistence of multiple entities that go by the same name. In its turn coexistence comes in varieties and takes different shapes. Here we have explored addition, translation, distribution (over different sites in the hospital, different layers of the body, and different moments in time), and inclusion. And if one begins to study the interferences between the enactments of two or three multiple objects (such as atherosclerosis *and* sex difference), then the complexities start to grow exponentially—though these are complexities to be investigated elsewhere, for this is the point where this study stops. It has done what it set out to do. A single/multiple disease has been described as a part of the practices in which it is enacted.

But what is it to do this? What is done along with it? The stories in this book do not finally unveil the truth about medical practice. Nor would I want to pose as a member of a small avant-garde of theorists who finally know what ontology is *really* about. None of this. Mind you, the stories assembled in this book are true and in as far as they are not, they need to be put right. And I take the theoretical apparatus mobilized and/or developed here to be worthwhile. But veracity is not the point. Instead, it is interference. Like any other representation, this book is part of a practice, or a set of practices. Attending to the multiplicity of the body

and its diseases can be done, or it can be left undone. It is an act. So in this final chapter I draw no final conclusions. Instead I briefly explore the act(s) this book engages in and point to some of those that it leaves undone.

*How Sciences Relate*

Shifting from understanding objects as the focus point of various perspectives to following them as they are enacted in a variety of practices implies a shift from asking how sciences represent to asking how they intervene. Over the past few decades many philosophers have stressed the importance of intervention as the dominant modern way of acquiring knowledge: epistemology lost its reverence for contemplation a long time ago. But, even if interference was important, interfering was not the point. The crucial issue in relating to objects was to get to know them. This book is part of a recent wave of studies that takes a further step away from disembodied contemplation. This means that it no longer follows a gaze that tries to see objects but instead follows objects while they are being enacted in practice. So, the emphasis shifts. Instead of the observer's eyes, the practitioner's hands become the focus point of theorizing.

Thus, this book contributes to a philosophical shift in which knowledge is no

*Method*

There is a large literature about method. Or rather there are three.

The first of these is of a legislative kind. It discusses how method should be shaped in such a way that the knowledge it helps to generate is *valid*. Valid knowledge should not contain the traces of the subjects who engage in knowing, nor of the situation in which the knowledge is articulated. It must be *pure*. No biases, no noise, should spoil a science's clear mirror image of the object. In this legislative tradition scientific knowledge should indeed be a mirror image of its object. The question of how this might be achieved is answered in a lot of different ways: very many legislative texts about method have been written. What holds this literature together is a quest after a method that is *good* in that it generates object-dependent, uncontaminated knowledge. (But what to refer to? There is too much of it. No single title representative. But see, for example, Suppe 1977.)

The second genre in the literature is critical. It undermines the first. It tells that those who join the quest after a sound method have so far not found it. Along the way the main effect of their attempts at legislation has been to demarcate science from other kinds of knowledge. Such boundary setting has helped to protect some communities, those that succeeded in calling themselves "scientific," against outsiders. A large variety of examples are presented—not of method, but of the way it fails to keep out *bias* even though it is socially effective in keeping out *strangers*. Thus, we have come to learn about the manifest sexism contained in twentieth-century medical textbooks (e.g., Dreifus 1978 ). And about the subtle sexism, too (Jacobus, Fox Keller, and Shuttle-

longer treated primarily as referential, as a set of statements *about* reality, but as a practice that interferes with other practices. It therefore participates *in* reality. And various other shifts follow from this. One of these is that we need to reconsider the character of the relations between the sciences. Since the nineteenth century the various branches of science (physics, chemistry, biology, psychology, sociology) have been understood as differing not primarily in method (as was earlier the case), but in their objects of study. These were given by nature. They hung together in reality and ontology was the branch of philosophy that made this coherence explicit—often using the image of the pyramid. Each object domain was like a layer in a pyramid of objects ordered from the small and relatively simple to the largest and the most complex. And each science had the task of studying the entities in one such layer. Thus, at the bottom of the pyramid the smallest particles and the force fields between them formed the object domain of physics, and at the apex the complex social relations between groups of people were to be studied by sociology. One of the dreams that went with this ontological monism was that, in the end, full knowledge about the behavior of the smallest particles would explain everything else. Physics would explain chemical laws; chemistry would predict what happens to living bodies; biology would be able to explain psychological makeup and social relations. Not everyone agreed with this picture. During the twentieth century considerable effort has been devoted to establishing the existence of thresholds in the ontological

worth 1990). And many stories have been told about the way in which midwives and others were marginalized in the nineteenth century, when their skills and knowledges did not come to be taught in universities and thus were not granted the predicate "scientific" (e.g., Böhme 1980).

The third genre in the literature not only abandons the quest for a sound method, but also the critical campaign against it. Instead, "method" is turned into an object of inquiry. A variety of questions is being asked about it—in empirical mode. There are historical studies that go into the question of how the experimental method that is still with us got shaped and how it happened that so much faith was invested in it (Shapin and Schaffer 1985). Others wonder why it was *method* of all things that came to stand out as the way of demarcating the scientific from the bogus (Dehue 1995). And yet other studies investigate scientific ways of working in an ethnographic mode: the sampling habits, labeling practices, ways of accounting, writing styles that may be found in present-day laboratories, offices, and scientific meetings. The knowledge that results from these ways of working does not mirror its objects. Do they fail to do so? But no. Mirroring is simply the wrong term. Passively rendering an object is not what science's systematic ways of working *do*. Instead, they actively constitute a traceable link between an object that is studied and the articulations that come to circulate about it. When

pyramid. Thresholds between dead matter and living organisms, which, unlike dead matter, can get ill and die. Thresholds, too, between biological facts about sex difference, skin color or disease, and social events that do *not* follow from these facts and therefore need to be spoken about in specific, social terms: gender, culture, illness.

In this order of things, knowing and talking about *disease* is both a task and the privilege of biomedicine. Chemists, even if they may know all about the molecules out of which cells are composed, cannot hope to explain the organism and its diseases. *Bio*chemistry is required, and it needs to include a pathophysiological branch. Medical practice meanwhile requires a further addition. For in order to attend to patients as *a whole,* biomedical knowledge of disease is not enough. The way people live with diseases should be attended to as well. In this way of thinking, "living with disease" was taken to be a psychosocial phenomenon called *illness*. Calls to attend to illness were often cast in critical language. Medicine was accused of prioritizing the physicalities of disease and neglecting its psychosocial aspects. But however harsh the criticism, it was built on a shared understanding of knowledge and the relations between the sciences, which was that knowledge is to be classified in terms of what it talks about and that these objects precede the knowledge. Body or mind. Disease or illness. Blood vessels or trouble with moving about. Biology or sociology.

However, if we come to the sense that knowledge is primarily about partaking *in* a reality, our understanding of the relations between the sciences also begins to shift. For whatever the relations between objects hidden inside the

moving from object to article we do not leave the material realm to enter that of theory and thought, but move, instead, from one sociomaterial practice (observation, experiment) to another (drawing, writing) (see Lynch and Woolgar 1990).

I separate out these three ways of relating to method here. They do not encompass all books that have been written on the topic—but leave some out that deal with different themes or ask different questions. And neither are the three ways separated here, separated out so neatly in libraries, at conferences, or in university departments. So there are fusions, gray zones, interferences. One of these is that criticism of current methodological legitimations (style 2) feeds into the design of new methods—to turn these into *better* methods (style 1). This comes with hopes, for instance, that if the white male gaze is joined by female and colored optics, unbiased knowledge becomes possible, and objectivity is reached after all (see for a variant of this Harding 1986). In an analogous way empirical inquiries into the way science is practiced (style 3) are mobilized as a resource in writings criticizing methodological pretensions (style 2). If "method" is just a local, practical achievement, it cannot offer a guarantee that the knowledge that comes out of it is true. But

body—atherosclerotic plaque, peak flow velocity, increased cholesterol level—the practices in which these objects exist are concerned with a lot more with expensive or cheap apparatus, blood or flesh, forms or conversations, work hours, self-esteem, or insurance schemes. Treatment decisions are informed by the length of a stenosis *and* the length of a hospital stay. In practice, such diverse phenomena do not belong to different orders. It makes no sense to delegate them to separate layers of reality. They are all relevant and have to be somehow reckoned with together. What different sciences have to offer practice is different points of leverage, different techniques for intervention, and, indeed, different methods. One specialism may have dyes at its disposal, another knives, and a third the technique of humming, but in hospital practice they must somehow align and coordinate their objects.

However physical an intervention, the practicalities belonging to the so-called social are always and inevitably implicated in it. That is not to say that they are handled well. The quality of handling disease/illness and the rest of the world in hospital practice has not been the explicit concern of this study. But if a critic wanted to criticize physicians for attending poorly to, say, patients' experiences, the present analysis suggests a different way of framing this criticism. The point is not that in such cases some object remains outside medical attentiveness. It is rather that some intervention receives insufficient attention when

this reflects back on the empirical study of science: its own methods hold no guarantees either. Then what makes *science studies* better than the self-interpretation of scientists, or lay opinion? What are the grounds for its own claims to expertise (Ashmore 1989)?

An important question, but not one that has to be posed in this paralyzing way. What turning method into an object of empirical inquiry has taught us is indeed that no knowledge is beyond critique. Another method might have lead to different conclusions. Thus, there is no longer a formal reason to go with this, that, or the other product of science, however sound its method. But this comes with another shift, which is that knowledge should not be understood as a mirror image of objects that lie waiting to be referred to. Methods are not a way of opening a window on the world, but a way of interfering with it. They act, they *mediate* between an object and its representations. One way or another. Inevitably. That means that it is not so surprising that the quest for a method for producing faithful representations took so long and that each time some critic was able to find biases that interfered with the objectivity of the results.

Studying methods empirically, then, generates another understanding of what they *are.* No formal guarantees, but specific mediators, interferences. The question to now ask is *how* they mediate and interfere. Donna Haraway has described an example that is illuminating in its exaggeration. It is a cage—a nuclear family apparatus—designed to study paternal love in monkeys. It was developed in the

medical activities are evaluated. In hospital Z, people with intermittent claudication are only considered for an operation if they report that their daily lives are seriously hampered. This implies that at this point operations are appreciated as a primarily *social* intervention. But this is not the case in studies that evaluate operations. Take the typical clinical trial comparing operations and walking therapy for atherosclerosis of the leg arteries. The list of parameters assessed will include "pain-free walking distance" but most probably not "actual weekly amount of walking," "changes in daily life," or "assessment of the intervention" in the patients' own terms.

How to attend well to the complex list of interventions that each medical activity entails? This question is left open here. But surely the first step is to consistently recognize that there are many entanglements in every action. To keep practicalities unbracketed. To treat everything in medicine as a practice. To engage in a praxiography. Praxiographic stories have composite objects. Disease is not different in kind to hospital stays or daily life. Each flows into the other. This means that the stories in this book are about disease itself just as much as they are about the practices in hospital Z that are intended to cure, alleviate, prevent, or investigate disease. The disease *as much as* the medical practices that intervene in it: the two go together. A microscope is used to look at plaque, while plaque, if it is to be practically relevant in a hospital, needs a microscope (and dissection, slicing, and staining techniques) to make it visible. Similarly, conversational skills (of both doctor and patient) and the complaint "pain when

sixties and seventies in the laboratory of Harry Harlow at the University of Wisconsin in Madison. Harlow first made "cloth mothers" and "bottle mothers" to test which of these offered the greater maternal love to monkey infants (who, faced with this awkward choice, preferred warm cloth over food bottles). Now it was the fathers' turn. "Each infant in the nuclear family apparatus, a planned social environment worthy of Disney Worlds, had access to the whole neighborhood, including his or her own father. 'Their parents, however, always remained home together' " (Haraway 1989, 240).

The nuclear family apparatus made it possible to isolate the variable "paternal love" as a specific behavior of male monkeys. This phenomenon wasn't available for study before the apparatus. The object wasn't lying there and waiting patiently. The apparatus delineated it. But if the monkeys hadn't responded so well, the use of the apparatus would soon have been abandoned. Did the monkeys respond well? "The fathers were nicely social with the babies and showed that they had a function in family life: threatening external enemies (experimenters mostly, Harlow recognized, in his always honest jokes)" (241). The nuclear family cage helped different observers ("experimenters") to make comparable reports. That was what it was made to do. But it did

walking" depend on one another. As do blood velocity and the duplex machine measuring it. And without the statistical calculations for extrapolating data from small samples there would be no at-risk populations on national scales.

This is why an ethnographic study may talk about disease. In the traditional ordering of disciplines, an ethnographer talking about disease transgresses the thresholds separating the layers of reality in the pyramid of objects. But the move made here is different. It is not a matter of turning the arrow round so that instead of the natural sciences explaining social phenomena a social explanation of molecules, cells, or bodies is being presented. Instead, another axis has been introduced, another approach taken: that of practice. The latter encompasses molecules and money, cells and worries, bodies, knives, and smiles, and talks about all of these in a single breath. Thus, it stands in an oblique relation to explanatory knowledge and the static pyramid of objects to which this refers. It approaches knowledge and object as parts of life, elements in a history, occurrences in strings of interrelated events. But no. To talk of an *oblique* relation is not quite right either, because this might seem to imply that the ontological pyramid, approached differently, is left standing as it is. But it is not. If practice becomes our entrance into the world, ontology is no longer a monist whole. Ontology-in-practice is multiple. Objects that are enacted cannot be aligned from small to big, from simple to complex. Their relations are the intricate ones that we find between practices. Instead of being piled up in a pyramid, they rather relate like the pages in a sketch book. Each new page may yield a different image, made with a different technique and in as far as a scale is recognizable, it may again, each time, be a different one. There is no fixed point of comparison.

more. It literally constructed the 1950s U.S. suburban nuclear family in a monkey version.

The point of stressing this is not to say observers should *not* interfere. They always do. In the same book Haraway beautifully shows how the ethologists who went to study primates out in Africa interfered as well. They pretended they were modest outside observers, who, by building no cages, left the reality of "their" primates untouched. But they made the animals *theirs* even so. They set up camps, appropriated the primates by giving them names in order to recognize them and communicate about them, arranged for them to get used to the observational presence of the ethologists, and so on. All this is not *bad* because it is interference. But it is interference. And the question of how to evaluate it shifts to a question of content. *How* does it interfere—and what to think of that?

Asking this question opens up a fourth and relatively new way of attending to method. A way that is normative again, and interested in the good: what is a good way of doing research, of going about the assembling and the handling of material?

The praxiographic approach allows and requires one to take objects and events of all kinds into consideration when trying to understand the world. No phenomenon can be ignored on the grounds that it belongs to another discipline. This doesn't make description easier. And since not everything can be held together in a page or two, other ways of delineating the world have to be found. Of course, there are many candidate traditions. In the present book I have built mainly on ethnographic techniques of observation and writing. But in various traditions of writing history, events have also been described with all their sociomaterial entanglements. It is no accident that history fails to fit into the ordered list of sciences where each is responsible for a slice of the ontological pyramid. History has always taken another entrance into reality. Another quite different but equally interesting resource for praxiography is found in the *material and methods* sections of scientific articles. In theory these specify as much as possible about the practices of investigation. They instantiate the recognition that the practices forcing an object to speak are crucial to what may be said about it. This recognition not only exists in written form but also resonates in interesting ways with the day-to-day self-reflection of medical professionals: a further resource for praxiography. In hospital Z, the death of a patient was always followed by a discussion in the staff meeting. The responsible doctor was required to describe the train of events that led to the patient's death. In this story, no particular "layer of reality" was privileged over any other. Deviant cells figured next to deviant dripping fluids; unexpected allergies next to the failure to check for them; heart problems were talked about in a single breath with names and

This time, however, the register in which *the good* is being played out has changed. Knowledge is no longer good in as far as it faithfully represents some object *as it is*. Objects do not slide silently, untouched, from reality into a text. Instead, there are cages or chairs, there is touching, asking questions, cutting up continuities, isolating elements out of wholes here, and mixing entities together a little further along. The new normative question therefore becomes which of these interferences are good ones. And when, where, in which context, and for whom they are good. Good knowledge, then, does not draw its worth from *living up to* reality. What we should seek, instead, are worthwhile ways of *living with* the real.

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

doses of drugs administered. If any actor was most central to these stories it was not the sick body but the speaking professional, for the leading question invariably was what, if anything, he and his colleagues might have done better.

The distinction between knowing *in* medicine (about disease) and knowing *about* medicine (that is about its practice) is blurred, not just in praxiographic studies like the present one, but also in historical studies, in material and methods sections, and in the hospital itself. Objects enacted and practices of diagnosing and intervening belong together. They are intertwined. They jointly differ from other object/practice constellations. The concomitant relevant axis of difference in the sciences, then, is no longer between the social and the natural sciences, or, more specifically, between *classes of objects* and the sciences referring to them. Instead, the axis of difference needing further exploration is between *versions of objects* and the (science-related) practices in which they are enacted. If a disease like atherosclerosis is more than one, it becomes relevant to ask which one "it" is made to be. Which one of its various versions is enacted at any specific site or in any particular situation? Is it an X-ray picture and the atherosclerosis that encroaches the arterial lumen; or is it a patient history and the atherosclerosis that gives pain-on-walking? Are surgeons operating on clogged vessels or are patients engaged in walking therapy encouraged by their physical therapist? This, then, is the crucial question in a world where ontology is accepted to be

move to the shelves that tell about the politics of academic work. Here I won't relate to that literature as a whole, but confine myself to what is on a single shelf. The shelf with the books that reflect on the effects of writing styles. (There is a lot on this shelf! But see, for example, Bazerman 1988; Trinh Minh-ha 1989; Clifford and Marcus 1986.) In different ways these three books tell us that what we are doing when writing academic texts depends not only on how the material is assembled. At least as important are the ways in which it is processed, rendered, mobilized. Written.

Is nature made to speak, or is a *materials and methods* section put somewhere prominently? Is "a culture" presented as if it existed out there, independent of the ethnographer who happened to come round to study it, or is it made clear throughout the writing that the stories told depend on scenes the author was a part of, even if it was only as an observer? Is the *subject* of writing staged as an observing outsider present in scenes she turns into "material," or rather as someone who approaches the field with fascinations, passions, and theoretical baggage that deserve a lot more attention than they get in methodological attempts to rule them out? (For general anthropological work, this is explored in, for example, Okeley and Callaway 1992. For a good example of what this may mean in science and technology studies, see Law 2000.) And what difference does it make if one presents one's study as a detective story, not just by using metaphors like "discovery" and "finding clues" but, more elaborately, by bringing

multiple: what is being done and what, in doing so, is reality in practice made to be?

*Doubt*

When I presented drafts of my articles or chapters of the many drafts of this book to my informants they were pleased when they recognized themselves and each other in my stories. Sometimes they suggested small corrections at points where I hadn't properly understood some technicality. Sometimes they nodded: yes, this is how it goes. But they also said they felt alienated. Somehow I made the familiar sound so *un*familiar to them. So strange. And yet one might say that hospital Z was not just the place where I assembled my material, but also a place where I learned a lot about the theoretical insights that I have presented here. For instance, the most concise way of articulating the idea that objects enacted depend on practicality was suggested by the resident who was my informant in the department of pathology. It was he, not me, who qualified his "this, here, is atherosclerosis" with the crucial *under the microscope*.

In hospital Z it happens time and time again that the practicalities of enacting a specific version of atherosclerosis are underscored. For instance, the techni-

this narrative plot to the fore and playing with it (as is done in Latour 1996)?

Texts are active. And they do so much. One cannot possibly engage in an explicit and articulate way with all of these activities in detail in any one text, all the more so if the text has something else as its core topic. Here, therefore, I've picked out a single stylistic characteristic to attend to. All academic texts somehow relate to the literature. The question I've posed to myself, and you, throughout this book, is how to do this. *How to relate to the literature?* By inserting a title. By presenting a quote. By relating a story. By giving one's text its place among others.

*Rationality*

When research is presented as requiring *method* in order to result in valid knowledge, the analogous recommendation for practice is that it must become more *rational*. In a variety of ways this has been claimed and propagated over the past few decades: medical practice is too messy and in need of purification. The irrational should be washed out of it. There is a large literature about *how* to do this. Its quest for rationalization comes with the hope that scientific order can come to rule practice. There is a second literature arguing that rationalization shouldn't be strived after. Neat ordering isn't possible since practice has a specificity of its own, different from that of science. A third literature investigates *what exactly alters* when rationalization strategies are actively put into practice. It takes "science" as a set of practices as much as "practice" and wonders what happens in the interferences between different working styles.

The present study is intertwined with, or could be read as a part of, the third kind of literature. It helps to undermine

calities in *materials and methods* sections of articles also get a lot of attention in research meetings. "But in how many patients did you find that?" Or "Why did you measure pressures only in rest and not after exercise?" Or "What did you say you used as a calcium antagonist?" For the participants in the research meeting it is a truism that the outcomes of a research project follow from such details. They shape the facts. As long as there is reason for or an occasion to doubt, the technicalities of an investigation are kept in focus. As long as various roads may still be taken, the entire trajectory so far is kept into view. It is only once outcomes are accepted as facts that the methods by which they were reached are, at least for the time being, abbreviated, allowed to fade out, forgotten. The two movements seem to go together: the consolidation of a fact and the bracketing of the means of its production.

In the diagnostic process something similar occurs. If a doctor doubts the diagnosis of a colleague, then questions about technicalities are raised. "But *how* did you ask when this pain occurs?" Or "Your pressures are odd: are you sure the arteries weren't calcified?" Or "Who the hell decided to make an angiography of this patient?" Once an indication for treatment is written down, however, once there has been a conversation with the patient about it, and once the treatment is scheduled, such doubts tend to evaporate. It is on to the next task. A crucial bifurcation point is passed, the past is closed off, the practicalities of diagnosing are erased—all that remains of them is their results and a plan for treatment.

the presumptions of the other two, which both differentiate between scientific order on the one hand and mundane practice on the other. The praxiographic way to go about these issues is not to propagate rationalization strategies in general terms, neither is it to warn against them in equally general terms. Instead, it is to investigate what they bring along. What they do. It is to open up the question of *how* rationalization strategies alter what they interfere with. There are a lot of ways to handle this question. Here, I will present you with just a few examples of this third approach. They come from different places and each bring their own concerns with them but all tackle the question of the improvement of health care.

The first book on my little list is *Health and Efficiency: A Sociology of Health Economics* (Ashmore, Mulkay, and Pinch 1989). It pays a lot of attention to the question of how *health economics* manages to present itself as rational in the first place. How does it stage its capability of improving the way decisions in health care are being taken? How does it present itself as being able to help increase the (market) quality and decrease the (financial) costs of health care—all in one go? The authors state that the economists' claims of expertise are strengthened by their shifting between two versions: a strong one (that holds big promise and suggests that if its own economic rationality was obeyed things would get better) and a weak one

"Obstruc. fem. art. left; bypass to below the knee" or something like that. If, however, something unexpectedly goes drastically wrong at a later stage, it is almost always possible to go back in time. To take out the photos and have another look at them. To search the file for small traces that were missed earlier on. The treating physician who traces a history after someone has died is likely to look back into the past in this way. Was there a moment when we were sure about something, but we should have stayed a while longer in doubt?

Attending to practicalities also happens when some treatment is doubted. It helps to make space for other possibilities. An internist critical of operations on leg arteries said to me in an interview: "They [the surgeons] look at these angiographic pictures and come to think that they can *see* atherosclerosis. There it is: a pipe that is blocked and they need to unplug it." And then he added: "But by staring at an angiographic image one would never invent walking therapy." The image of the pipe that needs unplugging makes atherosclerosis into something unlikely to be improved by walking therapy. After all, walking does not unplug the pipe that looks so stenotic on the angiographic image. In an attempt to raise doubt about the necessity of surgery, the internist tries to undermine the reality effect of the angiographic images. Do not think that it is *atherosclerosis* you see there, but keep the specificities of the imaging technique in mind.

In this book I have argued that different practicalities of research, diagnosis, and/or treatment each address a slightly different "atherosclerosis." This idea is not alien to the hospital: I may even have learned it there. But there is a differ-

(that can be fallen back on in case of resistance: we know there are other matters to take into account as well). The authors analyze the contents of the economists' expertise as well. They look into the specificities of option appraisal, clinical budgeting, and the evaluation of interventions by assessing their (positive or negative) effects on people's quality of life.

Meanwhile the authors self-reflexively attend to their own claims of expertise. What is it to present one's stories as *knowledge about* health economy? In their desire to be serious about establishing a symmetry between economic expertise and their own sociological expertise, Ashmore, Mulkay, and Pinch have written a book that is full of mockery. (At this point I must insert a remark. An aside. However much "writing" has become a topic that is theoretically discussed, there still aren't many books that *do* something to enrich, complexify, and change academic writing practices. Writing methods are still not taken as seriously as methods of gathering and analyzing material. *Health and Efficiency* is among the few exceptions. It brims with conversations, shifts in scenery, alternative presentations of material, self-reflexive remarks, and jokes. How to relate to that? In awe or with mere admiration?) So. So claims of expertise are robbed of their foundation.

The issue is not that health economics

ence. In hospital Z the other repertoire exists as well: that of bracketing practicalities. That of speaking about atherosclerosis *tout court* without mentioning microscopes, interview techniques, angiographies, or any other modality of enacting the disease. Of atherosclerosis in isolation. At such moments what one might think of as a virtual common object is projected into the body, an object that is hidden underneath the skin. An object that may be approached in various ways, that shows a variety of aspects, but that in the end is one. There it is, and suddenly it no longer seems to be a part of practice, but a referent in a preexisting reality: overwhelmingly real. At such moments doubt is smothered and certainty is being manufactured. "But surely we are all fighting the same disease? We share a goal, don't we? Obviously we all want to improve the health and lives of our patients." At such moments someone might say (to me, for instance, in reaction to this text): "But listen, people die, people suffer. There is a *real* disease out there." As if the certainty of death and misery necessarily brought with it the singularity of the real.

So in the hospital there are, at least, these two repertoires. Keeping the practicalities of enacting disease visible so that what happens may be doubted, *and* bracketing practicalities while working along and being confident in doing so. Making space for other enactments of reality, other versions of the disease to

should seek a better foundation from now on. "No, it is not the epistemological status of applied economics in any abstract sense that concerns us but rather the specific moral and political implications of its underlying assumptions" (187). If the authors have problems with health economics, its lack of rationality is not among these. The point is that in various instances interferences are made that could have been made otherwise. Had this been so, other outcomes would have followed. These are problems of *content*. An example. The QALY is a *quality-adjusted life year*. It is added to earlier epidemiological assessments that measured only survival. The addition is that the *quality* of the years patients survive an intervention are taken into account. But how? The QALYs obviously do so in a specific manner. One that allows accounting. One that fits into the forms of quantitative studies. One that supposes that "aggregate data on preferences correctly represent the individual evaluations from which they originate" (192). Ashmore, Mulkay, and Pinch point out that sociological investigations into people's appreciations of their life could also proceed differently.

However, Ashmore, Mulkay, and Pinch do not develop an alternative health economics. They cast doubt since their primary concern is with the arrogance with which economic expertise is presented as lying beyond doubt. A predicate of scientificity is used to close off discussion. The economists put themselves *above* the practice they aim to improve. An extensive quote here, for Ashmore, Mulkay, and Pinch have put it in a way that asks to be quoted. (In relating to the literature one comes across this style characteristic:

be diagnosed and treated, or closing off alternatives so as to move ahead on a given track. Doubt and confidence: in the hospital they alternate. My informants know how to shift between them; I abstain from doing so. This suggests that the strangeness of this book lies not in its novelty, but in the persistence with which it never comes to rest in a sure, single, mortal body, but keeps on pointing at the practicalities of living. Being so stubborn is like remaining in doubt. An analysis like this opens up and keeps opened up the possibility that things might be done differently. Look, they *are* being done differently: a little further along. If something is self-evident here, then in that other site and situation it is not. If atherosclerosis is a thick vessel wall here (under the microscope), it is pain when walking there (in the consulting room), and an important cause of death in the Dutch population yet a little further along (in the computers of the department of epidemiology). Reality is varied.

In stressing ontological multiplicity this book lays bare the permanent possibility of alternative configurations. The doubt that might lead there isn't always practiced, but it *may be*. Medical practice is never so certain that it might not be different; reality is never so solid that it is singular. There are always alternatives. There is no body-isolated that may offer us a place beyond doubt. But this means that no version of atherosclerosis should necessarily be practiced "because the body itself leaves us with no alternative." Bodies enacted are being

some texts are *quotable,* while others, even if they are well written, are not.) "Efforts at reform and change must, and will, continue. Applied social scientists of all kinds will continue to make a major contribution to these efforts. And as they do so, they will, like the health economists, be faced with the fundamental problem that the very practices they wish to alter will tend to frustrate their efforts. The point we wish to emphasize is that confronting this 'problem,' if it is understood in the way we suggest in this book, is the essential first step towards a better form of practice (if we may be permitted such a blatant evaluation): one that consists of a willingness to work with, rather than against, the actors in the domain of application; one that is collaborative rather than imperious; modest rather than megalomaniac; and wishing to learn rather than itching to instruct" (195).

This literature link is strong. The present book presents a very different study, but it leads to the same conclusion (or has this been one of its driving forces, a conviction that informed this study all along?). If there are so many *rationalities* in practice, in the plural, mixing with one another, interfering, then why present oneself as an outsider, who, with a single mode of ordering, may change everything? Why do so as a rationalist, a radical, a revolutionary, a rightist of whatever kind? The tenacious character of such hopes is all the more surprising when one looks at what happens, in practice, with rationalist schemes when they are introduced to a specific site or situation. It never happens that everything gets subsumed under the newest head-

done, which means that they cannot answer the question *what to do*. However uncomfortable this may be, this question, what to do, is a question *we* have to face. Not in circumstances where anything is possible, but still. Reality used to be a standard to live up to, but given the proliferation of technoscience the question that now needs asking is "what reality should we live with?" That means that reality moves. It can no longer play the role philosophy cast for it a few centuries ago, the role of something to get in touch with. The role of something to grasp. To hold on to. To be sure about. The crucial philosophical question pertaining to reality was: *how can we be sure?* Now, after the turn to practice, we confront another question: *how to live with doubt?* It isn't easy. But somehow we must come to terms with the fact that we live in an underdetermined world, where doubt can always be raised. Somehow we must learn to understand how it is that, given this possibility, we can still act.

This, then, how to live with doubt, how to live in an underdetermined world, is another question that this book leaves open. However, part of the answer lies in shifting repertoires when considering action. If the question *what to do* no longer depends on *what is real,* then what else might it be linked up with? I suggest that if we can no longer find assurance by asking "is this knowledge true to its object?" it becomes all the more worthwhile to ask "is this practice good for the subjects (human or otherwise) involved in it?" If faithful representations no longer hold the power to ground us, we may still seek positive interventions. Thus, instead of truth, goodness comes to the center of the stage. Or rather,

ing. Instead, one more mode of ordering is added to the many others that are already there. This is what we learn from the next book on my list: *Rationalizing Medicine: Decision-Support Techniques and Medical Practices* (Berg 1997). This book tells a few stories. A first is that rationalization strategies may claim to *improve* medical practices, but the standards by which good and bad, and thus "improvement," are assessed do not precede them. They are, instead, framed in the process of developing and introducing the rationalization strategies—with which they are inextricably linked up. A second story told is that the opposition between a messy practice and a single rationality that comes to its rescue does not hold because there are serious incongruencies between the various rationali*ties* involved. Computer-based diagnostic tools incorporate a quite different rationality than clinical decision analysis. Protocols are different yet again, and so are expert systems.

And then there is a third story in this book, one that says that when it is introduced into a practice an ordering device does not *expunge* messiness, but shifts it. Pushes it along. An expert system, for instance, may solve some problems, but creates others. It may suggest useful interferences between the data it is fed with and a diagnosis, but it obliges the people working with it to feed it with data and

not *goodness,* as if there were only one version of it, but *goodnesses*. Once we accept that ontology is multiple and reality leaves us in doubt, it becomes all the more urgent to attend to modes and modalities of seeking, neglecting, celebrating, fighting, and otherwise living *the good* in this, that, or the other of its many guises.

### *A Politics of Who*

The recognition that medicine is entwined with *the good* has led to the call for what is sometimes called "patient autonomy." Rather than professionals, "people themselves," "patients," are to decide what is good for them. Their norms are to be given weight. They have to make the judgments, and the role of professionals is simply to present patients with the options. Patients choose. A large industry (of literature, conferences, and committees) has grown to specify how to implement this requirement. What if the medical ideal of benefiting people clashes with the ideal of granting them autonomy? Are there moments when a professional should step in and decide for a patient? What to do with patients who are not capable of expressing their will in an articulate manner? Ignoring the complexities of such issues here, I want to stress that the growing interest in medicine's normativity has predominantly focused on *who* questions. Questions about *who* is being put, or should be put, in the position to decide what counts as good.

There are, roughly speaking, two ways in which "patients" are put in the position of making supposedly crucial normative decisions, two ways of living out a right to choose. The first is that of the market. Here, medical interventions are

then adapt these where they do not fit. What, for instance, if the system wants one to locate pain in the back or the front, while a patient tells about pain that moves from one place to the other? Practitioners working with systems that want to be fed with discrete information must constantly negotiate their fluid findings. And it is, finally, story four that tells that while decision-support tools claim to simplify practice, in fact they do not do so. They introduce, and thus *add on,* a further logic to those that are already there. Something that is likely to complexify practice yet further. This is not an argument against decision-support tools. A hammer may also complexify building practices and yet be a welcome extra tool. The question to pose, however, is what it might imply for designing tools. How, or so Berg asks, to build tools that help to improve practice, without fantasizing complexity away? Again a question that resonates with the stories I've been telling here. Improvement and rationalization are not quite the same.

The third book on my list is a sociohistorical case study. The case studied is that of the *clinical trials* set up to asses the value of drugs against the human immunodeficiency virus in the United States.

displayed as if laid out on a counter. Professionals turn into sellers who supply a product plus the information that allows the patient-customers to choose between the products on offer. The patient is to make the value judgments—even if to some extent everything on offer in a market is, by definition, a *good*. In a market, goods that are entirely worthless are supposed to disappear; there is no demand for them. Even so, the health care market is heavily controlled. Professionals are required to be licensed and to check the quality of their own and each others' products. Although in actual markets money is central, the crucial element of the market metaphor in the context of medicine is that the individual patient, being the customer of health care, is the actor of an individualized choice for or against some "care act" or isolated "intervention." An ideal patient-customer is able to find the goods that fit his or her specific needs and situation.

The second genre of handling choice is civic in character. Here, medical interventions do not figure as goods on a market, but as policy measures. They are interventions into modes of living—configuring professionals as kings rather than hawkers. The civic metaphor tends to turn the patient into a citizen who deserves to have jurisdiction over the interventions into his or her own body and life. Decisions have to be singled out, and the patient must then be able to argue civicly for one course or action or another. But the civic metaphor doesn't necessarily argue for individual choice. Intervening in one life, after all,

The book is *Impure Science: AIDS, Activism, and the Politics of Knowledge* (Epstein 1996). It underlines the fact that clinical trials depend on the cooperation of many —the patients not the least among these. In the trials for drugs against HIV, some of the exigencies built into local definitions of good universal science were incongruent with the way most patients perceived their own interests. In the United States, getting enrolled in a trial may be of direct interest to patients because it provides them with free treatment. Moreover, being enrolled in a trial was often the *only* chance of receiving an antiviral treatment at all. But patients were obliged to refrain from taking drugs other than the one being tested in the trial. For someone with AIDS who has opportunistic infections, this is an unreasonable demand. Epstein describes how ACT UP, a movement of patient advocates, came to voice this and similar issues. First, they made their voice heard in angry protests against the way trials were designed. And then, as a next step, they were invited to come and sit on the committees that designed the trials.

The designs were adapted. At various points there appeared to be elements to contest. The question of who was included: at the outset participation was limited to (mainly white) males who had been infected through gay sex. It was only after ACT UP protests against this that first women were included and then drug ad-

also influences others. This brings with it the requirement that individual decisions should not harm others. But where does harm start? If one person chooses to have offspring through in vitro fertilization, this alters the meaning of being childless for others; if one would-be parent chooses to abort a fetus with Down syndrome, this touches on what parenting a child with Down syndrome does to others. To account for the effects of policy measures into a single patient's situation on the situations of others, the civic metaphor has been further developed. Interventions are understood as a way of organizing not just individual life, but that of the entire *polis,* the *body politic*. They concern us all, as patient-citizens. This implies that in the civic version of the *politics of who,* "patients" must represent themselves whenever decisions (about the organization of health care, allocation of money, research efforts, and so on) are being made.

The market genre and the civic genre have a common concern with the question of *who* decides. Both genres are informed by the same suspicion of professionals who patronizingly decide what is good for the rest of us. Ethicists together with social scientists investigating health care have contributed greatly to the articulation of this suspicion and have stressed the importance of the question of who decides in medicine. They have contributed to a politics of who. But

dicts (more often of color). The rules about taking other drugs were altered; with some adaptation of the statistics used this could be done. Then there was another intriguing issue: what should be taken as a parameter to mark the success of treatment? The primary choice of the epidemiologists had been to count the number of deaths in the treated and in a control population. But when survival became a little more prolonged it was argued that this was too slow. An intermediate parameter, a T-cell count, was chosen, a measure that was later discarded again. An appropriate parameter was difficult to find. What is interesting in the light of the present book is that in this specific case it became clear to all those concerned that what makes a parameter *appropriate* is a complex question. Statistical issues, the immune system's behavior, patients' hopes and expectations, health care finances, the pharmaceutical industry's research style, government regulations—all of these are intertwined. The loudness with which the various elements are heard may differ depending on the specificities of the situation. And in this case, a patient-advocate movement willing and able to engage with the details of the science involved was crucial.

There is still a lot to learn about such engagements. What they require, for instance, is that the professionals involved allow themselves to be addressed, that they listen to what others have to say and take it as an argument to be included in the accounts. What they also require is that the "others" in question are able to mobilize the arguments with which to engage in such "addressing." Epstein stresses that the ACT UP people were highly educated —if in different fields. They didn't take long, moreover, to educate themselves in clinical epidemiology as well. This is not

such a politics of who has some problems. The first is that, although the customer and the citizen may be protected against such things as monopolies of suppliers or the power of the state, their will and their desires are supposed to be set, predetermined, and clear. The analyst takes the position of a lawyer for the patient movement whose task it is to make space for the patients' silenced voices. But the position of the lawyer is not the only possibility. What if the analyst takes the position of the patient himself or herself? Then it may well be that other matters become important. For instance, "how might we gain the right to decide" may be displaced by the at least equally urgent question "what should be done?" What might it be good to do? What might the good *be,* here and now, in this case or that other? The problem, then, is that in trying to give "the patient" a say, a politics-of-who remains silent about what, if one is a patient, one might actually say at the crucial moment.

The second problem with a politics-of-who is that it isolates moments when a choice is being made. It separates decision-making moments from the series of long layered and intertwined histories that produce them, as if somehow normative issues could be isolated and contained within those pivotal points. As if they were, indeed, pivotal points. Take the situation of a consulting room where a decision is being made about whether or not an operation would be a good thing for the patient who has come to seek help. A decent doctor would explain quietly about what is wrong with the patient's arteries and about the pros and

a story in which assembled experts were confronted by individual lay people who were only knowledgeable about their own case. ACT UP activists draw their insights from many people involved. They brought their own expertise along. Expertise about the daily lives of patients, to begin with. This allowed them to help build interferences between the daily lives of patients and the exigencies of doing clinical epidemiology research.

Thus, however much Epstein's story starts out from a sociological curiosity about the way lay people came to speak *inside* science, the lines of difference tend to be more complex than lay/professional divisions. For epidemiologists who had been involved in cancer research, for instance, the ACT UP points were less alien than for those who so far dealt with acute infectious diseases. The committees designing trials welcomed the expertise on patient's concerns and daily lives ACT UP brought along. Without this they knew they risked setting up studies nobody would want to participate in. And ACT UP people in the end became so involved in the clinical epidemiology that they in their turn found themselves confronted with outsiders in the movement speaking out in the name of daily life. So the *who* question weighs heavily in Epstein's sociohistorical account. He persistently asks questions about who speaks and who does not. But what Epstein also makes clear is that once they were listened to, all those in-

cons of an operation. But to concentrate on *this* situation hides many others. For instance, that—at least in the Netherlands—the patient is present without having to think about the costs of various diagnoses and treatments. Or that no structured walking therapy was ever offered and that, despite huge investments, no drug is so far good enough (maybe the grant application that would have led to the drug was turned down—so what about *that* decision?). Or that some other hidden factor made this patient's atherosclerotic process go so far as to give pain on walking, so why wasn't that process ever intervened in? Or how come the patient had not considered this pain (as others might have done) as simply a part of getting old? Every single moment always hides endless contingencies—which, if we look at them carefully, are likely not simply to be contingent. That means that most elements relevant to making or unmaking the *goods of life* involved in making a decision escape the moment of that decision.

The third problem with a politics-of-who is that it is designed to push the power of professionals back, claiming some choices, and then more and yet more, for patient-customers or patient-citizens. But this same politics of who has trouble getting inside professionalism. It does, after all, grant professionals the facts. It requires of them that they give information—as if, from the beginning, there were a neutral set of data to lay out on the table. But there is not. My informants in hospital Z would stress that however much they tried to give "neutral information" they always found that the way they presented the facts made an impressive difference in how these facts were evaluated. But there is

volved, professionals and lay, preoccupied themselves primarily with another question: *what*. What is important, what should be done? Actors who have gained rights to speak no longer worry about getting heard, but wonder what to say. Maybe it is a matter of time: one question is not in tension with but follows after the other. If so, I would like to mobilize Epstein's book here as a support for a claim. This claim. It is time, in health care, to assemble and develop the theoretical repertoires needed for a politics of *what*.

*Locality*

Where do texts come from and where do they go? What place or places do they carry along or within them? If we think of the present book, this question comes in various forms. One is that the material mobilized here may be situated as stemming from what in *anthropology*, despite energetic debate, is still called a culture. The way professionals and patients described here behave, calmly carrying out conversations, for instance, could be called *Dutch*. And so could the primarily clinical orientation of "my" vascular surgeons, highly consequential for what I say about the character of medicine. A second, *sociological* typification of the provenance of the material I have explored would be quite different: many sociologists would say that the object that I describe is micro. It is local in

more. Which facts should be presented? Which facts are pertinent to the reality of atherosclerosis: those of pathology or those of the clinic; hematological or epidemiological facts; duplex graphs or angiographic pictures? This is not simply a matter of which textbook page to turn into a nicely illustrated, suitably didactic leaflet. It is also a practical issue. Which machine to put to use, with what money to pay for it? Which hurts to evoke and which casualties to risk? Information, presenting some version of reality, does not come after practice. Neither does it precede it. Instead they are intertwined.

This book does not try to show that *the social* is larger than we took it to be while *the technical* is smaller. Instead, it suggests that technicalities themselves, in their most intimate details, are technically underdetermined. They depend on social matters: practicalities, contingencies, power plays, traditions. Thus, technicalities should not be left to professionals alone. They affect us all, for they involve *our* ways of living. But this does not mean that they are not also technicalities. That is why the present book does not try to push back the role or the power of the medical professionals a little further by revealing further patches of the medical domain where values exist alongside facts—and where, therefore, laypeople should make the decisions. What if values reside inside the facts? Then it may be better to stop shifting the boundary between the domains of professionals and patients and instead look for new ways of governing the territory together. But this suggests that ethnographers, philosophers, and soci-

the sense that it comes from somewhere small. The big picture is not sketched out. The social organization of health care, long-term developments in the biomedical sciences, the distribution of power, the flow of capital, what have you—all such macrophenomena escape the microsocial framework of the book. And then there is a third possibility. The *philosophical* tradition situates texts differently yet again. It does not link them to their places of origin, but rather to their destination. True philosophy, or so the dominant tradition suggests, comes up with *universally* valid theories. These transcend the specificities of any single site and move everywhere, without transportation costs. And since this book has generated no universalities, it would be said to fail as philosophy. If it deserves to be taken seriously at all, it is as *mere* social science.

So we have three different modes of localizing. Let's look at them in a little more detail. First, then, culture. There is the question of the so-called cultural specificity of the events that take place in hospital Z. Are they distinctively and locally Dutch? One of the reviewers of this book, a North American, kept on insisting on this. With an ocean between us, he or she saw *Dutchness* running as a thread through every page—and challenged me to acknowledge this. So what to say about this? First, yes, there is a topic here. But second, it is one that deserves its own investigation. What might *Dutchness* be? In the local bookstore I found a book on the topic, a book that draws together a

ologists of medicine as, or just like, patients need to explore and engage with professionalism. Once inside the hospital, the *who* question is linked to, or even overshadowed by, *what* questions. There, time and again, the question to share is: *what to do*. What to do, this is a question *we* face, and the "we," or so I would want to argue, should be taken as widely as can be. But what kind of resources do "we" need if we are to face this question? Framing languages and shaping practices for dealing with the question "what to do" is part of a *politics of what*.

### *A Politics of What*

For the medical profession, *what to do* has always been an important question, indeed recognized as having a normative dimension. However, the norms involved were naturalized. Saving lives, improving health—that was what medicine set out to do. The value of life and health was deemed to be given with our physical existence and beyond dispute. When patients may die of pneumonia if it is not treated and survive when an antibiotic is prescribed, no further questioning of the normativity of such treatment seemed necessary. And if insulin postpones the imminent death of patients with diabetes for decades its *goodness,* again, is accepted as obvious. When it is clear that the overall health of the population improves when people do not smoke, then warnings are printed on

lot of anthropological field studies done in the Netherlands. Not coincidentally, it is a book in Dutch (van Ginkel 1997).

The book situates the beginning of the anthropology of the Netherlands (beware: *Holland* is only a province of this country!) in an American text. It is a text written by Ruth Benedict in 1944 while at the Office of War Information in Washington. Dutch anthropologists had been active for decades in Dutch colonies, studying villages in Java, irrigation in Bali, rituals in New Guinea, and so on. The aim was to bring such places closer to administrators, merchants, and planters. But there was no need to bring home *itself* closer—for the Netherlands were nobody else's colony. But then, in the Second World War a lot of U.S. soldiers were to be stationed in the Netherlands. In order to reduce friction between the soldiers and the Dutch population, each group had to learn about the other. And this is why Benedict assembled whatever written material about the country she could lay her hands on and sent out students to interview Dutch immigrants. With this material she wrote an exposé of *the Dutch character,* not so much stressing our obsession with clean houses (something travelers had remarked on over the centuries) as the self-assuredness of the Dutch. The Calvinist majority in particular, Benedict wrote, is convinced that it has Right on its side. Quote: "One can fairly say that the typical Hollander is so sure of himself that he does not submit to dictation. He stands up for his rights. He hates sentences beginning 'You must . . .' A so-called true story illustrates the Dutch attitude: The postmaster asks a little boy at the stamp window, 'What must you?' (a colloquial phrase). The little boy answers,

packages and doctors tell us not to smoke. Medicine never concealed its normative character. But its self-reflection was not directed at its central goals: postponing death and improving health. It became the profession's central concern, instead, to see if its interventions indeed helped to achieve these goals. Since roughly the 1950s more and more *clinical trials* have been conducted to evaluate which medical interventions succeed and which others fail to bring about improvement. Clinical trails have become the dominant mode by which the value of interventions is judged by the profession.

Trials, however elegant instruments though they may be, are not sufficient if we are to engage in a *politics of what*. They were designed in a time when, indeed, the goals of medical intervention were taken to be given with the natural characteristics of the body. Survival and health. At some point these goals have proved to be insufficiently specific. The first difficulties arose in cancer research. As long as "survival" is accepted as a goal, a treatment for cancer may seem successful if those who receive it live, say, an average of six months longer than those who do not. However, the patients and the physicians and nurses engaged in their day-to-day care weren't always convinced that such "survival" entails an improvement. Six months in and out of a hospital, with a disintegrating body, with pain from both disease and treatment, may well lead to more suffering than relief. In the discussion that followed, the goal of "survival" lost its self-evidence. Maybe it wasn't a natural good after all. Maybe the extra life treatment may bring was only a good if it was spent well, if the months gained

'I must nothing. But you must give me a stamp of two cents' " (Benedict 1997, 226).

So maybe it is no wonder that I show resistance when a reviewer presses me to attend to the Dutchness of my text. Must I write on Dutchness? Oh no! I must not! (My Dutch character has arguments to support it, too. After all, only *exotics* are required to culturally localize themselves. And, one may wonder, what kind of imperialist power [benevolent or not] hides behind the interest this time?) But more is going on here. *How* to account for, how to typify *Dutchness?* Attributing a character to "a typical Dutch person" may have been useful to the writers of a pamphlet to be dropped from airplanes to inform the inhabitants of the country about the foreign soldiers. It may even have led to an instructive leaflet for the soldiers who needed to realize they differed from the natives. (The crucial point being that they were not to expect the prudent Dutch girls to be inclined to have sex with them.) But in most other contexts it has little pertinence. A large half century later anthropologists no longer tend to delineate national cultural characters at all. Anthropology has shifted from this to the investigation of patterns of shared meaning, and then on again to other ways of articulating similarities and difference.

*Notities over Nederlanders* lists a variety of ethnographic studies done in the

were indeed worth living. The term *quality of life* was coined to fill the gap left by many people's disappointment with survival alone.

So in current practice, clinical trials assess medical action not just against physical parameters, but also compare the impact of treatment on people's quality of life. Another step, maybe, toward a politics of what, but there are many more to take. For instance: in the quantitative research tradition of the trial, the question about what gives life quality and what does not is still taken up in a quasi-naturalizing way. A sociologizing way, or so one might say. What *the good life* might entail is not recognized as an essentially contested and thus a political issue. Instead, research is set out in a way that objectifies this good. Surveys are used to record individual opinions, weights are attributed to these, and they are entered into statistically sophisticated accounts. In this way quality becomes a quantity. Values are turned into facts, social facts. All the controversies around the question of what a *good life* might be are stifled. That people have different investments in life, that we clash when it comes to striving after the good, is turned into a mere calculative challenge. We are each accorded our own opinion. Here, fill in your form, of course your opinion will be taken into account. Not as a political act, however, but as a social given. Instead of being staged in a theater of discords, differences are flattened out onto a spreadsheet.

If I advocate a politics-of-what here, it is not to suggest that the state should get involved in every detail of what happens in the hospital by proliferating laws. It is, instead, to stress that all these details involve "the good life." Relating this to clinical trials, one might say that not only issues now categorized as relevant

Netherlands. Some unravel the fishermen's trade, others stem from field work in orthodox Protestant villages, yet others follow heroin users in Utrecht or boy prostitutes in Amsterdam. They all explore the specificities of these different sites and situations. However, if we take them together they make it more difficult rather than easier to answer questions about *Dutchness*. What might these sites and situations have in common with each other? What do they share with hospital Z? Some works are presented that report on care practices, the most intriguing being by visiting anthropologists from India who studied, horrified, the way in which elderly people in the Netherlands live: isolated, institutionalized if in need of care, far from their families—and not even wishing their daughters to take care of them. Very Dutch, to be sure. But then again, this specific setup differs little from the arrangements in Germany, Sweden, Denmark—or a range of other European countries.

The boundaries around the Dutch state do not map on to a cultural domain. This is not to say that the cultural domain is simply larger; say *Europe*. There are striking differences between different European countries. For instance, Madeleine Akrich and Bernike Pasveer have com-

to our *quality of life* are "more than natural," but everything evoked in trials. The end points, the very goals of medical interventions, are essentially contested. They are intertwined with different, dissonant, ways of life. It is in this sense than one may say they are political. Take the question of how to compare bypass surgery and walking therapy for patients with atherosclerosis in their leg arteries. Which parameter should be improved after these treatments? If an angiographic picture were used to evaluate both treatments, walking therapy would never stand a chance of coming out as a successful intervention: it doesn't alter the width of the stenotic lumen. If pressure drop over the stenosis is measured, surgery will again appear to be the more successful treatment. As it even may if "a patient's pain-free walking distance after three weeks" is turned into the parameter of success. Walking therapy improves other parameters: it has different strengths. It is more likely to come out as a successful intervention if the patient's overall walking capabilities after six months are assessed. Or if the patient's gain in self-confidence is taken into account.

A politics-of-what assumes that the end points of trials, the goals sought for, are political in character. But there is more. Interventions have other effects, too. They bring about more than they seek to achieve. In current practice, trials deal with a few of these, so-called side effects. Usually, they take one or two calamities into account—like the risk of dying from an intervention (more real in bypass surgery than in walking therapy, although there, as everywhere in life, it may

pared childbirth practices in France and the Netherlands, countries only a few hours by car or train—but also worlds apart (Akrich and Pasveer 2001). Whereas in France pain is driven as much as possible from the scene of birth, Dutch women learn to *dive into* their pain, endure it, and use it to get attuned to—no, not just to what is happening to them, passively, but to what, actively, they are doing. Whereas in France women are tied to an apparatus measuring their physicalities, in the Netherlands they are advised to move about and find a position that best suits their bodies. Whereas a French father is just about allowed to be present during childbirth, a Dutch partner is expected to help his or her woman breathe properly to control her contractions. So there are differences, contrasts. National cultures at last? No, Akrich and Pasveer hesitate to summarize their findings under two neat headings, French versus Dutch. What kind of entities are these? Where is the boundary between them? And what about the stories told to them by French women that resonate more with what happened in the Netherlands than with what went on in their own neighborhood—and vice versa?

So differences may be huge, even if not easy to *nationalize*. On the other hand, sometimes similarities between what is going on across borders are at least as impressive. But this raises further questions, instead of leading us to "cultural commonalties." Take David Armstrong's *Political Anatomy of the Body* (1983). To me, reading this book was astonishing. At the

occur). And alongside side effects, economic value enters evaluation: low cost is accepted as an important good. But most other aspects of the *mode of life* that come with enacting a disease in one way or another may enter clinical deliberation but have trouble getting represented in evaluation studies. Going twice daily for a serious walk requires a great amount of self-discipline: is that a good or not? Being taken care of by a devoted surgical team may be a treat—or not. And is it a rich experience or grossly alienating to become acutely aware of the color of the tissues beneath one's skin?

A politics-of-what explores the differences, not between doctors and patients, but between various enactments of a particular disease. This book has tried to argue that different enactments of a disease entail different ontologies. They each *do* the body differently. But they also come with different ways of *doing* the good. In each variant of atherosclerosis the *dis* of this *dis-ease* is slightly different. Different, too, are the ideals that, standing in for the unreachable "health," orient treatment. These and the other *goods* medicine tries to establish require further exploration. The study of ontology in medical practice presented here deserves to be followed up by an inquiry into the diverging and coexisting enactments of *the good*. Which goods are sought after, which bads fought? And in which ways are these goodnesses set up as being good—for there are huge

time, I was engaged in a joint research project into Dutch medical knowledge in (the second half of) the twentieth century. All *our* material was Dutch: medical professional journals, in Dutch, written by Dutch authors. And yet in Armstrong there were quotes that were almost literally the same. Armstrong attended to subtle shifts in professional investments in "the patient" and what this figure's characteristics implied for how patients should be listened to. This was one of our topics, too. And it became almost a game for me to compare the dates at which new configurations emerged. These did not run exactly parallel, but neither was one country always ahead of the other. Sometimes the British appeared to be a year or two earlier. At other times it was the Dutch. (If you want to make the comparison yourself, see Mol and Van Lieshout 1989.)

But what to make of this striking similarity? Instead of evoking *culture* in one way or another, it seems more promising to look at *money*. Due to the way health care was financed from the 1940s onward, general practitioners were relatively strong in Britain as well as in the Netherlands. In order to consolidate these arrangements, general practitioners came to stress their own specific strengths in contrast to those of the expanding medical specialists. These, they suggested, lay in the way they kept track of entire families over long periods of time, and not just individual patients; in attending not just to sick bodies but difficult life circumstances as well; and, finally, in conversational techniques that made them attentive to the points of view of their patients (techniques imported from the social workers with whom they collaborated in giving primary

differences between, say, conversational persuasion, scientific trials, ethical arguments, and economic power play. Or, another dimension of such a possible study, how do we live with lack and badness, and how do we practically handle the limits to the good?

These questions are not answered here. Investigating *the body multiple* merely helps to open them up. In suggesting that we pose them, however, there is a strong suggestion—or should I say sentiment?—that there is not such a thing as a single gradient of the good that "we" (whoever we are) might all agree about (whether convinced by the facts or in an open and honest discussion). Like ontology, the good is inevitably multiple: there is more than one of it. That is why for a politics-of-what the term *politics* is indeed appropriate. For a long time, and in many places, science held (or continues to hold) the promise of closure through fact-finding. In ethics, the promise of closure, or at least temporary consensus, through reasoning is widely shared. In an attempt to disrupt these promises, it may help to call "what to do?" a political question. The term *politics* resonates openness, indeterminacy. It helps to underline that the question "what to do" can be closed neither by facts nor arguments. That it will forever come with tensions—or doubt. In a political cosmology "what to do" is not given in the order of things, but needs to be established. Doing good does not follow on finding out about it, but is a matter of, indeed, doing. Of trying, tinkering, struggling, failing, and trying again.

care—who had in turn imported them from American social work and humanist psychology). And when they got a foothold in medical schools, general practitioners started to teach their conversational techniques to all future doctors. It is *this* that makes visiting a Dutch physician resemble visiting a British physician far more than, say, a German one—even if in seventy-five other ways the differences between Dutch and German "cultures" are much smaller.

So where a text comes from, how to specify its local provenance, is a topic rather than something to be taken for granted. This is an issue much discussed in recent anthropological literature, in part because delineating a site helps in its turn to specify what "a culture" is made to be. (See, for example, the various texts assembled in Fog Olwin and Hastrup 1997.) Is the specificity of the material in this book its *Dutchness*? Is it that of a country with a generally well-educated population? Or does it have to do with a health care organization where general practitioners are relatively strong? Or with places where most patients get all their health care costs reimbursed? Or can this story only be understood as stemming from an academic hospital in a medium-sized town that is neither in the southern Catholic part of the Netherlands nor in the severely Protestant north? The possibilities are endless. They can be piled up to the point where the material analyzed here can be said to come from hospital Z and hospital Z alone.

The goodness ingrained in different versions of any one disease is inevitably contested. But this does not mean that a politics of what can depend on the traditional other of knowledge and reasoning: choice. Multiplicity, after all, is not quite pluralism. Diseases may be enacted differently at different sites, but the *sites* in question are not *sides*. Instead, different enactments of any one disease are interdependent. They may be added up; patients may be distributed between them; and they may include each other. There are innumerable tensions inside medicine but clashes between fully fledged paradigms are rare. Even an internist who scolds surgeons for focusing on clogged up lumens while not attending to the atherosclerotic process *has no choice* when called on by a patient whose ulcerating wounds no longer heal due to a lack of oxygen. He sends the patient (with collegial greetings) for an operation.

The interdependence between different versions of any one disease makes "choice" an ill-suited term for articulating the quintessence of a politics of what. And so does the interference between the enactment of disease(s) and that of other realities. Diseases, after all, are not the only phenomena enacted in the hospital. There are many more: sex difference, age and aging, Dutchness and foreignness, professionalism, emotional wisdom and instability, and so on. Thus, when two variants of a disease are separated out as each other's alternatives, a lot more is at stake than these variants alone. Take, for instance, the reality of "the sexes." It is implicated in enacting atherosclerosis. The more operations surgeons do, the more important the layers of fat underneath the human skin become for what it is *to be* or *not to be* a woman—or a man. But does

Let us now turn to sociology. Since hospital Z, and only hospital Z, is where the fieldwork for this study was done, many sociologists might be inclined to see it as a *micro*study. A study into something small. But is it? In his *Postmodern Geographies*, Edward Soja talks about Los Angeles (Soja 1989). A quite different place from hospital Z. Equally small? Well, measured in square kilometers it is somewhat larger, but those who set the *micro* against the *macro* might still say that it is pretty small. However, Soja escapes such attempts at scaling. He aptly shows how the town he chose to study includes "everything." *It all comes together in Los Angeles*—as one of his chapter headings goes. One reason for this is that people from literally all over the globe have come to live there. And they have brought their clothes, food, marriage customs, language—everything—with them. But Los Angeles is a big container for another reason. Everything that Soja takes to be crucially important for postmodern times can be signaled in this single city: all the shifts and changes that have to do with cities, their (absence of) planning, their distances, their patterns of trade, their transport systems—everything that geographers take to be important is

that argue for or against operations? With every health care statistic produced, the difference between the sexes gets more difficult to disentangle, for the preprinted forms all ask one to come out as either M or F and thus tend to add yet another M/F difference to the pile that is already established. Setting normal values for each person individually, by contrast, empties out the relevance of differentiating between two sexed populations. It goes on and on. There are ever so many interferences between "atherosclerosis" and "sex difference." But how might those inform "choices" to be made as a part of a politics of what? It is difficult to see how to take just this single other relevance, let alone all realities enacted into account: one just cannot gain an overview. And one's evaluation of the enactment of any *one* object may well contradict one's evaluation of the other.

There is a third difficulty with the term "choice." If practices enact not just one entity, but evoke a world, then it is not only diseases that come in varieties, but people, too. They, or maybe it is better to say, *we,* whether figuring as professionals, patients, or something else, are caught up in this. We do not master realities enacted out there, but we are involved in them. There are, therefore, no independent actors standing outside reality, so to speak, who can choose for or against it. Take the surgeon. That figure varies along with the rest of reality. If atherosclerosis is enacted as a deviant condition that happens incidentally and accidentally, a surgeon is enacted as the welcome savior of an unfortunate patient. If, however, atherosclerosis is enacted as a slow process that should be

present in Los Angeles. And since it is all there, there is no need for the analyst to travel all over the place. There is no need to look for a *big* object. This single city serves. It contains everything.

The same is true for hospital Z. It makes little sense to talk of its size, let alone to call it small. Again, this is not just because the actual physical entities present in the building come from a lot of places. There are American journals; German measurement machines; Japanese televisions; computers made in the Philippines; there is South American coffee —and so on, as in all modern hospitals. People working in Z have also circulated—I mentioned before that some of them come from elsewhere (China, Portugal, Switzerland, Britain) while others may have been born in the Netherlands but have spent a few years doing research in Paris, Seattle, or Toronto—or practicing in some small African town. But there is more. If one wanted to study, say, angiography, then what kind of large place would one try to find? Sure, there are hospitals slightly bigger than Z, but one cannot study the workings and usages of an X-ray apparatus somewhere "macro." It is always "micro," in a particular place. And the same is true for surgery: this is done on one body at a time. Or talking to a patient. Or thinking about how to treat. Adding up figures that come from ten or a hundred hospitals doesn't gives a *bigger* picture—it simply depicts something else. It

stopped early on, a surgeon is someone who is always too late—someone who only alleviates symptoms and is quite incapable of getting at the real disease. The identity of the very surgeons who might want to "choose" between modes of enacting atherosclerosis interferes with the "choices" to be made.

Patient identity is equally at stake. What a patient *is* is not given once and for all and is not so strongly established outside the hospital that it may be carried along into the consulting room, the ward, the operating theater, or the research lab. With every enactment of atherosclerosis there comes another patient. An example. If atherosclerosis is enacted as a genetically based deviance, you are simply burdened with it if you have the wrong genes. However, in so far as the development of the disease is enacted as a lifestyle matter, someone with atherosclerosis may be accused of having led a bad, unhealthy life. In this context, then, the patient is marked as irresponsible. This is not only a strange qualification to opt for if one had a choice, but also one that disqualifies one's abilities to handle options.

So there are incongruencies between what is implied in the notion of choice and the coexistences and interferences between different versions of reality described in this book. All in all, "choice" may not be the best term to capture what needs to be done, and what is going on, in the politics of what that *we* as medical professionals, ethnographers, sociologists, philosophers, and, yes, as patients too engage or may engage in. We need other terms. We have some other terms: discord, tension, contrast, multiplicity, interdependence, coexis-

conveys, say, epidemiological rather than individual facts; a numerical rather than a narrative reality; aggregations rather than events. (Why is it still necessary to argue against the idea that there is such a thing as a big picture? The argument was made *in the literature* quite a while ago. The scientist shuffling with paperwork on a desk, Bruno Latour explained in 1984 in French, handles not *more* variables than the hunter armed with arrows out in the field but, usually, far *less.* The scientist's numbers are just simplifications from a wide territory skillfully drawn together. And instead of residing in some *macro* place, they are on a desk. For the English version of this argument, see Latour 1988.)

Events are necessarily local. Somewhere. Situated. And in as far as this book tells about events, its object is necessarily local, too. But the main object of this book is not even events, but something different yet again. *Coexistence.* Theoretically speaking, this book is about the modes of coordination, distribution, and inclusion that allow different versions of a "single" object to coexist. But where, in what *place,* might coexistence be studied? There may be long distances between the entities that coexist under a single name. Take McDonald's. It is a fascinating multiple object, with endless similarities and differences between its various outlets, worldwide. (The idea that there is such a thing as the one and

tence, distribution, inclusion, enactment, practice, inquiry—but we could do with more. Which ones? That is another of the questions this book opens up rather than answers. For now, the point is this. In contrast with the universalistic dreams that haunt the academic philosophical tradition, the world we live in is not one: there are a lot of ways to live. They come with different ontologies and different ways of grading the good. They are political in that the differences between them are of an irreducible kind. But they are not exclusive. And there is no *we* to stand outside or above them, able to master them or choose between them: we are implied. Action, like everything else, is enacted, too.

*Clinical Medicine*

That there are alternatives to each particular practice does not turn the hospital, or health care, into a state of permanent turmoil. Tensions crystallize out into patterns of coexistence that tend to only gradually dissolve. Though nothing is sure or certain, the permanent possibility of doubt does not lead to an equally permanent threat of chaos. Even if stability is never reached, tensions are tamed. There are recurrent patterns of coexistence between different enactments of any one disease. Addition, translation, distribution, inclusion: they keep the hospital together—just as they assemble the body and its diseases.

Describing health care in this way, or so I claim, is an act. How far this act may reach, whether and if so how this text will make a difference in practice remains to be seen. It depends on where this book is moved to, on who might run away with it, on the number of copies sold, on the (non)accidental overlaps between its concerns and those of some of its possible readers. What are you,

only, successfully globalized, McDonald's is done away with in Watson 1997.) But then again, if one is interested in modes of coexisting, it may well be that hospital Z *contains them all*. Coordination, distribution, and inclusion, at least these three, are all to be found in Z. It isn't even necessary to roam through the entire building to achieve this—there are lots of sites and situations in hospital Z that aren't mentioned in this book. A few practices relating to atherosclerosis of the leg vessels seem to compose a field *big enough* to contain as many patterns of coexistence as can be analyzed in a single book.

All this suggests that the precise *size* of a field is of little importance to the theorist who does not try to map that field, but tries to discern patterns in it, modes and modalities of, say, coexistence (but it might be something else as well). But if the size of the field is irrelevant—indeed unmeasurable—this does not mean that the fact that there is a field is of no importance. The patterns of coexistence described here exist *somewhere*. Whatever the place is called: hospital Z; the enactments of atherosclerosis; health care; the Netherlands; the last decade of the twentieth century; well-insured surroundings;

reader, going to do with my words? That is beyond me—it is up to you. But I can try, have tried, to be articulate about what this text does intellectually. In theory, so to speak. It does not engage in criticism. I have *not* pointed, here, at the wrongs of medicine in general nor at those of the treatment of atherosclerosis in hospital Z in particular. I do not seek to confirm that all is well, but have argued, instead, that separating out right and wrong is only possible if one has a standard. I have not deployed such a standard here, but have analyzed the coexistences of different enactments of reality and have claimed that ever so many standards, different ways of grading *the good,* come with them.

However, this is not a neutral book either. Far from it. Analyzing medicine as enacting different realities and different ways of qualifying the good is not just a way of talking about medicine but also a way of talking inside it. Inside the medical world, this book is one of many voices that resist the idea that rationalization is the ultimate way of improving the quality of health care. Rationalization as an ideal starts from the idea that the problem with the quality of health care resides in the messiness of its practice. However, even if it may be messy, practice is something else as well: it is complex. The juxtaposition of different ways of working generates a complexity that rationalization cannot flatten out—and where it might, this is unlikely to be an improvement. In those sites and situations where a so-called scientific rationale (be it that of pathology, pathophysiology, or, most likely at the moment, that of clinical epidemiology) is brought into practice, with sufficient effort it may well come to dominate the other modes that are already at work. But this does not so much improve medicine as impoverish it. And that loss is borne by the clinic.

medical practice. There is a lot more to say about these ways of naming, localizing. But what I want to stress for now is just this one thing: that my theoretical investigation into the coexistence of the various versions of a multiple object were, indeed, *localized.* That a philosophical interest in ontology was linked up here with the empirical study of a field. This goes against the dominant tradition in philosophy. For a long time, the endeavors united under the banner of *philosophy* were presented as having a peculiar relation to place. They were *universal:* valid everywhere—and rooted nowhere in particular. Philosophical concepts had to be of universal value. Norms had to be justified by arguments of universal pertinence. But all this could be done here and now. What was right in theory was supposed to be transportable anywhere—so easily that no attention was paid to what it might mean to transport "rightness." Universalities need no landing strips, telephone lines, or even satellites. The question of their transport is simply not posed. (For the obviously slightly more complicated history of the relation between philosophy and *place,* see Casey 1997.)

Some philosophers have opened up ways of leaving that dream of universality

In stressing multiplicity, this book lends support to clinical medicine. Clinical medicine is the tradition that departs from patient histories and presenting symptoms rather than from physicalities isolated in lab-like circumstances. The tradition, too, that lives with adaptable subjective evaluation rather than requiring objectified figures. A tradition of case histories rather than counting. This book doesn't support the clinical tradition by critically pointing out where it has lost, or is losing, ground. Instead, it does so by stressing its present, underacknowledged, importance. The surgeons of hospital Z, after all, only open up arteries if their patients' daily lives are likely to gain from it as a result. Clinical considerations are crucial to their treatment decisions. And only those patients who present themselves with complaints make it to the hospital in the first place.

The proliferation of medical techniques may give reason to fear that the lab is taking over, but something quite different is equally possible. Since each diagnostic outcome diverges from the others, the idea of *gold standards* may get undermined rather than strengthened. And if each therapeutic intervention achieves something different, what counts as improvement may similarly tend to become less obvious. The question "is this intervention effective" then dissolves into another question: "what effects does it have?" Clinical considerations, however fuzzy they may be, however badly they fit into forms and accounting systems, may well prove obdurate and tenacious. After all, they con-

behind. Walter Benjamin offers a wonderfully radical example. His *Passagen-Werk* (1999) was *both* situated *in* philosophy *and* somewhere *earthly* in particular. Paris. The modern city. Its architecture. Arcades. Encounters between strangers. It is this overt attentiveness to the *situatedness* of thinking (its objects, its possibilities, its enactment, its preformative effects) that marks the philosophical literatures that figure in the background of this book and form its venerated ancestors. The one to conclude with is Michel Foucault. In his writings it is an acute sense of situatedness that turns philosophy into something worthwhile in the first place. Something forever shifting, changing. A mode of engaging in philosophy that advertises itself as linked up with the here-and-now, with *ourselves*, cannot be—nor does it hope to be—*universal*. It is localized. Foucault mainly explored empirical matters in a historical manner—but ethnographic or rather praxiographic extensions easily follow. Please add, therefore "topographical" to the "historical" situatedness that figures in the following quote. So that this subtext relating to the literature may end, as is only fitting, with words *taken from* the literature. "The critical ontology of ourselves has to be considered not, certainly, as a theory, a doctrine, nor even as a permanent body of knowledge that is accumulating; it has to be conceived as an attitude, an ethos, a philosophical life in which the critique of what we are, is at one and the same time the historical analysis of the limits that are imposed on us and an experiment with the possibility of going beyond them" (Foucault 1984, 50).

cern daily lives. And daily life is what, when it comes to it, matters most to people. It is where patients, *we,* have to live with our doubts and our diseases. No, all is *not* well. But where rationalization risks to overrule the clinical tradition with ever more statistics, accounting systems, figures, and other carriers of scientificity, this book sides with those voices that seek to improve the clinic on its own terms. Which terms? How to *do* the clinical good better? These are further questions I leave open here.

So even if it is not critical, this is not a neutral study. There are other modes of partiality than that of passing judgment. Undermining the traditional hierarchy between the sciences is a way of strengthening the disciplines that occupy the lower ranks in the hierarchy. Pointing at the persistent possibility of doubt eats at the self-assuredness (and the convincing power) of the techniques that claim that they are finally able to bring light and science to messy practices. Rather than comparing different interventions within a given dimension, laying open the various dimensions of comparability makes space for and gives visibility to dimensions that currently attract the least attention. Not going primarily with a politics of *who* but stressing the necessity of a politics of *what* helps to open up the professional domain instead of pushing it back. And doubting whether choice is the best term to use in a politics of what (a politics that includes ontology rather than presuming it) acts against rationalist fantasies of what it is to strive after the good. Presenting the *body multiple* as the reality we live with is not a solution to a problem but a way of changing a host of intellectual reflexes. This study does not try to chase away doubt but seeks instead to raise it. Without a final conclusion one may still be partial: open endings do not imply immobilization.