

For Mike Lynch

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Much of the talk in the Iran-Contra hearings concerned how people's actions should be seen (see Bogen and Lynch 1989, 1996).¹

One came to know that Lynch was Garfinkel's student. The discipline was sociology, the phenomena were ethnomethodological (Garfinkel 1967) and his initial preoccupation was science—science in practice (Lynch 1985) and science as a practical accomplishment in the light of phenomenology, Wittgenstein, conversation analysis and Garfinkel's studies of work program (Lynch 1993). But then it was also the criminal law, from congressional committee hearings to plea bargaining to courtroom interaction, including the use of video. And then, like Sharrock, whose prowess he rivals, it was everything—a re-examining and interrogating of sociology's conceptual foundations, theoretical proclivities and methodological practices (Lynch and Sharrock 2003; Button, Lynch and Sharrock 2022), including ethnomethodology itself! Given that a proper accounting of this astounding corpus of inquiries is a project that exceeds my grasp, what follows is simply a record of my scholarly encounters with the work of this most accomplished sociologist. It starts with sociology in general, moves on to ethnomethodology with a bow to Wittgenstein, then takes in social constructionism before doubling down on criminal law. It borrows heavily from the second edition of *A Sociology of Crime* (2017) in which Lynch makes numerous appearances. It concludes with his brief reflection on ethnomethodology and politics.

“SOCIOLOGY'S ASOCIOCLOGICAL ‘CORE’”²

Like that of just about every other new academic hire in sociology departments my teaching load in 1976 included Introduction to Sociology. It was wholly typical of standard works in sociology, including innumerable introductory textbooks, to adopt an opening strategy that begins by presenting everyday members' knowledge in the form of factual propositions about the subject in question, to characterize this knowledge as “common-sense knowledge,” what everyone thinks they know about the subject, and then to contrast it with the “scientific” knowledge of the subject that the professional sociologist is about to impart—what is really going on (see contributions and discussions of the exercise by Sacks, and Garfinkel, in Hill and Crittenden 1968, 12–17; Zimmerman and Pollner 1970; Lynch and Bogen 1994, 70;

1 Hester and Eglin (2017, 226).

2 The title of Lynch and Bogen (1997).

Hutchinson et al. 2008, 19). “Virtually canonical in these preliminary discussions is a list of statements describing how sociological knowledge differs from commonsense belief” (Lynch and Bogen 1997, 486).

The practice of distinguishing between common sense and science trades on the equivalent distinction between the knowledge of the ordinary or lay person and that of the expert or professional. Indeed, it may be said that the professional’s claim to professional status rests on being able to show that their knowledge is different from, not to say better than, that of the layperson (Cuff 1994 [1980]). The justification of quantitative sociology rests on the assumption that the observable world of appearances is a poor reflection of the “real world” beneath the surface that only in its general outlines (both of distribution and trend) is detectable in observable appearances (Barrett 1978, 52). With it comes the correlative assumption that it is only the theories and specialized methods—preferably quantitative ones like survey research—of the professional social scientist that can reveal the order in the mess of observable particulars. Again, referring to introductory textbooks, Lynch and Bogen (1997, 486) write:

The contrast between science and commonsense usually plays two roles in the overall structure of the text. First, it counters the commonplace view of sociology as little more than a ‘science of the obvious’ ... ; second, it demonstrates the importance of rigorous empirical methods for establishing the special status of sociological knowledge.

But then the contrast routinely fails insofar as “the common-sense knowledge of everyday life is the unquestioned but always questionable background within which inquiry starts and within which alone it can be carried out” (Schutz 1962a [1954], 57). Or, in its classical formulation:

The thought objects constructed by the social scientists refer to and are founded upon the thought objects constructed by the common-sense thought of man living his everyday life among his fellow-men. Thus, the constructs used by the social scientist are, so to speak, constructs of the second degree, namely constructs of the constructs made by the actors on the social scene. (Schutz 1962b [1953], 6)

Professional sociological accounts of any social phenomenon already presuppose and depend on lay sociological accounts of that phenomenon, even as they strive to eclipse them. Moreover, to the extent that professional accounts do manage to have some purchase on the nature of actual social life, it can be shown that the currency of that life is always already inhabiting them. We must not

forget that students of social problems are masters of the vernacular (that is, they are members) before they begin their studies [and that their professional sociological] accounts are produced and justified as *further versions* [of members’ accounts] that have their own rhetorical and practical uses (Bogen and Lynch 1993, 230, 219, emphasis in original; see also Lynch 2008, 716).

ETHNOMETHODOLOGY

I'd been infected by ethnomethodology as a graduate student and did my best, to the extent possible, in each course I taught to convey the idea that "ordinary ('ethno') methodology consists of situated investigations by ordinary cultural members of practical action and practical reasoning" (Lynch 2001, 145).

From its beginnings in Garfinkel's classic studies ethnomethodology diversified into a variety of strands of EM and CA that Hester (2009, 240–43) distinguished as follows: studies of accountability, rule use and mundane reasoning, the two strands of conversation analysis comprising sequential analysis and membership categorization analysis, Garfinkel's studies of work programme (Garfinkel 1986) and what might be called the "Wittgensteinian ethnomethodology of mind" (for example, Coulter 1989). A cursory review of Lynch's prolific output of publications reveals that he has contributed to *all* the aforementioned strands, not least the studies of work programme in the form of the sociology of scientific knowledge or science and technology studies and EM's natural language studies (Lynch 1993, 2014), extending into studies of legal settings, as we will see below.

As just stated, the most prolific strand, conversation analysis, shows sequential analysis, membership categorization analysis and more recently the "Epistemic Analytic Framework" (Drew 2018). While Lynch has contributed to MCA (Lynch 2004), sequential analysis through Sacks, Schegloff and Jefferson (1974) has become a major field in its own right, although reservations have long been expressed about the extent to which it continues to remain firmly attached to its ethnomethodological foundations (Coulter 1983; Lynch 2000a; Sharrock 2000), a critical view that has been extended to include the contemporary subfield of epistemics (Heritage 2012; Drew, 2018). Lynch's own assessments have been central to these critiques (Lynch 2020; Lynch and Macbeth 2016; Lynch 2016, 12–18), which have come to include re-examinations of Sacks's *Lectures* themselves (see Button, Lynch and Sharrock 2022), the more purely methodological aspects of that opus having already been brilliantly conveyed in Lynch and Bogen's (1994) critical review of Sacks's "primitive natural science".

Wittgenstein and Garfinkel

From Wittgenstein we learn not to ask for the meaning of words, but to look for their grammars of use. We "move from epistemology to the sociology of science" (Lynch 1992a; see also 1992b). We "renounce the 'craving for generality'" that underwrites positivistic inquiry (Lynch 1993; 2001, 148, note 18; Hutchinson et al. 2008, chap. 3; Wittgenstein 1965 [1958], 17, 18) and Marxist dialectics, and seek instead to assemble reminders about the methods and knowledge we have, take for granted and so do not readily see. Furthermore, we seek not to solve sociology's problems, but to dissolve them by showing that they typically arise when "language goes on holiday" and we forget where its natural home is (Wittgenstein 1972 [1953], para. 38, emphasis in original). Bewitchments follow (para. 109).

Garfinkel's central recommendations are very akin to Wittgenstein's (Hughes 1977, 736; Sharrock and Anderson 1991, 62; Lynch 2007, 107). Ethnomethodology's "key assumption is that the production of observable social activities involves the local and situated use of mem-

bers' methods for doing such activities [and that] with respect to these methods, the mastery of natural language is paramount" (Francis and Hester 2004, 20). Moreover, like Wittgenstein's philosophical investigations, Garfinkel's "ethnomethodology offers a valuable form of therapy for social theorists" (Lynch 2001, 147; see also Lynch 2000b).

Both were indifferent to the project of reforming or correcting 'common sense' with constructed logical languages or (in the case of sociology) explanatory models, and both were more interested in explicating practical actions in ordinary as well as professional settings (Lynch 2016, 11, fn. 5).

Together these ideas entail the view that the ordinary, everyday life we live with and among others is not just "social" in character, but fundamentally sociological. That is, sociology is not simply the name of the specialized conceptual and methodological apparatus brought to the analysis of social life by professional practitioners of an academic discipline called sociology, but is a collection of such practices that all members of society engage in as a condition of living everyday life.

The study of common sense knowledge and common sense activities consists of treating as problematic phenomena the actual methods whereby members of society, doing sociology, lay or professional, make the social structures of everyday activities observable (Garfinkel 1967, 75).

By "everyday life" or "everyday activities" is meant all of social life lived under the auspices of what phenomenologists call the "natural attitude" or the "attitude of everyday life," whether in everyday or specialized settings. Moreover, professional sociology is underpinned by this lay sociology.

This approach entails the view that, far from being deficient in terms of professional sociology's formal theoretical schemes for explaining social action, persons' actions can routinely be seen to be intelligible in terms of the practical circumstances with which they engage. Furthermore, insofar as "*language is understood as practical action*" (Lynch 2014, 112, note 19; emphasis added) then, "following Wittgenstein, persons' actual usages are rational usages in *some* 'language game.' What is *their* game?" (Garfinkel 1967, 70, emphasis in original).³

This goes for ethnomethodology itself. What kind of a game is it in its own right? "Ethnomethodology is NOT a corrective enterprise. It is NOT a rival science in the worldwide social science movement" (Garfinkel 2002, 121). After all, the edifice of Cartesian reason stands. It's a social fact. That it is, is the remarkable thing! The attitude being adopted here is one of awe at the fact that such socially organized life exists at all. "Not how the world is, is the mystical, but that it is" (Wittgenstein 2007 [1922], 6.44, p. 107).

'That' is 'there,' like end-of-the-line in a supermarket service line or queue (Lynch 2009, 110)! The implied critique is then not of the edifice itself but of the standard (philosophy of science) accounts of what holds it up, the pretensions of formal rationality (Lynch 1999).

3 Garfinkel might have better said, "What is that game?" Because "a language-game is any array of human activities within which the use of language is embedded," then no language game is "anyone's singular 'possession'" (Coulter 2016, 2).

The point, to adopt Wittgenstein's phrase (Sharrock and Anderson 1991, 62), is to assemble reminders, in the form of perspicuous demonstrations and tutorials, of just what does hold it up. EM is filled with them, from service lines to discovering sciences.

While Schutz's work was a significant resource for Garfinkel's development of ethnomethodology (Lynch 1998, 25), Winch's *The Idea of a Social Science* (2008 [1958]), which brought Wittgenstein's philosophy into sociology (Lynch 2000b), was particularly influential for British ethnomethodologists of the Manchester school (Hutchinson et al. 2008, 34; Psathas 2008). W. W. Sharrock has written extensively in this vein. The natural-language philosophy of Gilbert Ryle and J. L. Austin was also drawn on by the Manchester school. Its kinship with phenomenology may be gauged by the fact that Austin describes his own work as a sort of "linguistic phenomenology" (Austin 1961, 130). The interpretative turn in sociology is thus related to the "linguistic turn" in analytic philosophy (Rorty 1967) and to the "practice turn" (Lynch 1993) in science and technology studies (Lynch 2014, 112, note 19).

SOCIAL CONSTRUCTIONISM

Although required to include "Intro" in my teaching load at the start, I was actually hired to teach criminology (about which I knew nothing!). I was in an undergraduate department of sociology and anthropology at a small Canadian university and, not wishing to reproduce mainstream, positivistic, cause-and-cure approaches to inquiry, but with little opportunity initially to introduce ethnomethodology proper into the curriculum—although, as I said, I tried to get in a few licks nevertheless—I brought a broadly interactionist/constructionist stance to teaching courses in not only criminology but also social problems, the sociology of suicide and the sociology of mental illness. Once it appeared, almost the first thing I assigned students to read in the mental illness course was Lynch's "Accommodation practices: Vernacular treatments of madness" (1983).

Taken as a whole, accommodation practices reveal the organizational construction of the normal individual ... Troublemakers(?) ... integration into society was not a cumulative mastery learned "from inside"; it was a constant project executed by others from the "outside" ...

My interest has been ... to investigate how individual *normality* is socially constructed ...

On a theoretical level, the analysis of accommodation practices enables us to consider the self as a social and normative construct as much as an internal province of operations. This is more than to say that the self is an *attribution* by others, since accommodation includes concrete actions to manipulate and maintain a semblance of normal selfhood on behalf of a troublemaker (Lynch 1983, 161, 162).

Like others of us Lynch went on to drop the language of social construction from his analytic vocabulary, instead turning his hand to the critical recovery of the concept's ordinary usage from its theoretical misappropriation. Given the latter's enduring ubiquity it is no small task.

If Kingsley Davis in 1959 was correct in saying, as George Homans put it, "we are all functionalists now," then it would be equally true to say, more than a half-century later, "we are all constructionists now" (see also Holstein and Gubrium 2008, v). Across the human sciences, but particularly in social theory, the language of construct and construction is pervasive and

can be found in various guises: from the sociology of social problems, deviance and crime to the sociology of scientific knowledge, and from cultural studies to semiology to poststructuralism and postmodernist formulations of deconstruction.

Not surprisingly the list of things said to be socially constructed is long. Lynch sums up the state of affairs this way: “While it may be impossible to define what adherents to the various constructivist approaches hold in common, at least they have the word ‘construction’” (Lynch 1998, 23; see also 14). Lynch is here alluding to the Wittgensteinian idea that an indefinitely extendable list of family resemblances may be found to characterize the various scholarly enterprises that adopt the term ‘constructionist’ to describe themselves, without it being necessary that they share any single characteristic in common beyond the word itself. Since ethnomethodology is often mislabelled as constructionist readers may wish to consult Lynch (2007, 110–17) for an elucidation of their differences, there in relation to the analysis of courtroom testimony. Thus ethnomethodology’s use of “production” should not be confused with constructionism’s use of “construction”: “to show how a social phenomenon is produced or achieved does not necessarily threaten its ontological status by showing that it is other than it seems” (Lynch 2008, 723).

Nevertheless, in stock usage, to say that a social problem, crime or concept is ‘constructed’ is to say that it is not simply given in the nature of things. It is at least the product of human conceptualization, generally expressed in the terms of language. Given that any phenomenon can be described in different ways, any particular description will reflect the point or purpose for which the description is made. In this sense all uses of language are ‘constructed’ (see Coulter 1989, 115).

This much, at least, of the meaning of “construct” should be uncontroversial. Moreover, to use the concept of construction to direct attention to the activities of those seeking to gain a hearing in the marketplace of public problems for a grievance they think needs remedying also seems pretty unobjectionable. There is an art to writing a cogent letter to the editor, an opposing editorial, an effective press release or, indeed, some draft legislation (see Berard 2015, 228–31 on expanding hate crime legislation to include new offenses, discussed in terms of “socially constructing the real”). For maximum efficacy one needs to *construct* such documents and the claims they contain carefully.

Insofar as claims-making is intended to persuade others, whether particular constituencies or the government or the public in general, of the salience of the claims made then it naturally enters the domain of politics. This only enhances the need for careful attention to the contingencies of claims making in such arenas. That this is so is attested to by the existence of courses of instruction and specialized personnel, whether in education or public relations, whose job it is to look after such matters (see also “political consultants” in Ibarra and Kitsuse 1993, 57, note 3; then see Bogen and Lynch 1993, 234, note 19). Whether one is making claims or responding to them they are necessarily attuned to constructing their communications effectively for the purposes at hand. If the constructionist sociologist wants to use “construct” and “constructing” in the aforementioned ways, then it is hard to see how anyone might object as the usage is no different from the stock use of those words in those contexts of everyday life.

Unfortunately, the professional sociological obsession with producing “theory” with its attendant “craving for generality” leads the practitioner to take the language of constructing

“on holiday” where it gets into all sorts of trouble. Perhaps the fatal step is to switch from the verb “construct” to the abstract noun “construction.” For, while nobody is in any doubt about the meaning of the expression “construction site,” to say that the labels “drug problem,” “wife battering,” “Montreal Massacre,” “child abuse” or “serial killer” are “constructions” leads very quickly to the supposition that there is something fabricated about them. And what “fabricated” means here is not simply “built” but “made up” or “invented” (see Lynch 1998, 27; 2008, 716–17). That is to say, being “constructed” they are not really real. Instead they are exaggerated or slanted or misleading or distorted or organizationally self-serving versions of the “real thing.” Indeed, prominent practitioners of social constructionism such as Joel Best (1993; 1995) and Philip Jenkins (1994, 226–27) are content to opt for a “contextual constructionism” over a “strict” version even while, in Best’s case, acknowledging the force of the “strict” position (Best 1995, 352), and betraying a certain frustration at having to deal with the issue at all (Manning 1998, 160). That is, they are content to say both that social reality is constructed and that such constructs may or do stand in some less than fully faithful correspondence to the real thing. They want to have their constructed cake and eat it too.

But one can’t have it both ways. Either reality is socially constructed or it is not. To try and have it both ways is to engage in what Woolgar and Pawluch (1985) trenchantly call “ontological gerrymandering” (although see Bogen and Lynch [1993, 234, note 21] for the limitations of their argument). Rather than rendering claims-makers’ activities, including their coining of labels for the issues that concern them, in professional sociology’s theoretical language of “constructing reality” we should return them to the institutional contexts in which they arise, live and have their being so that it can be seen what they are doing just there. Abstracting them from their homes for the purposes of a misbegotten sociological metaphysics or epistemology is really a very strange thing to do. It “*is to demand something extraordinary*” (Bogen and Lynch 1993, 220; emphasis added).

Coleman and Sharrock (1998, 106, note 6) point out that social constructionism tends to treat “actors as ‘reality dopes’, i.e. as though they were persons who conducted their everyday affairs on the basis of a philosophical theory, namely that of ‘naïve realism’” (Coulter 1989, 113), a criticism that Pollner’s (1975) work on reality disjunctures (and Eglin’s 1979 based on it) does not escape. Coleman and Sharrock are here coining “reality dope” on the model of Garfinkel’s judgmental, cultural and psychological dopes, and on Bogen and Lynch’s (1993, 221–22) likewise derived label of “philosophical dope” (see also Lynch 2012). Bogen and Lynch’s paper is particularly acute in dissolving the problem.

Stripped of their philosophical trappings and the rhetorical structure of their explanatory form (Woolgar and Pawluch 1985) the descriptive content of constructionist accounts can be illuminating and instructive (Bogen and Lynch 1993, 225). Whereas we may be inclined to think that social problems, deviance and crime are bad because they are bad, the constructionist perspective reminds us that somebody has to define the actions in question as bad, and then work to get that definition accepted in society more generally. Their “bad” status, that is, is the product of enterprise, involving claims-making. Furthermore,

The fact that social problems may always be bound to temporal and practical circumstances does not make them unreal, nor does it justify an invariant attitude of skepticism toward them. So far from arguing in

favor of a remote epistemological attitude toward the field of social problems research, we would rather encourage further research about the discursive practices through which social problems manifest in specific social and historical circumstances (Bogen and Lynch 1993, 231).⁴

CRIMINAL LAW

As I've said, courses in criminology—Hester and I re-named them Sociology of Crime from the perspective of criminalization via the law, the courts and the police—were my bread-and-butter teaching throughout my career. It was hard to miss Lynch's extensive contributions to this field. For a detailed review see Dupret, Lynch, and Berard (2015a). See particularly Lynch's "Law courts as perspicuous sites for ethnomethodological investigations" (2007), Lynch, Cole, McNally, and Jordan (2008; see below), Dupret, Lynch, and Berard (2015b), Lynch (1982) and Brannigan and Lynch (1987). (See also Burns, this volume).

Lynch et al.: determining crime through DNA fingerprinting

Nearly half a century after Garfinkel's jury study we find the same ethno-phenomena in play but in relation to a new feature of criminal justice. DNA profiling or fingerprinting has become the "gold standard" of forensic evidence whereby suspects are revealed as criminals. It has become a "truth machine," widely regarded with the same breathless awe as once greeted fingerprinting itself. For example:

Leo Adler, a Toronto lawyer who specializes in DNA cases, said most lawyers simply don't believe they can beat a DNA case and seek a plea bargain. "I would say that 99 times out of a hundred, nobody is fighting it any more," he said. "I suspect that there are lawyers whose clients say, 'Hey, it wasn't me,' and the lawyer says: 'You're going to go down, so let's work something out'" (Makin 2010).

Born in science DNA fingerprinting has enhanced the role of science in legal proceedings, bringing with it challenges for courtroom personnel, not least juries in criminal trials. Just how did DNA profiling become a "truth machine," and on what does its probative value actually rest? Is it simply a matter of the claimed hardness of science's data and methods, or does the science rest on an unacknowledged assemblage of "soft" social considerations that make its "hardness" possible?

In *Truth Machine* Lynch and his colleagues address both the historical and sociological questions. Their 400-page book is the result of a series of historical and ethnographic investigations over a fifteen-year period between the late 1980s and the early 2000s primarily in the

4 Such discursive practices include: the language used in junk mail sent to potential contributors to single-issue causes; the arguments and counterarguments used in dialogues between callers and radio talk show hosts; slogans shouted at protest marches and pasted on bumper stickers ["Meat is Murder"]; journalistic summaries of arguments on both sides of a public controversy; or the arguments used in a class action suit in which the plaintiff charges a company with having created a public hazard by releasing wastes into a local river (Bogen and Lynch 1993, 227–28).

United States and Britain. The methods used included interviews, observations, participation at laboratories and courtrooms, conversation analysis and documentary reconstruction. “Although focused on specific legal decisions, legal briefs, trial transcripts, and selected videotapes of the O. J. Simpson trial, our study attempted to encompass a much broader scope than usually is the case for detailed studies of text and talk” (Lynch, Cole, McNally, and Jordan 2008, xv). It particularly deals with cases in which DNA profile evidence was challenged in court. Such challenges put in question the status of “science” and the expertise of the “expert,” in relation to the “common sense” of the “ordinary person,” the “reasonable man” presupposed as the arbiter of legal decision-making in the Anglo-American criminal justice system. Rather than approaching the relationship of science and common sense in decontextualized, philosophical terms, the study addresses how these terms are deployed in legal proceedings.

Such pragmatic construals may or may not compare well with philosophical or sociological conceptions of science, but the important point is that they *constitute what counts* in the legal system as science and expertise. Such constitutive work and its pragmatic effects are phenomena for this study (ibid, 16; original emphasis).

The term “deconstruction” has come to be used to describe the way expert evidence is taken apart in courtroom examination. Though similar in meaning it should not be confused with the post-modernist concept. Thus,

consistent with the research policies in the field of ethnomethodology, it is necessary to view courtroom deconstruction as an interactional production, and not as a method for unmasking a reality that supposedly lies beneath the construction of expert evidence ... it is itself a contestable (and frequently contested) source of evidential claims and counterclaims (ibid, 20).

The Smith case is particularly revealing of how “the scientific credibility that DNA typing currently enjoys has much to do with the administration of objectivity” (ibid, 141).

The Smith case—chains of custody and administrative objectivity

The Smith case involved a rape and murder. Much of the prosecution’s case turned on using DNA fingerprinting to match the accused’s blood and semen with those in stains found at the crime scene. The defence/defense held that evidence up to scrutiny by documenting the complex journey travelled by the evidence from crime scene to courtroom in the course of which it was constituted as evidence. “Much in the way a suspect passes through a series of stages before becoming a criminal, crime scene and suspect samples pass through a series of stages on the way to becoming criminal evidence” (ibid, 121). What the study brings out is how the credibility of the scientific evidence rests upon a whole series of administrative tasks that have

nothing to do with the science itself, but with what the authors call “the administration of objectivity.” This series of tasks is known to the personnel involved as the chain of custody.

The chain is made up not of tandem repeats of identical elements, but of representations of many different kinds: denatured material extracts, designer fragments, gel images, paper transfers, radioactive markers, x-ray exposures, visual matches, numerical codes, and statistical estimates. Expanding the chain so that it extends beyond the lab, we find such items as tamper-evident bags, courier services, forms, signatures, and oaths. And in addition to metaphoric bar codes inscribed on autoradiograms, we find real bar-code stickers fastened to test tubes, records, and other items. These bar codes are organizational artifacts used for tracking the identity and assuring the integrity of samples. They are rails of justice: materials that facilitate the legally accountable transport and transformation of evidentiary cargo from one designated staging point to another ... Throughout the process, bureaucratic records stand as documents of identification, certification, and organizational memory: a signature, appropriately inscribed on a form by the relevant official, performs witnessing, independently of what the official may actually have seen or heard ... Representation unquestionably takes place, but not in accordance with a single rule or set of criteria, and never with the certainty demanded by the metaphysicians (*ibid*, 132–34).

The credibility of the DNA match presented at the trial rested on an acceptance of the various statements, attestations, and official certifications, embedded in, and embodied by, the paper trail. And so, while it might seem as though science, after being subjected to challenge, triumphed at the end of the day, its victory was sustained and supported at many points by a small, mostly hidden, labour force of bureaucratic officials, clerks, and lorry drivers (*ibid*, 132).

The reference to lorry (aka truck) drivers is interesting. Cross-examination revealed that a clerk had used a photocopy of a form with his signature already on it when certifying the proper transfer of a blood sample from one forensic lab to another. The defence demanded the original form, but since there wasn't one the trial was delayed while the prosecution contacted the clerk and the courier service involved.

According to the courier service's records, the sample in question was moved by four different lorry drivers on different legs of its journey ... The Crown attorney's office located the four lorry drivers and two controllers working with the courier firm and arranged to have all of them [attend court], along with the clerk ... This now-expanded group of agents was prepared to testify that the evidence sample moved through an unbroken chain of transfers on its [journey] (*ibid*, 131).

As it turned out the defence accepted the integrity of the evidence on the basis of other testimony so that “the clerk and lorry drivers no longer were required to play their cameo roles at the Old Bailey” (*ibid*, 131). Smith was duly convicted and imprisoned. The ethnomethodological-sociological point, however, is not to bring the verdict into doubt but to show in a perspicuous manner how the scientific objectivity of the evidence rests upon the administrative objectivity of the chain of custody which rests upon the practical reasoning of all the actors

involved, *in any case*. Smith's criminality is the interactional accomplishment of the criminal justice system's management of a diverse, if not blinding, array of practical contingencies.

[W]hen we consider the resources (scholarly, financial, etc.) that feed into big court decisions, as well as the consequences of such decisions, . . . instead of proposing what judges should know about the sociology of science, we may instead hope to enrich the sociology of science by examining how judges articulate phenomena of interest to our field (Lynch 2014, 109).

CONCLUSION: POLITICS

My final contribution to ethnomethodology—which I did in time manage to teach, as a fourth-year seminar—was a plenary address given to the International Institute of Ethnomethodology and Conversation Analysis in 2017. It was devoted to reviewing my own efforts to understand the field over the previous almost fifty years. The substantive part of the talk began by acknowledging that Mike Lynch had beaten me to the punch in his 2009 article “Working out what Garfinkel could possibly be doing with ‘Durkheim’s aphorism,’” a study I had read only five weeks before giving the talk. Accordingly, I began with Garfinkel’s formulation of the aphorism: “The objective reality of social facts is sociology’s fundamental principle” (1996, 10).

For its investigations, ethnomethodology took this to mean [that] the objective reality of social facts in that and just how every society’s locally, endogenously produced, naturally organized, naturally accountable, ongoing, practical achievement, being everywhere, always, only, exactly and entirely members’ work, with no time out, and no possibility of evasion, hiding out, passing, postponement, or buyouts is *thereby* sociology’s fundamental phenomenon (Garfinkel 1996, 11).

Through his remarkably productive corpus of writings, Lynch has been clarifying what this and other formulations of ethnomethodology could mean as the serious challenge to conventional sociological wisdoms and certainties that it is.

I finished the talk by effectively confessing that I was giving up ethnomethodology for politics. But here too I could not avoid taking note of what Lynch had himself already said about ethnomethodology’s *own* politics. To the degree that it may be said to have a politics at all that politics is antiadministrative.

By this I do not mean that ethnomethodologists necessarily are hostile to administrators. Instead, the idea is that no amount of foresight or planning will ever be sufficient to guarantee the effective realization of administrative schemes. This lesson applies to reformists no less than to reactionaries (Lynch 2008, 728).

For a would-be political activist it is a reminder about rules’ limits that is both cautionary and liberating at the same time. Good on yuh, Mike!

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