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The Work Users Do

Some representations of social life require their users to do a lot of work. How many users have the knowledge and skills it takes to do that work? What happens if they can't or won't do it? How do the makers of representations deal with the differential ability and willingness of users to do the work their reports demand?

Construing

Some representations seem to give up their meaning easily. You take it in at a glance, like picking an orange off a tree in the backyard. Others require more work, more thinking about, more pondering of the implications. Let's use the word *construal* to refer to the message recipient's making something of it, interpreting it, giving meaning to it or taking meaning from it.

A user can take any representation of society in one of those two ways: as obvious, with the meaning so "just there" as to require only minimal and routine message handling, or as dense, requiring careful attention to all the details. "Obvious" and "dense" aren't natural characteristics of objects or events. Rather, they describe the way we decide to attend to those things.

We attend to representations in the ways we have learned. Representations seem obvious to users who already know all they need to know to take in their meaning, and dense, requiring more work, when the users haven't encountered anything quite like that before. We have all had some training, starting as small children, in construing such

objects, but we haven't all had training and experience with all kinds of representations. These abilities are distributed differentially along all kinds of lines of social division.

We can read every photograph as either obvious or dense (I'll show how you can read the same photograph in different ways in chapter 10). Many photographs make use of conventions well known to so many kinds of people that just a few hints tell experienced users, people who ordinarily come in contact with them, the whole story, the way most of us can guess at the full text of signs we only see fragments of. In well-organized representational worlds, users know how to construe the representations they routinely run into. Take, as an example, sports feature photographs—not the action photographs made during the game or match or competition but the ones of the other activities surrounding the big game—which are organizationally constrained (Hagaman 1993) to be highly formulaic so as to be easily readable by experienced viewers. They deal with a small selection of situations, well known to the newspaper readers who routinely see them.

The most common images (I follow Hagaman's analysis closely here) deal with a player or team winning or losing. Every game that has a winner, of course, also has a loser. Which side the picture shows as the winner depends on which town the newspaper for which it is made serves. Photographs in Chicago papers treat the Cubs and Sox as "our team," whose wins we celebrate, while New York papers treat the Yankees and Mets as "ours." Readers don't have to figure that out; it's part of the equipment they bring to their interpretive activity. (Photographs made for the wire services, which service many newspapers in many cities, usually include a selection from which local editors can choose one appropriate to their hometown team.) When "our" team wins, we see the jubilant winners, individually or collectively, their arms up in the air, heads thrown back, mouths open, or hugging one another. When "our" team loses, we see a lone loser sitting on a bench, head down, shoulders sagging, perhaps with another player's consoling arm around the shoulder. These stereotypical poses appear in photographs of athletes of all kinds: amateurs and professionals, women and men, adults and children.

Well-socialized Americans (and, no doubt, more and more people everywhere) learn this language of gesture and posture as children and so take only a second to extract the intended meaning from a picture of an athlete with his arms reaching into the sky and a big smile on his face. What else could it mean? He won! In the same way, they know the language of losing. When they see someone sitting on a bench, alone, head down, they know, from the hundreds and thousands of such pictures they have seen before, that that player lost. What else? The meaning is not obvious because such gestures, presented in that visual language, are inherently obvious. It's obvious because users have learned the language the way all languages are learned, through constant repetition. They *know* how to read that image.

Photographers picture winners and losers in that easily construed way so that newspaper readers need give the images only a second or two while scanning the results of yesterday's games. The images give up their essential meaning quickly to those who know the code. Because users know the language and photographers know that they know it, such images are easily made, once the image makers master the language, so that they can meet the requirements of the editor who sent them to cover the game quickly and efficiently.

Easily read images—images made in a widely known visual language—show up beyond the sports pages. The standard topics of big-time, serious photojournalism—war, famine, assassinations—have a repertoire of similarly canonical pictures, which use highly conventional visual language easily interpreted by any well-socialized user. Famines dependably produce the small child with the swollen belly. Assassinations come in two forms. The photographer lucky enough to have been on the scene when the killing happened gets the assassin pointing the gun as the victim falls to the ground. Photographers who arrive later must content themselves with the victim lying on the ground in a pool of blood. And everyone who sees such a picture knows "what it means."

Making such an easily read image takes skill. The photographer has to fill the frame with the formulaic image, excluding details that would distract users from the formulaic clues or blurring those "extraneous" details (what editors sometimes refer to as "clutter") through selective focus (Hagaman 1993, 50–51, 59–63).

As we have seen in Walker Evans's work, other pictures, just as skillfully made, have the opposite intention: to include details whose meaning is not obvious, which do not use already well known conventional visual language, details that reward attentive study and thought. These images seem plain or uninteresting to people who don't look at them carefully. They don't use the commonly understood codes that tell users what they are about. Instead, they force users to pick out relevant materials consciously and work out their interconnections, see what can be made of them.

This is what makes artists who take up the work of social analysis so interesting. They don't want to present the formulaic and already known or use already well known language. They want to show the people who look at their pictures something they haven't seen before. When photographers do use visual language everyone knows, they want to make the viewer see new meanings in it.

The conceptual artist Hans Haacke exemplifies this point (Becker and Walton 1975). Haacke once described his work as the study of systems: natural systems, as in his early sealed plastic cube containing a small amount of moisture, whose alternate condensation and evaporation displayed the systemic character of those processes; and, in his later work, social systems, in pieces explicitly displaying the workings of political and economic power (Haacke 1975, especially 59–123).

His "Guggenheim Project" (Haacke 1975, 59–67), for example, consists of seven panels of type containing a lot of facts about the trustees of the Solomon R. Guggenheim Museum in New York City: who the trustees of the museum are, their family connections (they are almost all members of the Guggenheim family, though many have different surnames), what other boards (of companies and organizations) they sit on, and many facts about the crimes committed by those companies, especially their exploitation of indigenous workers in third world countries. The Guggenheim piece announces no conclusions and makes no generalizations; there is not a hint of Marxist or any other variety of political analysis—just the recitation of facts. Haacke does not point a finger at guilty parties or allege any conspiracies. Still less does he say that this bastion of modern art and progressive artistic thinking gets its support from wealth based on the exploitation of labor in countries less advanced than the United States.

But someone who inspects this work would have to be extraordinarily obtuse and willfully unseeing not to arrive at just that conclusion. Haacke takes advantage of ordinary readers' habitual methods of reasoning by using a well-known format, a simple listing of unquestioned facts: names, dates, places, official offices held. So you learn who the trustees of the museum are, that most of them belong to the same extended family, that they sit on the boards of several corporations, that these corporations engage in mining activities throughout the world. As you take in each "obvious" fact, you add it to what you already know, and . . . the conclusion that the museum is financed by the exploitation of oppressed laborers around the world is there for the taking.

But it isn't there just like that; you have to know how to take it. Since most users know that, the conclusion results from the work they do, arranging these simple and indisputable facts as syllogisms and drawing the conclusions those syllogisms, apparently inevitably and naturally, lead to. Haacke used the same technique to display, for example, the political (most importantly, Nazi) connections of a German industrialist who was chairman of the Friends of the Cologne Wallraf-Richartz-Museum, which had donated Edouard Manet's painting *Bunch of Asparagus* to the museum (Haacke 1975, 69–94).

I used the word *construe* to refer to this activity, through which users in interpretive communities (I'll come back to that expression shortly) easily and "naturally" extract and make sense of a representation's meaning. I did that to make clear that such work must be done before a representation gives up its meaning to a user. What is that work? *Construe* refers, in its primary meaning, to analyzing the grammar of a statement, to understanding the terms in which it is put and how they are connected to each other; the more extended meaning is "to discover and apply the meaning of; interpret." Let's take this seriously.

Users often skip this step and, in fact, may ignore the representational artifact so carefully constructed for them altogether. I don't mean the kind of casual look and quick read, the flipping through a book of photographs from the back that so irritates photographers. I mean the practice Lawrence McGill describes in his study of the reading practices of students taking a class in science, in which they were required to read many articles containing large numbers of numerical tables. He says:

> The students' orientation toward reading these articles is that they must "get through them" in order to meet the requirements of their classes. These students take pains to avoid the dross, that material extraneous to "the point" the article is trying to get across. Statistical tables, descriptions of methodology, and results are thought of as canned procedures appearing in virtually every research article (that is, these are the sections that read as though they were "written because they *had* to be"). Their purposes are known and understood, and students pay attention to them only if given a good reason. (McGill 1990, 135)

Since they seldom found such a good reason, they pretty much ignored the tables that constituted the heart of the articles they read, reasoning that those tables must say what the authors said they said or the editors would have rejected the article. They memorized the conclusions, which they thought were sure to be what the class examination would quiz them on, accepting on faith that the other stuff in fact supported those statements.

So users might not do the work left to them, might just not bother at all, not look at the photograph; they might go to sleep during the film, rush past the table, skip large sections of the novel. It happens.

But often enough, it *doesn't* happen, and even when it does, we might decide to ignore people who ignore what we have made for them. We'll keep our eyes on the alert for interested viewers who are willing to do the work necessary to disentangle the meaning from the package it comes in.

We can begin the analysis of the construal of representations by noting that all these representations serve as devices for summarizing data and ideas. Every version of social science analysis has to do the job of making less out of more, in the process making what has been gathered more intelligible and assimilable (this important topic gets chapter 6 all to itself). Latour (1987, especially 233–43) describes in detail how scientists summarize and reduce their data, removing

more and more detail from what they report in order to make what's left more transportable and comparable. He calls this series of transformations a *cascade*.

What the reader has to do is sometimes called, in connection with written texts, "unpacking" the representation, that is, undoing the summarizing that has produced the artifact we are inspecting. We can begin our thinking here by taking up one set of examples, the collection of tables and charts I put together for my seminar on this topic. These demanding tables and charts required some interpretive work, some construal.

Some tables are simple enough but very detailed, giving a level of detail most readers today would consider excessive, requiring too much attention for what they deliver. It's quite possible that confronted with these tables that go beyond what's conventionally expected, readers would just, as some of McGill's interviewees did, skip them, trusting that they say what the author who presents them says they say.

Consider two tables in W. E. B. DuBois's study of Philadelphia's historic black area, the Seventh Ward, the smaller of which, occupying only half a page, is labeled "OCCUPATIONS—MALES, TEN TO TWENTY-ONE YEARS OF AGE. SEVENTH WARD, 1896"; the larger one, which takes up two and a half pages, is the same except for the age group, changed to "TWENTY-ONE AND OVER" (DuBois [1899] 1996, 105–7).

These tables give a very detailed breakdown of the occupations of black juveniles and adults, far more detailed than anyone needs now or, probably, than anyone needed in 1899. What purpose would anyone have for a breakdown of boys 'occupations by one-year age intervals? And for a contemporary reader, some of the occupational names no longer mean anything. Many students in my seminar had no idea what a "hostler" did, that being one of the many esoteric and no longer well known occupations Dubois counted. (I knew it had something to do with horses but had to consult a dictionary to learn the full definition: "a person who takes charge of horses, as at an inn; a stableman.") More to the point, why bother to list, in a table divided into age categories, occupations like china repairer or wicker worker,

of which there were only one each? Still, it's all there for the taking, if you're in the mood to take it.

The table contains more information than any of us now think we need. Nevertheless, everyone in the seminar I confronted with this material knew how to read it. Many people, perhaps especially social science students, know how to do that. We all knew that the table was two-dimensional, that the dimensions were occupation and age, and that the numbers in the cells opposite the occupational titles and under the age headings told how many of each there were. The cell for "31–40 year old wicker-worker" had a "1" in it, meaning that there was one of those, just as the "28" in the cell for "21–30 year old barbers" meant there were twenty-eight men that old in that trade. And so on.

Many people find two-dimensional tables less obvious than these trained graduate students did. I discovered that when I had to teach a class of graduating college seniors majoring in sociology how to make sense of such an object (as I explained one in the last chapter), saying that the vertical dimension represented one element that had different values, the horizontal dimension a second variable that also had different values, and that the cells contained the number of cases (people) meeting both criteria.

The charts that often decorate social science reports serve as metaphors, two-dimensional representations of a complicated social reality. I'll analyze these metaphors in detail in chapter 10, just noting here that charts, no matter how simple they are, require construing and that what's to be made of them is never obvious. Looking at them, you have to consciously say to yourself, "Let's see, this line means this and that line means that; when you compare them, this line is longer than that one, so the quantity represented is greater." Or like some of the charts discussed later, they use symbols and formats created for the occasion, specific to these data and this analysis, so that the reader has to consciously identify the components and learn what they stand for and what, therefore, can be taken from the chart.

Plays, novels, films, and photographs generate different problems, particularly when the people who make them are artists. Artists usually think that their work speaks for itself, that they have already said everything there is to say about the topic, whatever it is, in the work

itself, and that any lack of clarity means the viewer didn't do the work necessary to make the meaning clear. That could be put as "You didn't read carefully," or "You didn't look at the photograph carefully," or "You were asleep when the crucial event in the play took place." In general, they charge that the viewer didn't pay the kind of complete attention the work requires.

Who Knows How to Do What? Interpretive Communities

If makers leave it to users to interpret the work, deriving its ramifications and consequences for themselves, its final meaning rests on what users know how to do with it, and with works like it. Knowledge of how to interpret what a user makes is not always—not usually, really—distributed uniformly through a community of makers and users of a given kind of representation.

Steven Shapin was interested in that problem as it arose at the beginning of the development of modern science. He wanted to know how Robert Boyle, the seventeenth-century English "experimental natural philosopher," communicated his findings in physical science to his colleagues and other interested parties. Shapin's analysis (1994) does not deal with telling about society, but it does explain how ways of telling depend on viewers' ways of understanding, and how, therefore, makers change how they tell their story when they want to reach a new audience. Shapin's analysis gives us a template for understanding how ways of telling about society might similarly change.

Speaking about Boyle's reluctance to put his findings in mathematical language and his preference for a verbal, though necessarily longer, way of reporting, Shapin says:

> Boyle understood mathematics to encompass an abstract, esoteric, and private form of culture. That was a major reason why he worried about its place within experimental natural philosophy. If experimental philosophy was to secure legitimacy and truth by implementing a public language, then the incorporation of mathematical culture might threaten a new privacy. In specifying that mathematics was written for mathematicians, Copernicus had only given prominent voice to widespread understandings of the place of mathematics in the overall literate culture. As Kuhn has observed, it was only the nonexperimental mathematical sciences that were characterized, even in antiquity, "by vocabularies and techniques inaccessible to laymen and thus by bodies of literature directed exclusively to practitioners." Boyle reportedly remarked upon the relative inaccessibility of mathematics. To go on as mathematicians did was, in his view, to restrict the size of the practicing community. Such restriction risked its very capacity to produce physical truth. To be sure, mathematical culture possessed very powerful means of securing *belief* in the truth of its propositions, while the proportion of those *believers* whose assent was freely and competently given was small. In contrast, members of a properly constituted experimental community freely gave their assent on the bases of witness and the trustworthy testimony of other witnesses.
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> . . . Boyle sought to make historically specific experimental performances vivid in readers' minds and to make it morally warrantable that these things had actually been done as, when, and where described. This type of narrative was also reckoned to be more *intelligible* than alternative styles of communication. His *Hydrostatical Paradoxes* specified that he *could have* reported findings in more stylized and mathematical form, but had *chosen* not to do so: "Those who are not used to read mathematical books, are wont to be so indisposed to apprehend things, that must be explicated by schemes [diagrams]; and I have found the generality of learned men, and even of these new philosophers, that are not skilled in mathematicks," so unacquainted with hydrostatical theorems that a more expansive and inclusive exposition was indicated. Notions of this sort could not "be thoroughly understood without such a clear explication of [these] theorems as, to a person not versed in mathematical writings, could scarce be satisfactorily delivered in a few words." Many words had to be used. It was, Boyle confided, "out of choice, that I declin'd that close and concise way of writing." He was writing not "to credit myself, but to instruct others," and, for that reason, "I had rather geometricians should not commend the shortness of my proofs, than that those other readers, whom I chiefly designed to gratify, would not thoroughly apprehend the meaning of them." (Shapin 1994, 336–37)

Boyle worried that an inappropriate mode of representation might bring about an undesirable restriction of the potential audience. He feared that readers would ignore unfamiliar language and styles of

reasoning, and some of the language of science developing in his time was esoteric in just that way, especially in its use of mathematical formulas, geometric diagrams, and the forms of reasoning to conclusions associated with them. I'll leave aside the question of whether such a restriction on who can read what an analyst of society can write is something that must be avoided or whether it is necessary to the development of scientific thought. That's an old and not particularly fruitful debate.

Let's instead appropriate the question for our concern about telling about society, exploring the less contentious sociological question of the different ways that the knowledge necessary for making and reading representations of social life is distributed. Who understands the work an analyst of society presents? At one extreme, some works about society present themselves, we could say, "to whom it may concern": to any competent member of the society who might be interested. At the other extreme, some works are presented to a very small and select group of people who alone can be expected to understand them and be able to interpret their arcane, not generally familiar, terminology and modes of reasoning. The two can be exemplified by, on the one hand, novels or photographs or films—and most especially the one among those that is aimed at the largest and most heterogeneous audience, the Hollywood film—and, on the other, the mathematical model.

The people who make Hollywood films mean them to be understood by anyone (with the dialogue dubbed in the appropriate languages) in the entire world. The language of film is, as a historical fact, now interpretable by anyone. There are probably no longer any people so isolated from Western marketing as to make such simple mistakes as thinking that an actor who had been killed in a film had really been killed and thus could appear in any future films only as a ghost, or wondering where actors went when they stepped out of the frame. (It has sometimes been said that tribal peoples with no exposure to Western cultural products have made such mistakes, but I can't find anyone who has said that in print. Let it stand, nevertheless, as a possibility.) No, everyone understands that those simple devices are just that, devices—and everyone understands much more complex matters, such as the way techniques like fades and wipeouts indicate the passage of time or the movement of the film's action to a different geographic location. Everyone understands, as well, the meaning of the sequential cuts from one face to another that indicate a dialogue is taking place between two people or that things are now being seen from someone else's perspective.

Which doesn't mean that audience members "know" these technical devices in the self-conscious and manipulable way a filmmaker or film aficionado knows them. They don't. They know it when they see it, but they don't know it to talk about, let alone to make one themselves. So there is a real separation between the makers of these representations, the film professionals who do it all the time for a living and have done it for years and years, and the people who watch these works for entertainment or possibly for information (or maybe they just get the information along with the entertainment, not having asked for it and perhaps not really wanting it). One group knows things the other doesn't. And so less-informed viewers can be "fooled" or "misled," moral problems of representation I discuss at length in chapter 8.

We can find the extreme opposite of this widespread knowledge of how to use a representation of social life in the world of mathematical model-making. Such a model creates an artificial world of carefully defined entities with a few simple properties, which can interact and influence one another only in a few equally carefully defined ways governed by specific mathematical operations (see chapter 9 for a lengthier explanation of math models). The advantage of such a model is not that it is a realistic depiction of how social life really works anywhere but that it makes clear what the world would be like if it did operate according to that model. And this is something worth knowing. One of the models described later tells you something that would interest many people: what the repertoire of a symphony orchestra would consist of if the orchestra replaced old works with new ones by following certain simple rules strictly (not that anyone does, but that's not the point).

Anyway, briefly, and maybe a little inaccurately: anyone who knows how to read, interpret, and understand a mathematical model

also knows how to make one. That is, construing these models, understanding them, requires a general acquaintance with the way mathematicians reason and a substantial understanding of the area of mathematical reasoning used in the particular case. To understand the analysis of symphonic repertoire made in the example I just gave, and to be able to be properly critical of it, you would need to know something, say, about difference equations; for the analysis of kinship systems made in another example given later, you would have to be familiar with Markov chains. Not many people know these things, and the ones who do usually (though not always) know them well enough to make models themselves. (And if you've put in the time and effort to learn all that, which few social scientists, and especially sociologists, have, you probably want to put those hard-won skills to work.) So, to oversimplify somewhat, the community of users of math models and the community of makers of math models are essentially coterminous and identical. It's just two different activities engaged in by the same people. Sometime they make models, sometimes they consume the models others have made.

Boyle, as quoted above by Shapin, is talking about something like the world of math models, though that's not exactly the kind of mathematics he had in mind. His complaints suggest some of the features it's useful to compare in discussing what we can call "interpretive communities," the groups that share enough knowledge (how much is a question, of course) to interpret the representations commonly made and used by their members.

Note, to begin with, the empirical generalization Boyle is working with, which goes something like this: the more complex and technical the expression of the results, the fewer people will be able to read and understand them. In itself that's no cause for complaint. Plenty of technical matters interest no one outside the relevant community of specialists, and there are many other things specialists think outsiders have no need to know about. But it is a common cause of complaint, because people who are not specialists do want to know enough so that they don't have to worry that someone is pulling a fast one on them (complaints about medical doctors often take this form). Here are some specific questions we can raise about this.

Whom do the makers want to reach? Put another way, whom is their world organized to reach, and what does that aim impose on them as a standard of intelligibility? People who make the kind of representation I make usually do it because some group of people somewhere wants something like that, and I make what I make (film, math model, whatever) in such a way as to be intelligible, pleasing, and useful to them. With whom, then, does their world's organization make them want to communicate routinely?

If you know the audience the makers want to reach, you can understand the features of any particular representation as the result of the makers' attempt to produce something that will reach those people in a form they will understand and approve. They will understand it because they have learned how to understand things like that, and they will approve it because it meets the standards they have acquired as part of that learning.

But Shapin's example of Boyle's practice shows that a maker might in fact have a choice of audiences to reach and that the choice of audience would imply a choice of representational style. So Boyle might reasonably have aimed for an audience of other learned scholars for whom the shorthand language of mathematical formulas and geometric representations of physical phenomena would have been no problem. But he wanted to reach beyond them, to a larger and more varied audience of educated gentlemen, who would understand the arguments he had to make *if* he made them in the plain language of ordinary, high-class, quasi-literary discourse that all gentlemen, more or less, knew.

So he had to use a less economical form of representation than he could have used had he confined himself to the technically experienced audience of his scientific peers. And that entailed using not just different words but a different style of proof. Mathematical proofs relied on the force of logic. What you showed to be true mathematically was true *of necessity.* If you accepted the premises and the reasoning was sound, the conclusion was inescapable. But what you showed in the world of empirical research was true in a different way. It was true because it was what people had observed to happen in the real world of real material stuff, and you knew it was true because it had been

observed to be true. Not by you, because you, the reader, couldn't be there to observe everything scientists were reporting, but because it had been observed to be true by someone who could be believed. And what kind of person could be believed? Gentlemen, who were bound by a code of truth telling. You, as another gentleman reader, understood the system of social controls that required them to tell the truth and so could decide to accept their report as credible for yourself, because you understood the risks to his own honor such a person would run if he lied.

These gentlemen-philosopher-scientists, further, needed a way to judge credibility that could avoid disputes. Disputes arose when someone refused to believe a report made by someone else, But gentlemen, in that time and place, could not question one another's word without giving serious offense and possibly provoking, at worst, a duel. A duel? Over a scientific finding? Though the penalties for misstating what you have observed are very serious today—loss of grants, jobs, and your scientific reputation—they aren't life threatening. If I said I saw *X* and you said I couldn't have seen such a thing, that was equivalent, in Boyle's time, to the terrible insult of "giving the lie," accusing the other of being a liar. And that was a true offense in a culture of honor, one that had to be dealt with in the appropriate way, which, even at that late date, was a fight, potentially to the death.

Boyle and his colleagues were unhappy with mathematical reasoning, because it aimed not just at precision but at certainty, which led to "civic disasters," disputes that could not be resolved without, in a very gentlemanly way, insisting that since someone was right, someone else had necessarily said what wasn't true. These scientists didn't want to fight over disagreements. They wanted to have a civil conversation about their disparate findings. They depended, after all, on each other's testimony for evidence, since they could not see everything for themselves. So they had to accept other men's sincere reports as possibly as right as someone else's contradictory, but equally sincere, reports about what might be the same matter.

This led to ways of investigating and reporting that were properly circumspect: "The naturalistic and the normative were systematically bound together. Practitioners recognized others as honest and competent, and they told each other how they ought to behave, only in respect of a shared view of the world which they investigated. Experimental culture shared norms insofar as its members shared a view of reality. It was this ontology which was the ultimate sanction on members' conduct. If you are a genuine investigator of the natural world, then *this* is how your reports ought to look and *this* is the epistemic status you ought to claim for them" (Shapin 1994, 350). Only by viewing the world as various, and not necessarily homogenous in the way mathematical treatment required, could you have the conversation among mutually trusting equals that would allow empirical scientific activity to go on. This leads Shapin to a speculation:

> *Every* practice, however committed to the production of precise and rigorous truth about the world, possesses institutionalized means of telling members when "reasonable agreement" or "adequate precision" has been achieved, when "enough is enough," when to 'let it pass,' when to invoke idiopathic "error factors" and not to inquire too diligently into the sources of variation in testimony. The toleration of a degree of moral uncertainty is a condition for the collective production of *any future moral certainty.* This toleration allows truth-producing conversations to be continued tomorrow, by a community of practitioners able and willing to work with and to rely upon each other. (Shapin 1994, 353–54)

The generalization of this statement that we need for the investigation of reports on society is that any interpretive community—defined as the network of people who make and use a particular form of representation—shares some rules governing what its members should believe and when and why they should believe it. How some members of that community represent and communicate what they know, and how other members interpret the communications they get, will be governed by more or less agreed-on rules, and those rules will embody understandings about the kinds of people who will be involved in each of these activities.

We needn't think that the definitions of the kinds of people involved will always be based on a code of honor and mutual respect. It way well be just the opposite: many makers of representations of

society don't think that users will know much or that they can be trusted with much. As a consequence, the representations they make use conventions that presuppose users who won't know much and thus include many aids—they are (as we say now) user-friendly.

So? The work of making representations is divided among makers and users. The work that makers do is there for users to use. What makers don't do, users must do. They may not all know enough to do what the makers want and require, they may know how to do it but not do it consciously, or they may do it differently. When they do it in their own way, they may well produce results different from what the makers had in mind. Different worlds of representation making divide the work quite differently. What seems inescapably the work of the makers in one world—labeling the rows and columns of the analytic table, for instance—becomes the ordinary work of users in the world of documentary photography. Every kind of representation offers the possibility, and probably the fact, of a different way of dividing up the work, with consequences for the look of what's made and for the fact of what's made of it.

5

Standardization and Innovation

Let's take stock. Representations are organizational products. The organizations and communities that make and use them divide the labor of selecting, translating, arranging, and interpretation between makers and users in a variety of ways. We can never take for granted how that's done, because the division of labor keeps changing. The makers choose what to include and how to arrange it. Do they do it "the way we've always done it," or do they try something new?

Most often, makers produce representations in a standard form that everyone understands and knows how to make and use. Occasionally, however, for whatever reason, someone begins to make representations of a particular kind differently, violating some of the existing agreements and provoking disagreements and conflicts. Such situations, bringing into question standards that have until then been taken for granted, provide the best possible data for sociological analysis of the day-to-day work of representing society. The polarity of standardization and innovation brings many features of the process into relief.

Conflicts occasioned by innovations in representation frequently take the form of arguments over what the best way to do it is. To do what? To make whatever kind of representation you and the other people who make and use them want. Representations can be and have been made and used in many different ways, and makers and users always have strong opinions about how do it. It's never easy or obvious which way would be best. What is the best way to write a scientific paper for publication in a sociology journal? What is the best