Michael Lynch

# A Sociology of Knowledge Machine

Ethnographic and historical research on scientific controversies and laboratory practices has opened up numerous technical 'black boxes' to expose hidden sources of uncertainty and contingency. When presented as a straightforward research agenda, a constructivist analysis of a scientific fact involves an effort to unravel a complex socio-technical background which is obscured by the practical and linguistic treatment of the fact as an 'eternal' entity, process, or law. Karl Mannheim distinguished knowledge for which 'existential factors in the social process [are] merely of peripheral significance' from beliefs, thought styles and world views that are bound to particular existential conditions, whereas more recent sociologists of knowledge have finessed that distinction by defining ***as a constructive process*** the very establishment ***that*** the 'historical and social genesis' of a particular matter of fact is 'irrelevant to its ultimate validity' (Mannheim, 1936: 271). Without directly impugning, for example, the factual status of 'charmed quarks' in particle physics or of a particular growth 'factor' in biochemistry, sociologists of scientific knowledge treat the (historical) ***factuality*** of the fact as itself a social construction (Pickering, 1984; Latour and Woolgar, 1979). This conception of fact offers something of a metaphysical guarantee for the sociology of knowledge, but in order to convert it into hard empirical currency the constructivist researcher is well-advised to go into detail to expose the technical loose ends, suppressed and forgotten dissent, and alternative conceptual possibilities that are glossed over when a fact is treated as a constituent of a natural world that transcends the local conditions of its emergence. The research required for producing convincing demonstrations of constructivist interpretations of science can be quite difficult in particular cases, but in some circumstances others do the sociologist of knowledge's work for her. The law courts, for example, provide occasions in which the unravelling (or deconstruction) of (apparent) facts is routinely accomplished (and, as we shall see, at times all too well) (Jasanoff, 1991:215-38).

The adversary system by means of which adept and well-informed attorneys can build up or undermine expert testimonies is an instance of what Wigmore calls 'the greatest legal engine ever invented for the discovery of truth' (Wigmore, 1940: 1043 at 29). The potential for malfunctioning is a well-documented feature of this engine, but on particular occasions it can provide an impressive 'sociology of knowledge machine'[1] we are referring here to courtroom hearings which determine if a given principle, fact, or technique is accepted by relevant communities of scientists. In such hearings, one set of witnesses alleges that certain evidences are based upon adequately performed techniques, accepted standards, and theoretical knowledge in a specialist community; and the adversary team then attempts to cast doubt on one or another of these claims. There is no guarantee that

[1] For a discussion of the 'machinery' of interrogation see Lynch and Bogen, 1996, Chapter 4. For a discussion of how patent disputes are relevant to science studies questions about discovery, invention and replication, see Cambrosio, Keating & MacKenzie, 1990: 275-293.

this sociology of knowledge machine will be effective in a particular case. An attorney may pass on the opportunity to contest expert testimony offered by the other side. Occasionally, however, an attorney (or team of attorneys) pulls out all stops and mobilizes a concerted attack on the factual status of the expert testimony provided by the adversary. (The ongoing O.J. Simpson murder trial in Los Angeles promises to be especially interesting in this regard, given the immense publicity and the resources mobilized on both sides to 'construct' and 'deconstruct' forensic evidence.) Even less spectacular adversary conflicts can provide interesting cases for sociology of knowledge purposes.[2]

A partly fortuitous set of circumstances has led us to take interest in courtroom controversies over the use of recombinant DNA techniques in criminal forensics. Several years ago, we began collaborating on a study of the 'dispersion' of specific molecular biology techniques. We were interested in how protocols, ingredients and equipment, embodied practices, and perceived importance and effectiveness of standard techniques varied with the circumstances of application.[3] One technique we investigated - the polymerase chain reaction (PCR) - became increasingly familiar to an ever-larger public during the time of our study. PCR was dubbed 'molecule of the year' by ***Science*** magazine in 1989, and Kary Mullis the 'inventor' of PCR was awarded a Nobel Prize in 1993.[4] PCR is one of two molecular biological techniques which have been developed recently for forensic application in criminal trials. The other technique, Restriction Fragment Length Polymorphisms (RFLPs, popularly called 'DNA Fingerprinting') was developed earlier, and generally has been used in more cases. Other, more established but generally less sensitive techniques include ABO blood typing and blood protein analysis, which often are used in conjunction with the more recently developed techniques. Both RFLP and PCR techniques have been put 'on trial' in preliminary hearings of criminal cases, and many of the questions we were asking about the practical identity and reliability of PCR when used under different circumstances of application have been raised and debated in the courts.

PCR has become increasingly familiar in the past few years, and nowadays one often finds explanations in newspapers and television. Partly because of the way the Simpson trial is likely to 'feature the most detailed course in molecular genetics ever taught to the US people,' (Lander and Budowle, 1994: 735). PCR (which is one of the techniques used by the prosecution in that case) is rapidly changing from a technical topic to one that is almost mundane.

In principle, PCR works by harnessing chemical tools that are said to be 'naturally' involved in cellular reproduction. To accomplish the procedure, laboratory personnel first collect a sample of blood, semen, saliva or other bodily material believed to contain trace amounts of DNA from a particular person. The sample is placed in a small test tube containing enzymes, chemical constituents of DNA, and other ingredients. This 'cocktail' as it is sometimes called is then heated above a particular temperature to separate ('denature') the double-stranded DNA into

---

[2] See Oteri, Weinberg, & Pinales, pp. 250-59.

[3] See Jordan and Lynch, 1992: 77-114 and Jordan and Lynch, 1993: 160-80.

[4] For an account of the development of and cultural significance of PCR, see Rabinow, 1992: 7-10; Rabinow, forthcoming.

single strands. It is then cooled to allow 'primers' in the solution to 'anneal' or latch on to the beginnings and ends of the single-stranded segments of interest. The ingredients are heated again, only this time to an intermediate temperature that enhances the action of an enzyme (polymerase), which is said to be naturally involved in cell reproduction. According to standard accounts of the procedure, the polymerase acts to synthesize a complementary sequence for each strand in the sample, thus doubling the amount of the segment of interest.[5] This is repeated as many times as needed, and ideally with each repetition, the initial amount of the segment of interest doubles. There is, of course, much more to it than this, and the procedure can prove difficult to perform.

The technique is often likened to a photocopy machine, because its main function is to 'amplify' (increase the amount of) an initial sample of DNA. Unlike a photocopier, however, PCR is said to work very selectively, as it copies only a specified sequence of nucleotides from the target DNA. It is less like a photocopier that reproduces an entire page, than like a word-processing function that locates, extracts and then copies a specific passage from a lengthy text. Imagine a computer file about a million pages long, containing a few billion characters (analogous to human chromosomal DNA, and with its sequence of four polynucleotide 'letters' A, C, G, T). A 'search' function would employ two sequences (primers), one of which is designed to match a sequence of characters at the beginning of the passage of interest, while the other matches a sequence at the end. (Note that in PCR the primers are complementary, rather than being duplicates of the DNA strand to which they latch.) Assuming the search function was designed specifically enough, a particular fragment would then be pulled from the text and another function would copy it in a geometric progression, doubling the number of copies with each iteration. This method of iterating the selected textual fragment is what is called 'amplification'.

DNA typing methods (both RFLP and PCR DQ alpha) are designed, in principle, to locate discrete regions of human chromosomal DNA which have the same beginning and ending sequences (which enables them to be extracted by the same primers). Researchers who develop these primers identify a relatively small number of chromosomal sites which vary greatly (are 'polymorphic') from one individual to another. These sequences vary in length and molecular weight in different individuals, and dozens of variations have been identified in the human population. Like other DNA sequences, polymorphic regions are said to be inherited, and can be traced through parental lines. For that reason, DNA 'fingerprinting' (usually RFLP) is commonly used in paternity cases. When using the PCR technique in criminal trials, forensic specialists isolate and amplify a

[5] The enzyme most commonly used in PCR applications, Taq polymerase, is derived from a microbe (***Thermus aquaticus***) discovered in Yellowstone Park geothermal springs. This microbe is able to live and breed under high heat conditions, and thus the polymerase extracted from it does not break down in the laboratory when mixed with samples containing DNA, when the sample is heated sufficiently to 'denature' the double-stranded DNA with each iteration of the procedure. This polymerase was patented by Cetus corporation, and is a key constituent of the PCR patent the company sold to Hoffmann-LaRoche in 1991 for $300 million. For a highly readable account of the discovery of Taq, and the controversies surrounding the commercialization of this microbe see Robbins 1994: 90-95. This article is one of many recent indications of the broad dissemination of PCR and stories about it.

specific, variable region of chromosomal DNA extracted from samples taken from a crime scene, and they compare these to results from the analysis of samples taken from suspects and/or victims. Forensic specialists then use 'probes', which are specially designed sequences which 'detect' each of the variants (alleles and genotypes) which have been found to occur at the specified HLA DQ alpha chromosomal region (Higuchi, et al, 1988: 543-46).

The DQ alpha system was developed by Cetus corporation, and has been extensively tested for forensic application by the FBI laboratories. The discrimination allowed by this system is not nearly as powerful as RFLP,[6] but PCR has the advantage of being applicable to tiny samples of bodily fluids found at a crime scene. According to the large technical literature on the subject, the DQ alpha system can be used to analyze samples of bodily material 'donated' by a perpetrator or victim at a crime scene. Such inadvertant donations can include spittle left on bubblegum, or on stamps and envelopes, tiny amounts of blood or semen, and hair follicles. It also enables analysis to be performed on badly preserved materials. Although they work differently, a battery of forensic techniques C PCR, RFLP and older techniques of blood, protein, and fiber analysis C often are used to compare samples found at a crime scene with those taken from a victim and/or suspect. An 'exclusion' is declared when no match is found, and when a match does show up, a estimate is made of the probability that such a match would occur in the population at large. From studies of population samples from different 'racial' groups (e.g., 'Hispanic', 'Caucasian', 'African-American'), forensic analysts have estimated the probability that each of the alleles and genotypes identified at the DQ alpha region will occur in the population subgroups. These estimates range from 0.0012 to 0.12 (Comey, 1988: 73-77, at 73).

Controversies about the forensic applications of RFLP and PCR have been discussed extensively in scientific and legal journals, as well as more popular sources, and the scope of these controversies has included the very question of whether there is (or should be) controversy in the first place. One government forensic specialist we interviewed claimed that there really is no controversy. According to him, what the press have covered is only a 'perceived' controversy. Similar sentiments are expressed in a recent article, tendentiously titled 'DNA fingerprinting dispute laid to rest' and conspicuously co-authored by a prominent geneticist (Eric Lander) and an FBI forensic scientist (Bruce Budowle). The article points out that until recently the co-authors represented (or were widely 'perceived' to represent) opposite sides on the (perceived) controversy (Comey, 1988: 73-77, at 73). We have no doubt that, perceived or not, the controversy will continue. Its announced 'closure' is likely

[6] RFLP differs from the PCR HLA DQ alpha system in a number of significant ways. It involves the location of a few different variable sites on human chromosomal DNA, and the use of radioactive probes to locate these. A sample is chemically fragmented, and then electrically 'driven' through an agarose gel which acts like a sieve to separate molecular constituents of the DNA by size. The probes mark the molecules of interest on the image derived from the gel columns, enabling profiles developed from different samples to be visually compared. When a match is declared at each of the particular sites assessed in the two samples, the probabilities of each match are multiplied together, giving a product that, in some calculations, has ranged into one chance in millions, and even billions, of the samples being from different human subjects.

itself to touch off further debate in the public arena.

One problem we face in our investigation of this controversy is that it is difficult to stay ahead of the public preemption of our possible 'findings'. To some extent this is akin to having one's findings 'scooped' by another researcher in one's own field, but in this case the 'scooping' is performed by our subjects of study. It is as though the news we once would have hoped to present about PCR and the DNA fingerprint controversy is continually being preempted by lawyers and judges summarizing court cases, journalists and scientists writing for the science press, and even the more vulgar voices represented in the tabloids and TV commentaries.[7] The problem is not so much that these commentaries announce what we would like to have gotten credit for 'finding out' about PCR. Instead, it is that they reconfigure the field in which we previously thought our study would have been situated. So, for example, by announcing that there no longer is (and perhaps never was) a ***real*** 'controversy' about forensic use of particular DNA techniques, Lander and Budowle problematize an assumption our study previously took for granted. If we maintain our initial assumption that there is a controversy, we run the danger of ending up on the losing side of a 'perceived controversy' about whether there is (or ever was) a controversy about DNA fingerprinting. Lander and Budowle both are key subjects of our study, as both have been involved in court cases and exchanges of arguments in the science press. Their collaborative article creates a new burden for us, however, which is to defend our initial treatment of the situation ***as*** a controversy. The situation might be likened to that faced by contemporary chemists or physicists who apply for funding to do experiments on cold fusion in the aftermath of the public closure of that controversy. The declared fact of closure can now be used as grounds for denying support for research that presumes the controversy is open, or might be reopened. Fortunately for our purposes, this (non)controversy left tangible traces throughout the courts in the United States, so that even if it is declared 'dead' as a scientific matter, it is likely to remain alive in other areas civil society. To begin to explore the value of the controversy for the sociology of scientific knowledge, we will discuss a particular case, ***New Jersey v. Richard Charles Williams***, which occurred in 1991. A brief review of some aspects of this case should provide a more substantive basis for introducing the sociology of knowledge machine.

**A Case: New Jersey v. Williams**

Richard Charles Williams was indicted in 1983 and charged along with co-defendant Thomas Manning with the 1981 murder of a New Jersey State Policeman, Philip Lamonaco. According to the prosecutorial brief, the defendants were both sitting in a blue Chevy Nova which had been stopped on Interstate Route 80 by Trooper Lamonaco. The prosecution alleged that Williams shot Lamonaco, but that the Trooper was able to discharge his own weapon after being fatally wounded. The Nova was found abandoned a few hours later, and blood was recovered from the passenger's seat, headrest and door panel. Ballistics evidence identified a gun that was recovered as the murder weapon,

[7] For example, during the O.J. Simpson trial, a TV 'tabloid' news show host (the much-maligned Geraldo Rivera) commented that the distance between the experts and the lay public had collapsed almost to nothing due to the incessant publicity about all aspects of the case.

and other evidence indicated it had been purchased by Williams that same day as the murder. His fingerprints also were found on items left in the car, which also were purchased that day. Williams and Manning remained fugitives before being arrested in 1984 and 1985, respectively, and they were jointly tried in 1986-87. Tests for blood type and enzyme markers presented at the trial indicated that the blood in the Nova could have come from either defendant, but not from the victim. The trial resulted in a hung jury, and Williams was then tried separately from Manning in 1991. Prior to Williams' retrial, the prosecution commissioned a new set of tests on the blood samples. One test employed newly-developed method using the PCR DQ alpha system, while others used older methods of blood analysis. The more sensitive RFLP method was not used, because the blood samples taken from the Nova in 1981 were judged to be of insufficient quality.

The phase of the case on which we focus is the pre-trial hearing. This hearing concerned whether or not the results of the forensic analysis should be admitted to the trial and presented before the jury. There are two reasons why this particular preliminary hearing is relevant for our purposes:

First, a practical reason: the materials are ready to hand. They include case summaries, written briefs and appendices on behalf of defense and prosecution, and court transcripts of the testimony of expert witnesses taken during a preliminary hearing.[8] These case materials are relatively compact, yet they are complex enough to suit the present state of the collaborative investigation we are conducting together with a group at Cornell University.[9] We should note that the documents and transcripts we shall examine from the case, ***NJ v. Williams*** are not suitable for a more extensive or detailed examination of testimony at such hearings. A more extensive collection of materials is being assembled as this project develops, and many of the other written, interview and observational materials we have collected informed our analysis of ***Williams*** and other cases.

There also are some 'intellectual' reasons for using ***NJ v. Williams*** in this paper. It was the first case in New Jersey in which the PCR DQ alpha test for analyzing criminal evidence was introduced into trial. At the time, New Jersey, like many other states in the US held preliminary hearings concerning the admissibility of DNA typing evidence under guidelines. This case was, in part, based on the precedent of ***Frye v. United States***, 293 F. 1013 (D.C. Cir. 1923). In the past several years an increasing number of states opted for an alternative (Federal Rules of Evidence) standard, and a 1993 US Supreme Court judgement (Daubert v. Merrill Dow, 1993) decided in favor of that option. ***NJ v. Williams*** was conducted under the auspices of ***Frye***, and like other such cases it highlights a couple of themes of special interest in social studies of science: relevance and consensus.[10] According to the ***Frye***

[8] The materials include the prosecution and defense briefs, plus transcripts of the direct- and cross-examinations of two expert witnesses.

[9] The group at Cornell is headed by Sheila Jasanoff, of the Department of Science and Technology Studies. Jasanoff is Principal Investigator of a research grant from National Science Foundation, Studies in Science, Technology & Society (Ethics and Values Studies) Program, Award # 9312183, 1993-94, subcontracted to Brunel University.

[10] In Garfinkel's terms, it is a 'perspicuous instance', the examination of which enables unique insight into the familiar intellectual themes in question (consensus, relevance). See,

standard, as interpreted in US courts, expert testimony based on a scientific principle or discovery can be admitted as testimony only when it has 'gained general acceptance in the particular field in which it belongs' (***Frye***, at 1014). Accordingly, a technique like PCR must be judged to have passed from the 'experimental' stage to reach a 'demonstrable' stage before the courts will recognize it. This standard invokes two familiar and problematic concepts: consensus and relevancy, both of which proved to be a source of contention.[11] These problems are easy to appreciate, especially for those of us who are familiar with scientific controversies more generally: 'consensus' is a concept for which it is difficult to specify 'hard' criteria, and for a technique like PCR numerous scientific fields can be argued to be 'relevant'. In the courts, consensus is indicated by testimony from representative scientists. According to the prosecution brief in Williams, not all scientists in a field count equally when it comes to representing consensus:

> Courts generally will not consider the entire spectrum of scientists, but will instead consider only those scientists whose scientific background and training are sufficient to allow them to comprehend and understand the scientific process involved and form a judgement about it.
> (Prosecution Brief, p. 23)

In prior court cases, and in our own research, scientists have given divergent assessments of PCR and how well it works. We have collected descriptions of PCR and assessments of how well it works from a range of people, including: directors and staff members in small university-based biology, physical anthropology, and population genetics labs; managers and staff scientists at biomedical research companies and commercial firms specializing in pre-natal diagnosis; and administrators, forensic researchers, and case specialists working at a large government laboratory. These descriptions and assessments vary considerably, and it often seems as though they are not describing the same technique at all. Testimony by expert witnesses for the prosecution and defense at numerous pre-trial hearings also indicates that a population geneticist testifying for the defense may ***know*** PCR in quite a different way than a forensic case analyst testifying for the prosecution. Both, however, can be counted as experts from a 'relevant' field who are competent to testify about the general acceptance and reliability of the 'same' technique. We should add to this, however, that our orientation to these differences is at odds with some court decisions. Take for example, the following pronouncements by Hon. William D. Mudd, justifying his finding in favor of admitting evidence from the PCR DQ alpha system during a preliminary hearing in the case ***California v. Moffett*** (1991):

> . . . it appears to me that the P.C.R. technique is not an issue. All of the evidence in front of me indicates that the P.C.R. technique at the DQ-alpha loci or the technique itself is not in question. In

for example, Garfinkel and Wieder, 1992: 175-206.

[11] The US Supreme Court attempted to remedy some of the problems associated with the ***Frye*** standard when it reviewed ***Daubert v. Dow Merrill*** in 1993.

> fact, all of the witnesses agreed that in certain spheres it is acceptable. And I'll comment on some exceptions in a moment. The question really is only its application in the area of forensics, and in that particular vein I'm going to indicate that I find that the particular field of acceptability must include the entire field, research, diagnostics, as well as forensics, because the same system, the same test, the same technique is being used in all of these areas.[12]

Judge Mudd then addressed a previous case (***California v. Mack***) in which the court decided against admitting PCR and RFLP. Mudd expressed incredulity that the judge in ***Mack*** was persuaded by the testimony of biologists and medical researchers who argued that the PCR DQ-alpha system was newer and more problematic in forensics than in their fields: 'I somehow find it patently offensive, almost, to have a research biochemist come into court and point a finger at forensics when he's never done any of the work or never even talked to people in the field, and yet that's exactly what's being done.' These comments address two related sources of trouble for deciding whether or not the scientific technique in question is accepted in relevant fields. One is the problem of determining if ***relevancy*** should be defined narrowly or broadly. Do experts in criminal forensics make up the relevant community, or does it also include scientists from a more expansive range of fields in which the techniques are used? A second question concerns whether testimony to the effect that DNA typing is accepted for specific diagnostic purposes should count as evidence in favor of its acceptance in forensics. According to a report by the National Research Council of the National Academy of Sciences, the two contexts of application differ significantly:

> DNA diagnostics usually involves clean tissue samples from known sources. It can usually be repeated to resolve ambiguities. It involves comparison of discrete alternatives (e.g., which of two alleles did a child inherit from a parent?) and thus includes built-in consistency checks against artifacts. It requires no knowledge of the distribution of patterns in the general population.
>
> Forensic DNA typing often involves samples that are degraded, contaminated, or from multiple unknown sources. It sometimes cannot be repeated, because there is too little sample. It often involves matching of samples from a wide range of alternatives present in the population and thus lacks built-in consistency checks. Except in cases where the DNA evidence excludes a suspect, assessing the significance of a result requires statistical analysis of population frequencies.
> (National Research Council**,** 1989: 53-54.)

[12] The People of ***the State of California v. Jessie R. Moffett***, 1991: 1007.

Judge Mudd decided that PCR was credible in forensics because it was well established in a number of other fields, and he seemed unwilling to credit the expertise of representatives of those other fields who testified that forensic applications were different, and less well established.

A transcript of Judge Mudd's summary remarks was presented in an appendix to the prosecutorial brief in ***NJ v. Williams***. It was cited in the brief as a precedent which demonstrated that the technique in question had been admitted as evidence in courts in other states on the grounds that it was widely accepted in the relevant scientific community. The prosecution brief also recited an impressive list of scientific and medical fields in which PCR had been used and accepted:

> These procedures have been used for years for reliable identification of genes in the broad areas of general research, genetics, infectious disease and cancer research, immunology, evolution, ecology, prenatal diagnosis, and transplants, and in the specific areas of detecting diabetes, sickle cell anemia, hemophilia, rheumatoid arthritis, multiple sclerosis, cystic fibrosis, muscular dystrophy, AIDS and AIDS research, thalassemia, Huntington's disease, Lyme disease, bone marrow grafts, paternity testing, missing persons identification and tissue typing. (p. 11)

The prosecutorial team used this list in order to demonstrate that PCR is a widely accepted practice. However, the list also can be used to justify enlisting experts from a broad array of 'relevant' fields to testify about the use and effectiveness of that technique.

Already, we can see that the courts assessing the novel technique in accordance with the ***Frye*** general acceptance and relevancy standards were faced with a set of interesting analytical problems. These problems have a family resemblance to several problems that face sociologists of knowledge who intend to trace the social distribution of knowledge, to determine disciplinary boundaries, and to decide at what point in its history a particular 'content' of science can be counted as stable and consensually accepted fact or 'black box' (Jasanoff, 1991: note 3). In light of these parallel concerns, there are two options we can consider at this point. The first would be to entertain the idea that the sociology of scientific knowledge (SSK) provides a source of expertise about these analytical matters which might even be of assistance to the courts. This option most likely would appeal to many participants in the field who believe that SSK can, or should, provide a normative basis for evaluating and reforming science and its public knowledge. The second option is to describe the way participants in Frye hearings practically address the 'sociology of knowledge' questions at hand. This option may seem less flattering to the science and technology studies community, as it does ***not*** assume that we are in a position to advise the courts when it comes to deciding when a given technique has become accepted in the relevant field. According to this view, the science studies literature does not supply strict criteria for deciding when 'consensus' exists in a field; instead, it tends to treat the construction of disciplinary boundaries and the

development of consensus as interesting topics of study and debate. The issue, however, is not that social scientists are incapable of delivering the knowledge the courts so desperately need. It is certainly possible that sociologists would be able perform systematic surveys of scientists and reconstructions of bibliometric networks which would help the courts develop more precise ways to handle questions about relevancy and general acceptance. If they did this, however, one set of sociologists would be supplementing the practical court 'methods' of interest to another set. Those of us who remain interested in how the courts constitute 'consensus' and how they resolve the difference between the uncertain 'research frontier' and the unquestioned 'core knowledge' in a field would be inclined to view any practical contributions by sociologists as part of the field of interest rather than as solutions to theoretical problems.[13] This is a deep issue, which has been discussed at length elsewhere, and for reasons that we hope will be apparent by the end of this paper, we are taking the second 'constitutive' option.[14] One may read the remainder of this paper as a demonstration of the ***point*** of taking that option.

**Discursively Certified Facts**

One of the salient features of court discourse is that matters of fact and procedure are certified through agreements between the adversary parties. A simple demonstration of the production and procedural consequences of such agreements is the following sequence from a transcript of ***New York v. Edward J. White*** (1992).[15] Mr. Cerio is the District Attorney, who is examining an expert witness (Dr. Word) for the prosecution, and Mr. Lupia is the defendant's attorney:

> MR. CERIO: Now, your Honor, at this time we'd move to have the Court recognize this witness as an expert in the field.
>
> THE COURT: Any objection to that, Mr. Lupia?
>
> MR. LUPIA: No, your Honor.
>
> THE COURT: The motion is granted. The Court will accept the testimony of Dr. Word in her field as an expert in her field.

This is a relatively formal instance of a more pervasive phenomenon: facts (in this case, the fact that Dr. Word is an expert in her field) are established through the absence of objection or rebuttal by the participating parties. Such agreed-to-facts (which include non-contested items of

[13] The distinction between core and frontier science is made by Stephen Cole, who earnestly recommends it as a way of moderating the 'relativism' in the sociology of science. (Cole 1992). It never seems to occur to Cole that at least some of the sociologists of science he criticizes treat the core-frontier distinction itself as a 'social fact' rather than a transcendental fact that reasonable sociologists should respect. Cole's blindness on this point may arise from his tendency to focus only on causal theses in SSK, whilst disregarding the 'constitutive' orientation that is prominent in ethnomethodologically informed lines of research.

[14] This position is sometimes given the name 'ethnomethodological indifference' Garfinkel and Sacks, 1970: pp. 337-366. For a discussion of the difficulties associated with this research policy, see Lynch 1993: Chp 4. For a clear demonstration of the differences between an 'instrumental' and a 'constitutive' orientation to the social field of interest, see Woolgar and Pawluch, 1985: 214-227.

[15] ***New York v. Edward J. White***, Madison Co. District Court, 1992: No. 92-14.

record) have procedural consequences.[16] This is a practical matter. It is imaginable that the judge could refuse to grant the D.A.'s motion, despite the absence of objection by the defense, but if he were to do so it would call for an explanation, and if none were given it would provide reason for complaint. Note that the judge gives no account for accepting the motion in the above sequence. It also could happen that Mr. Lupia would later challenge Dr. Word's expertise in her field, but the judge or District Attorney would then be in a position to ask why he passed on the earlier opportunity formally to contest the matter. A more regularly taken option by the defense attorney is to state an objection, either in response to a formal motion as in the above excerpt, or, as more often happens, to spontaneously object to what the adversary attorney or witness has just said:

> DR. WORD: . . . they set forth guidelines that needed to be followed for laboratories to be doing DNA testing and have now made this accreditation program.
> MR. LUPIA: . . . Judge, I'm going to make an objection relative to this particular line of questions of the American Association of Blood Banks. We're relying on some hearsay statements. They are about to be accredited. I feel it's inappropriate.
> THE COURT: Objection sustained. Hearsay. The answer is stricken in its entirety.

Note the objection here is not to a matter of fact, but to an alleged use of 'hearsay' testimony. In this case both Mr. Lupia and the judge give a specific legal account for the objection and for sustaining it.[17]

Agreement or absence of contestation among the parties enables a court hearing to move forward cumulatively, whereas formal objections and other challenges to a witness's testimony provide occasions for explicating the sources of disagreement. This is not a unique feature of court discourse. As Harvey Sacks once remarked in reference to ordinary conversation, 'members do not explore the

[16] See Sacks,. Schegloff, and Jefferson, 1974: 696-735.

[17] Shortly afterwards, Mr. Lupia states another objection (op. cit., note 25, Word/Direct, pp. 14-15):

> Q: And in relation to the guidelines and requirements as set forth by the AABB, would you tell us what is the present status of compliance by Cellmark?
> MR. LUPIA: Objection, your Honor.
> A: Sustained.
> DR. WORD: We're in compliance.
> THE COURT: The objection is sustained. You are not to answer.
> THE WITNESS: Oh. I'm sorry.

Neither party gives an account, but it can be argued that the parties are treating the reason for the objection and its being sustained as 'evident'. This sequence occurs very shortly after the objection sustained about hearsay, and the D.A.'s just prior question mentions the 'guidelines and requirements' about blood type analysis. Apparently, the relevance of the hearsay rule is evident to the defense attorney, judge, and DA (who retracts the question), but not the witness, who answer the question and then is told not to do so by the judge.

sources of their agreements'.[18] One might figure that this method for certifying matters of fact is remarkably weak in comparison to the heroic efforts in the natural sciences to subject matters of fact to rigorous testing and proof before counting them as confirmed (or at the very least, not falsified). The contrast is not as extreme as might be imagined, however, when the informal shop talk in laboratories provides the relevant basis of comparison.[19] We should also add that matters of fact are not simply 'let pass' unchallenged in courtroom dialogue, as they are often explicitly marked for certification. Although many of the matters so marked are not challenged, the very way they are explicitly laid out highlights how they are enunciated ***for the record***, as opposed to being 'presupposed'. Consequently, courtroom discourse often comes across to the non-practitioner as a language game in which trivial matters tend to be drawn-out and belabored. Those of us in the academic world can appreciate this point by noting how a court incorporates an item that we take for granted into its body of exhibits. This is from the direct examination of an expert witness for the prosecution in the case ***Florida v. Andrews*** (1987, p. 5):

> Q. All right. Professor Houseman, I will show you what's been marked for identification purposes as State's Exhibit A and ask you to look at it and tell me if you recognize it. Tell me what it is, Sir?
>
> A. Yes sir, I will. This document is what is called my Curriculum Vitae. This represents the activities, professionally, that I have been engaged in since receiving my degree. And it indicates the list of publications in the field of genetics that I have published under my name.

Note the way that Prof. Houseman is invited to 'look at' and identify the item, as though his C.V. were an object in need of inspection before being identified. A significant aspect of such exchanges of 'talk' is that they are oriented to a written record. Not only is the talk transcribed (or made available for transcription), it is ***spoken*** for the record. Witnesses who nod their heads, or otherwise indicate agreement without speaking, are instructed to ***say*** 'Yes' or 'This is correct', and as indicated above, some of their utterances may be 'stricken' from the record when deemed not to comply with the requirements of legal evidence. Moreover, interrogators tend to itemize matters for agreement, and to recite previously agreed-to matters when soliciting further testimony.[20] Each item is presented to the witness for confirmation before being formally certified as a matter of record. Inevitably, some matters are glossed and quickly passed over without being so marked, but the strikingly

[18] This quotation is taken from unpublished notebooks on agreement which were assembled by Gail Jefferson after Sacks's death in 1975. For a discussion of 'preference for agreement', see Sacks, 1987: pp. 219-225; Atkinson, and Drew, 1979; and Lynch, 1985: Ch. 6.

[19] Lynch, ibid., Ch. 7. This is connected to the more general theme of 'trust' which Shapin discusses in relation to the history of science. (Shapin, 1994).

[20] Some of the procedures for iterating the record are discussed in Bogen and Lynch, 1989: 197-224. Also see Lynch and Bogen, op. cit., note 5, Ch. 7.

'belabored' quality of courtroom discourse indicates a relative difference from other, more ordinary, occasions. (Professor Houseman appears to be no stranger to the system, as he gives an appropriately pedantic description of what his C.V. 'represents'.)

Although agreement is sufficient for certifying facts for the record, this is not 'mere' agreement. It is agreement in the context of an adversary system in which it can be assumed (although one should not always assume this, given the possibility of collusion between the attorneys and the court) that an attorney who ***can*** disagree ***will*** disagree with any allegation that tends to support the adversary case. They do not, of course, disagree with every allegation. In ***N.J. v. Williams***, for example, the defense argues that

> . . .the State has failed to prove that use of the PCR technique to analyze forensic evidence is capable of producing valid and reliable results. Further, far from being generally acceptable in the scientific community, the ***forensic*** use of PCR is at the center of controversy and continued experimentation within the relevant scientific field.
> (Defense Brief, p. 4)

The attorney does not contest, for example, the molecular biological principles associated with PCR, or the reliability of its use in, for example, prenatal diagnostics or paternity testing. Instead, her arguments contest the forensic uses of PCR, and (elsewhere in the transcript) some of the contingencies associated with its application to the particular case. It might be argued that the fact that the attorney does not contest certain matters indicates that there is good reason to assume the truth of the uncontested facts about DNA and PCR. While it may be the case that the attorney does not contest many matters because, for example, there is no longer any controversy about the double-helical model of DNA or about the reliability of certain applications of PCR, this does not demonstrate that these 'core' principles therefore reflect 'nature' and not 'social context'. In the courts, what is or is not contested has a great deal to do with what has been contested (successfully) in prior cases. It also depends on the availability of expert witnesses, the attorney's understanding of the technical issues, and assessments of what might prove persuasive in the particular trial. It is more of a question of what an attorney ***can argue*** than of what is or is not ultimately true. We return to this issue later.

**Credibility**

During ***Frye*** hearings, the direct examination of an expert witness typically begins with a ritual presentation of the C.V. The written document is offered as evidence of the witness's expertise, and the questioning leads the witness through an oral rendition of educational background, professional societies, numbers of publications, and selected publication topics. This precedes any questioning about the expert's assessment of, and involvement with, the scientific facts, principles, techniques and applications of technique in question. The prosecution in ***N.J. v. Williams*** called a total of nine witnesses. This is a rather large number, perhaps reflecting the fact that this was the first case in which the admissibility of evidence produced by the particular PCR application was 'tested' in

New Jersey. When introduced by the prosecution, the roster of witnesses looked impressive indeed. It included a group of distinguished scientists in a number of fields related to DNA typing, as well as a group of variously distinguished forensic scientists and practitioners. The prosecutorial brief presented this roster of witnesses in a roughly descending order, starting with the most prestigious figures and ending up with specialists with more practical claims to forensic expertise. The first figure mentioned was Henry Erlich, at the time the Director of the Human Genetics Department at Cetus Corporation, and one of the co-inventors of PCR. Dr. Erlich's vita included an impressive list of awards, and hundreds of publications. The second witness on the list, Dr. Michael Conneally, was described as a distinguished professor of medical genetics, neurology and probability, who had authored more than 300 publications. The third witness, Dr. Haig Kazazian, of the Johns Hopkins University School of Medicine was also well published (150 articles). Following this trio of 'notables', three scientists were mentioned whose vitae were less impressive, but who offered specific experience with related applications of PCR. These specialists included Dr. Henry Lee, who worked in the forensic science laboratories of the Connecticut State Police, and later became notable as an expert hired by the defense in the Simpson case. At the tail end of the list three other specialists were introduced, two of whom had a more direct role in performing and supervising the analyses of the blood samples submitted in the case.[21] These two were Dr. Edward Blake, a serologist at Forensic Science Associates, who performed the PCR DQ alpha testing, and Brian Wraxall, executive director and chief forensic serologist of the Serological Research Institute (SERI), who extracted the DNA from blood-soaked threads taken from the Williams' Chevy Nova. Blake's lab then analyzed the DNA. Wraxall also performed blood protein analysis on the samples. The prosecutor's description of Wraxall's credentials focused exclusively on his practical training and experience.

Consistent with the litany of academic and practical credentials, the roster of witnesses was deployed by the prosecutor in a kind of division of labor. Erlich and the other 'big shots' gave brief tutorials about molecular biology. They explained the principles involved in PCR, and the methods of population estimation used in conjunction with that technique. Each gave a positive, and largely unqualified, assessment of the validity and reliability of the forensic techniques in question, and of the application of those methods to the case at hand. The more notable forensic specialists like Henry Lee discussed the reliability and general acceptance (in forensic science) of the DQ alpha test, and they also reviewed the results produced by Blake's and Wraxall's laboratories. The prosecutor summarized these testimonies by mentioning that four of the witnesses he called

> . . . agreed that the procedures used by Dr. Blake were designed to produce scientifically reliable and accurate results and that the results obtained were correct. Positive and negative controls

[21] The third expert was Dr. Moses Schanfield, identified as the director of the Analytical Genetic Testing Center in Denver, Colorado. He was assigned more impressive academic credentials than Blake or Waxall, and he was called upon specifically to review the methodology used by Mr. Waxall.

> were properly employed and the tests were done blindly, i.e., without knowing the actual typing of defendant's or Manning's blood. . . . Moreover, the matching test results and the preservation of additional evidence indicate the objectivity of Dr. Blake's procedures.
> (Prosecution Brief, p. 14)

The prosecution brief also summarized the laboratory work performed and/or supervised by Blake and Wraxall, and the methods for estimating the probability that the matches that were obtained between analyses of Williams' blood and of the samples recovered from the Nova were due to chance. The descriptions included ideal-typical accounts of the procedures, as well as more case-specific mentions of how Dr. Blake and Mr. Waxall coordinated their analyses of the frozen threads, the way they treated the samples, and the controls and other precautions they employed.

If one were to read the prosecutorial brief alone, it would be difficult to imagine how the testimony given by these various experts could be impugned. The defense attorney nevertheless found ways to undermine the credibility of the experts and of the techniques they described. Although tied to the case at hand, many of his arguments were practical variants of general themes and lines of argument in the sociology of scientific knowledge literature. Two of them were prominent in the defense brief: (1) Vested interests and collusion, (2) Discrepancies between practices and written accounts.

**(1) Vested interests and collusion**

The defense attorney opened her summary with a recitation of the roster of prosecutorial witnesses:

> The prosecution called nine (9) witnesses. All were qualified as experts in various fields of molecular biology, microbiology, genetics, immunology, population statistics, polymerase chain reaction, forensic serology, forensic science, forensic biology, DNA molecular biology etc. Each of these witnesses was infirm either by reason of close association and economic and professional reliance upon Cetus and the test in particular or they were not competent to give an opinion in the forensic context of PCR.
> (Defense brief, p. 5)

The attorney went through the list of prosecution witnesses one-by-one, citing different reasons for imputing 'interests' or a lack of 'competence' to each. The interests imputed to several of the witnesses were linked together in a commercial conspiracy. For example, when discussing Henry Erlich, one of the co-inventors of PCR, the counsel made the point that Erlich was employed by, and held stock in Cetus Corporation, and that this company was in deep financial trouble. She quoted the company CEO's announcement that the company should aggressively pursue more alliances which will help us build our business and lower our risks'. She then alleged that Erlich's appearances as an expert witness was part of the company's efforts to promote the forensic uses of PCR (Cetus held the patent prior to selling its rights to Hoffman-LaRoche). Others of the

witnesses were also connected to Cetus, either as consultants or employees.

Many of the qualities mentioned by the prosecutor in support of the 'expert' status of the witnesses were converted by the defense attorney into grounds for suspicion and doubt. The prosecution had mentioned that some of the witnesses had testified about the reliability and general acceptance of DNA typing in dozens of prior cases. Although this was mentioned as evidence of the witnesses' experience, and of the fact that their testimony was accepted by the courts in other cases, the defense attorney treated it as evidence of the Cetus Corporation's efforts to promote its products. The impressive credentials of some witnesses were also impugned for being 'merely' impressive. For example, toward the end of the summary the defense attorney remarked that 'if a juror cannot quite understand allele drop-out or mixed samples, the issue should not be admitted because Dr. Erlich wears a five hundred dollar suit and has a C.V. four pounds in weight.'

The defense attorney also took rhetorical advantage of the difference between the credentials of the academic 'heavyweights' like Erlich, and those of the 'applied' specialists like Blake and Wraxall. Blake was portrayed as a defensive practitioner, resentful of the academic authority associated with critics of the scientific status of forensic investigations. According to the defense brief, Blake exhibited 'credibility problems' (p. 20) in his testimony because '[t]he vitriol he reserves for persons who do not agree with him was demonstrable in his demeanor as well as his testimony.' The defense attorney cited some of Blake's remarks about 'ivory tower' academics who criticize the state of the art in forensic research without taking into account the 'real world' problems faced in that field.

Whereas Ehrlich's credibility is attacked by associating him with the 'Cetus family', and his very prominence as a co-inventor of PCR is used as grounds for imputing interest to him, Blake is attacked for lacking such prominence:

> His scientific, educational background are undistinguished and his post-doctoral work sparse indeed. Whatever his technical expertise may or may not be, he is certainly not of the same caliber as the other State witnesses.
> (Defense, p. 20)

Wraxall also came off badly in the defense attorney's review of his credentials. The attorney cited the record of another case (***State v. Gentry***, 1991), quoting the judge's expressed doubts about Wraxall's lack of credentials (no Ph.D.), and about the 'integrity' of his testimony in a prior case. The defense did not specify a connection between the past problems with Wraxall's testimony and the present case, but she did cite the testimony of one prosecution witness (Dr. Schanfield) who, according to the defense 'consistently hedged with regard to the reliability of . . . Wraxall's test' and who 'characterized as Humpty-Dumpty' the 'method used by Mr. Wraxall.' Note that, in this connection the defense attorney preserves the reputation of 'the knowledgeable Dr. Schanfield' when using his testimony to attack the other prosecution witnesses.

The defense attorney not only imputed vested interests to each witness, she also recontextualized particular agreements between their testimonies:

> 'the close connection between Dr. Ed Blake/Forensic Science Associates and Dr. Henry Ehrlich/Cetus is apparent throughout the testimony. For this reason, Dr. Ehrlich knows how many cases (250) and samples (2000) Blake has handled. He states that proficiency testing is important for all labs and then hedges where Blake's lab (experienced and busy) is concerned even though he has memorized the source and number of earlier proficiency tests results Blake has participated in.'

The defense attorney's arguments are of course locally organized, and they seem purposively designed to rebut the prosecutorial case. They do not trace back to a coherent 'theory' of science. Although the attorney does attempt to link the various 'interests' she imputes to the witnesses to a common underlying factor (a direct or indirect connection to Cetus Corp.), in many respects her arguments are ad hoc, employing whatever she can dredge out of the records of prior cases, the testimony and records submitted in the present case, and publications in scientific journals and the science press. She goes through the roster of witnesses, one by one, only occasionally linking different witnesses under a single argument.

**(2) Discrepancies between practices and written accounts**

A common point of attack in criticisms of DNA typing analysis is the system of relays through which samples and analyses pass as they travel from a crime scene, through various laboratory units, and through the hands of different practitioners. The relays associated with police work are often given the name 'chain of custody', where the item in custody is not a prisoner, but a sample. Like a prisoner, a sample can prove to be an elusive party that exploits loopholes in the chain. Numerous hazards have been identified: mixing together of samples, contamination of a sample by the person(s) handling it, mislabeling or misreading the labels placed on sample containers, and deliberate sabotage and subterfuge.[22] Various checks and remedies have been set up to avoid these problems. They include the separation of laboratory regions in which different samples are handled, the use of disposable instruments (like pipette tips), strict protocols for handling and labeling samples, and arrangements through which each step taken and each inscription made by a laboratory technician is officially 'witnessed' by another practitioner. Record keeping becomes especially important, as lab notebooks become 'evidence' in a dual sense: they provide records the practitioner consults in order to keep track of the procedural steps and sample identities, and they provide a record that may later be used in court to demonstrate what was done with a specific sample. The latter sort of evidence is answerable to legal demands for an authentic, contemporaneous historical account of the laboratory procedures used. In the Williams case, the defense attorney took issue with the inadequacy of Dr. Blake's and Mr. Wraxall's record keeping:

> Dr. Kazazian was asked to review the work done in this

[22] According to the Defense brief, chain of custody issues would be discussed in the trial. Related issues about the handling of samples in the lab, and the communication of results from the lab to the court were discussed in the pre-trial hearing.

> case by Dr. Blake and he performed flawlessly. However, on the cross-examination regarding Blake, Wraxall and the raw data of the extraction of certain threads, even re-reading the data as to what was happening with the extraction of the control threads he could not understand what had occurred. . . . This might indicate that bench notes in this case are not complete and do not always reflect the events as they occurred during the processing of these samples.

Much has been written in social studies of science about the difference between embodied laboratory practices and the written notes, records and reports that variously refer to them. The lack of transparency between scientific reports and laboratory practices provided one of the original incentives for conducting laboratory ethnographies. The idea was to penetrate beneath the facade of writing produced by scientists and their publicity agents to investigate more closely what practitioners actually do in the private domains of their labs. Relatedly, non-transparency was a key theme in discussions of the difficulties that scientists often encounter when trying to replicate one another's results. In light of such studies, we can appreciate the difficulties the courts encounter when attempting to understand and assess the technical practices performed in forensic laboratories. For all their authority, judges are not always prepared to understand technical accounts of lab practices. In the present case, it seems that a provisional solution was devised by the prosecution, which was to set up a kind of relay team. As noted in the above quotation, Dr. Kazarian is positioned as an authoritative reviewer of Dr. Blake's laboratory research (and Dr. Blake in turn reviewed the work of Mr. Wraxall, and it is likely that both of them reviewed the work of unnamed technicians and staff scientists, who themselves may have followed a protocol through which one technician 'witnesses' the work of another).[23] The relay team was not simply a channel for transmitting 'information' from the lab into the courtroom, it was also a set up for certifying the credibility of what was not transmitted directly. At the near end of the chain, the credentialled Dr. Kazarian spoke on behalf of the unseen practices and less credentialled scientists and technicians who handled the 'raw data' at the far end. In the defense attorney's account, however, Dr. Kazarian's 'flawless' performance breaks down under cross-examination, as he is unable to give a precise account of how Dr. Blake and Mr. Wraxall produced their data. The attorney then circumvents Dr. Kazarian's mediation, and recites a litany of Dr. Blake's technical lapses and short-cuts.

> While Dr. Blake's testimony raises some questions as to his technical expertise (his failure to make certain entries in bench notes, the lapses of time between amplification and reading of the tests, notes added to bench notes after submission to Prosecutor for discovery, his lackadaisical attitude to the temperature problem in the thermal cycler,

[23] In ***Florida v. Andrews***, the defense counsel repeatedly asks the witness, who supervised work done by others, to describe just what he actually observed, as opposed to reconstructed from notes.

> no proficiency testing - too busy, doesn't want FBI regulation, his 'little story' with the evidence, etc.) there is no reason to doubt that he carried out the testing in this case at the state of the art ***as it now exists***. It is the defense contention that it is still premature and an untested method.[24]

In social studies of science it is asserted with formulaic regularity that actual laboratory work is 'messy', contrary to the 'cleaned-up' accounts in textbooks and published reports. This distinction between actual practice and idealized results offers the enterprising attorney a virtual guarantee that if she pushes hard enough, she should be able to find evidence of the indefensible or inarticulate practices which, when presented in contrast to an ideal-typical protocol, may be described as failures to conduct required controls and lapses in procedure.[25] Note that in the above quotation, the attorney adds to the litany of errors by asserting that such errors are ***normal*** features of state-of-the-art applications of PCR in criminal forensics. While this may excuse Dr. Blake from personal responsibility for sloppy work, it targets the technique that he and the other prosecution witnesses testified was generally accepted in the relevant scientific fields.

**Sociology of science at one step removed**

While discussing ***N.J. v. Williams*** and selected materials from other cases, we have emphasized points of convergence with arguments in the sociology of scientific knowledge. As discussed here, similar arguments played an instrumental role in an attorney's efforts to undermine expertise and expose gaps between protocol statements and singular performances. At this point we should mention that despite the defense attorney's efforts, the court decided in favor of the admissibility of the forensic results in question. Perhaps one could draw lessons from this about the relative ineffectiveness of 'interest' arguments, but it seems likely that on another day in another court, such arguments might have succeeded. If there are lessons to be drawn from these materials, we believe they have more to do with a familiar set of problems associated with the ***location*** of the sociology of scientific knowledge.

An argument that was a familiar part of ethnomethodology long before it became associated with actor-network theory and discourse analysis in the sociology of science, proposes that facts and modes of argumentation often used as ***resources*** for sociologists, are better viewed as aspects of the practices that make up the ***topics*** for ethnomethodological investigations. A special case of this distinction applies to the natural sciences: as a practical matter, the natural sciences ***incorporate*** a sociology of science into their accounts of fact and method. There are numerous ways to outline and develop this insight, and at least three distinct variants have been advanced in ethnomethodology over the years:

1) Perhaps the earliest and most familiar was Garfinkel's version of the 'reflexive'

[24] Defense Brief, p. 17. In a later phase of this study we shall examine the transcript of Blake's testimony.

[25] See Oteri et al., op. cit., note 6.

or 'incarnate' production of sociological methods and accounts (Garfinkel, 1967; Oxford: Polity, 1986). For example, Mannheim once described 'documentary interpretation' as a distinctive historical method. Garfinkel appropriated Mannheim's phrase to describe a commonplace 'method' of practical reasoning which also came into play in social (and by extension, natural) scientific research.[26] Contrary to the longer-standing construal of commonsense knowledge as a domain of prescientific 'notions' to be replaced by more precise 'scientific' standards and logically grounded procedures, Garfinkel proposed instead to describe 'the documentary method of interpretation' without prejudicing the case in favor of professional historical research.[27]

2) A less familiar argument was made by Harvey Sacks some 30 years ago, but only recently published.[28] This was the idea that a minimal requirement for replicating scientific observations and experiments is the communicative production of 'accounts of human behavior' which enable others to reproduce the relevant findings. In other words, the very practice of science necessitated the production of adequate descriptions not only of things observed but also of how to perform the observations: namely, praxiological descriptions of, for example, how to separate a beam of sunlight into its constituent rays by using a prism.[29] Sacks concluded that such 'sociological descriptions' no less than the findings with which they were associated were vindicated by the success of the natural sciences.

3) A more recent proposal was made by Garfinkel, and is summarized with his dictum that 'each natural science is to be recovered in the entirety of its identifying, technical material contents as a distinctive science of practical action' (Garfinkel, et al, 1989: p. 2). Read as a methodological proposal, this presents the would-be ethnographer of science with a severe participant-observation requirement (the unique-adequacy requirement of methods). More than that, however, it makes a substantive claim about the radical disunity of scientific practices, which implicates the endogenous 'sociologies' in, of and as the specialized sciences.

In science studies circles today, Latour's disavowal of any attempt to develop social explanations of science, such as those identified with the strong programme in the sociology of science, is the most familiar variant of the ethnomethodological argument that a practical 'sociology' is found ***in*** the sciences, prior to and independent of any effort by professional sociologists to study the natural sciences.[30] Woolgar's criticisms of 'interest' explanations, and Mulkay's and other discourse analysts' identifications of rhetorical forms used by natural scientists and sociological investigators alike, also make the point that social explanation is a commonplace,

[26] Ibid., p. 78. Mannheim, 1952: pp. 33-83.

[27] Note that the generic use of the term 'methods' to describe ordinary and scientific practices may have some advantages over the projection of commonsense 'theories' and 'models' into the ordinary person's (often unconscious) mentality, but it does misleadingly retain the analogical use of 'general science' terminology to cover 'commonsense reasoning'.

[28] See Sacks (1965) 'Appendix I: pp. 802-805.

[29] For critical discussions of Sacks's conception, see Lynch, op. cit., note 24, Ch. 6; Lynch and Bogen, 1994: 65-104; and Lynch and Jordan, 1995.

[30] Latour and Callon 1988: p. 9 sometimes referencing their debt to ethnomethodology on this point.

and circumstantially relative, feature of the discourse and practice of working scientists.[31]

All of the writers who have made proposals along these lines recognize that they present the sociologist of science with a problem: the very argument that the 'contents' of science are ***inherently*** social also removes those contents from the purview of general sociological theory and method. Paradoxically, a 'sociological' approach to the contents of science necessitates an abandonment of the standard procedures of the sociological discipline; either that, or it entails an ethnographic ***study*** of those standard procedures themselves. Many professional sociologists also recognize this when they refuse to acknowledge that ethnomethodology, actor-network theory, discourse analysis, and 'relativistic' sociology of scientific knowledge are part of the sociological discipline.

There are ways out of the paradox. One that Latour advocates is to turn to semiotics as a general system which can analyze a field of action without prejudicing the case in favor of 'human' and 'non-human' agencies and objects. This raises another set of problems, which has to do with the status assigned to semiotics as a transparent, neutral, and adequate system of reference that does not partake of the 'terms of the tribe' it helps to explain. Another solution is to invoke a distinction between the uses of language in the practical field investigated, and the ***analytic*** stance that makes a topic of such usage. Again, this stance is expressed by the often-repeated maxim not to confuse the methodological (and rhetorical) ***resources*** employed by practical actors with the ***topics*** of analytic investigation.[32] All too often, however, this distinction between topic and resource hardens into an epistemological ***warrant***, and becomes associated with an older image of the objective observer, stepping back from the world to observe it from outside. Alternatively, out of a recognition that no such warrant is in hand, the analyst topicalizes ***analysis*** itself by critiquing exemplary studies in the sociology of science, by writing in a confessional mode, or by violating conventions of writing (and the readers' expectancies that go along with them), in order to expose their tacit organization. In this way, the writing is de-naturalized (or auto-naturalized), but at the same time it loses the ecstatic 'engagement' with the work of the natural sciences that was once so crucial for the sociology of science.

The present paper explores another alternative, although in its present state it gives a bare and uneven outline of this alternative. Instead of trying to escape the problems associated with a sociological orientation to the 'contents' of science, we have focused on how ***the courts*** attempt to bring off inquiries of just that sort. The courts produce natural language inquiries which are oriented to lay audiences, while also attempting to come to terms with 'the very nature and content of scientific knowledge'.[33] Especially under the ***Frye*** rule, the courts faced the problem of how to settle questions about scientific

[31] Woolgar, 1981: 365-394; Mulkay, Potter, and Yearley, 1983, pp. 171-203.

[32] Garfinkel & Sacks, op. cit.; and Zimmerman and Pollner originally invoked the topic-resource distinction (Zimmerman and Pollner, 1970: pp. 80-103). Gilbert and Mulkay employ this distinction, without attribution, as a cornerstone of their discourse-analytic approach in the sociology of science (Gilbert and Mulkay, 1984).

[33] The quoted phrase is from the opening lines of David Bloor's ***Knowledge and Social Imagery*** (London: Routledge and Kegan Paul, 1976).

knowledge within a different system of argumentation and procedure. The practical methods devised by the courts may not be exemplary for sociology, but those methods for conducting and closing off inquiries about consensus and relevancy were substantive phenomena. Although the sociology of scientific knowledge may never solve its problems, it may find out how analogous problems are 'solved' in an arena that ***constitutes*** the order of affairs investigated by sociology.

## Bibliography


Atkinson, J. Maxwell, and Drew, Paul, (1979). ***Order in Court: The Organisation of Verbal Interaction in Judicial Settings***, London: Macmillan.

Bloor, David, (1976). ***Knowledge and Social Imagery***, London: Routledge and Kegan Paul.

Bogen, David and Lynch, Michael, (1989). 'Taking account of the hostile native: Plausible deniability and the production of conventional history in the Iran-contra hearings,' ***Social Problems*** 36: 197-224.

Bruno, Latour, (1988). ***The Pasteurization of France***, Cambridge, MA: Harvard University Press, p.9.

Cambrosio, Alberto, Keating, Peter and MacKenzie, Michael, (1990).'Scientific practice in the courtroom: The construction of sociotechnical identities in a biotechnology patent dispute,' ***Social Problems*** 37: 275-293.

Cole, Stephen, (1992). ***Making Science: Between Nature and Society***, Cambridge, MA: Harvard University Press.

Comey, Catherine T., (1988). 'The use of DNA amplification in the analysis of forensic evidence,' ***Crime Laboratory Digest*** 15: 73-77, at 73.

Garfinkel, Harold and Sacks, Harvey, (1970). 'On formal structures of practical actions,' in J.C. McKinney and E.A. Tiryakian (eds.), ***Theoretical Sociology: Perspectives and Development***, New York: Appleton-Century-Crofts, pp.337-366.

Garfinkel, Harold and Wieder, D. Lawrence, (1992). 'Two incommensurable, asymmetrically alternate technologies of social analysis,' In Graham Watson and Robert Seiler (eds.), ***Text in Context: Contributions to Ethnomethodology***, London: Sage, pp.175-206.

Garfinkel, Harold, (1967). ***Studies in Ethnomethodology***, Englewood Cliffs, NJ: Prentice Hall. (Oxford: Polity, 1986).

Garfinkel, Harold, Livingston, Eric, Macbeth, Douglas, and Robillard, A.B., (1989). 'Respecifying the natural sciences as discovering sciences of practical action (I and II): Doing so ethnographically by administering a schedule of contingencies in discussions with laboratory scientists and by hanging around their laboratories,' unpublished paper, Department of Sociology, UCLA, p. 2.

Gilbert, G. Nigel and Mulkay, Michael, (1984). ***Opening Pandora's Box: An Analysis of Scientists' Discourse***, Cambridge, UK: Cambridge University Press.

Higuchi, R., von Beroldingen, C.H., and Erlich, H., (1988). 'DNA typing from single hairs,' ***Nature*** 332: 543-46.

Jasanoff, Sheila, (1991). 'Judicial construction of new scientific evidence,' in Paul Durbin (ed.), ***Critical Perspectives in Nonacademic Science and Engineering***, Bethlehem, PA: Lehigh University Press, pp.215-38.

Jordan, Kathleen and Lynch, Michael, (1992). 'The sociology of a genetic engineering technique: Ritual and rationality in the performance of the plasmid prep,' in A. Clarke & J. Fujimura (eds.), ***The Right Tools For the Job: At Work in Twentieth-Century Life Science***, Princeton, NJ: Princeton University Press, pp.77-114.

Jordan, Kathleen and Lynch, Michael, (1993). 'The mainstreaming of a molecular biological tool: A case study of a new technique,' in G. Button (ed.), ***Technology in Working Order: Studies in Work, Interaction and Technology,*** London and New York: Routledge, pp.160-80.

Lander, Eric S. and Budowle, Bruce, (1994). 'DNA Fingerprint dispute laid to rest,' ***Nature*** 371 (October): 735-738 at 735.

Latour, Bruno and Woolgar, Steve, (1979). ***Laboratory Life: The Social Construction of Scientific Facts,*** London: Sage.

Lynch, Michael and Bogen, David, (1994). 'Harvey Sacks's primitive natural science,' ***Theory, Culture & Society*** 11: 65-104.

Lynch, Michael and Bogen, David, (1996). ***Discourse, Memory & Textual Practice:***

***The Production of History at the Iran-Contra Hearings***, Durham, NC: Duke University Press, Chapter 4.

Lynch, Michael and Jordan, Kathleen, (1995). 'Instructed actions in, of, and as molecular biology,' ***Human Studies*** 18.

Lynch, Michael, (1985). ***Art and Artifact in Laboratory Science***, London: Routledge and Kegan Paul, Ch. 6.

Lynch, Michael, (1993). ***Scientific Practice and Ordinary Action: Ethnomethodology and Social Studies of Science***, New York: Cambridge University Press, Chapter 4.

Mannheim, Karl, (1936). ***Ideology and Utopia***, New York: Harvest Books, p.271.

Mannheim, Karl, (1952). On the interpretation of ***Weltanschauung***, Ch. 11 of ***Essays on the Sociology of Knowledge***, London: Routledge and Kegan Paul, pp.33-83.

Mulkay, Michael, Potter, Jonathan, and Yearley, Steven, (1983). 'Why an analysis of scientific discourse is needed,' in Karin Knorr-Cetina and Michael Mulkay (eds.), ***Science Observed: Perspectives on the Social Study of Science*** , London: Sage, pp.171-203.

National Research Council**,** (1989). ***DNA Technology in Forensic Science***, Washington, DC: National Academy Press, pp.53-54.

***New York v. Edward J. White***, (1992). Madison Co. District Court, No. 92-14, July 28 and August 18.

Oteri, J.S., Weinberg, M.G., and Pinales, M.S., 'Cross-examination in drug cases,' in B. Barnes & D. Edge (eds.), ***Science in Context: Readings in the Sociology of Science*** , Milton Keynes: Open University Press, pp.250-59.

Pickering, Andrew, (1984). ***Constructing Quarks: A Sociological History of Particle Physics***, Chicago: University of Chicago Press.

Rabinow, Paul, (1992). 'Studies in the anthropology of reason,' ***Anthropology Today*** 8: 7-10.

Rabinow, Paul, (forthcoming). ***Bio-Technology Emergent: The Making of the Polymerase Chain Reaction***.

Robbins, Jim, (1994). 'The microbe miners,' ***Audubon,*** November-December, pp.90-95.

Sacks, Harvey, (1965, 1992). 'Appendix I: 'Introduction', ***Lectures on Conversation, Volume 1***, G. Jefferson (ed.), Oxford: Basil Blackwell, pp. 802-805.

Sacks, Harvey, (1987). 'On the preference for agreement and contiguity in sequences in conversation,' in Graham Button and J.R.E. Lee (eds.), ***Talk and Social Organization***, Clevedon, UK: Multilingual Matters, pp.219-225.

Sacks, Harvey, Schegloff, Emanuel A., and Jefferson, Gail, (1974). 'A simplest systematics for the organization of turn-taking in conversation,' ***Language*** 50: 696-735.

Steven, Shapin, (1994). ***A Social History of Truth: Civility and Science in Seventeenth-Century England***, Chicago: University of Chicago Press.

The People of ***the State of California v. Jessie R. Moffett*** (1991). Superior Court of California, San Diego County, May 21, No. CR 103094, Hon. William D. Mudd, Judge, at 1007.

Wigmore, John Henry, (1940). ***Wigmore's Code of the Rules of Evidence in Trials of Law***, 3rd Edition, Boston: Little Brown, '1043 at 29.

Woolgar, Steve and Pawluch, Dorothy, (1985). 'Ontological gerrymandering: The anatomy of social problems explanations,' ***Social Problems*** 32: 214-227.

Woolgar, Steve, (1981). 'Interests and explanations in the social study of science,' ***Social Studies of Science*** 11: 365-394.

Zimmerman, Don H. and Pollner, Melvin, (1970). 'The everyday world as a phenomenon,' in Jack Douglas (ed.), ***Understanding Everyday Life: Toward the Reconstruction of Sociological Knowledge***, Chicago: Aldine Publishing Co., pp.80-103.