

# A tale of two laboratories

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## *Abstract*

Lynch is highly regarded in SSST for his studies of scientific practice and is recognised as making original contributions that have developed and evolved the discipline. However, what may be less understood is that the ethnomethodological studies he has undertaken implicitly bring into question predominate epistemological orientations in SSST. A case in point, drawn from the formative years of SSST, is the contrasting studies of scientific laboratories conducted by Lynch and by Latour and Woolgar. These studies are often run together as equivalent ground-breaking studies of the social organisation of the scientific laboratory that powerfully demonstrate for SSST an ethnographic approach to the study of science and technology. However, what is less observed is the difference between Lynch's ethnomethodological approach and the social constructionist approach of Latour and Woolgar's ethnography. This difference has significant epistemological and methodological consequences for SSST in as much as ethnomethodological investigations can compromise the constructionist tendency displayed in its formative years and which is still very much in evidence today, though in different guises to its original manifestation. The manner in which Lynch articulates his laboratory study will, thus, be shown to call into question many of the now commonly accepted epistemological underpinnings of Latour and Woolgar's study. This, and the distinctive ethnomethodology of Lynch's study will be drawn out through a point-by-point comparison of the two examinations and lessons relevant for SSST today will be drawn.

## INTRODUCTION<sup>1</sup>

Michael Lynch is widely known in the field of Social Studies of Science and Technology (SSST) for his research and administrative contributions. He was the editor of its prestigious 'house journal' *Social Studies of Science* from 2002 until 2012, and the President of its major organisational organ *The Society for Social Studies of Science* from 2007 until 2009. His research has ranged across several important themes within SSST such as representation, visualisation, and discursive interaction. His study of scientific practice (Lynch 1985) contends with the well-known study by Latour and Woolgar (1979) as the first ethnographically based

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1 I am grateful to Douglas Macbeth and Oskar Lindwall for their comments and editorial administrations on an earlier draft of this paper.

study of ‘science in action’ within scientific laboratories<sup>2</sup>. Lynch’s work is, however, not just acknowledged as making significant contributions to SSST, he is also acknowledged as making some of the most significant contributions to contemporary ethnomethodology, attested to by the lifetime achievement award he received from the Ethnomethodology and Conversation Analysis caucus of the American Sociological Association. His book, *Scientific Practice and Ordinary Action* (Lynch 1993), addresses the interconnections between ethnomethodology, and what SSST was then known as, Social Studies of Science (SSS), proposing how each can draw on the other in their respective examinations of science and technology—the book was sub-titled *Ethnomethodology and Social Studies of Science*.

## SSST AND ETHNOMETHODOLOGY

On the face of it, there may appear to be overlaps between SSST and ethnomethodology. Researchers in both areas have developed interests in how scientific practice is replete with ordinary everyday practices, and both have questioned received versions of science. However, the mutual interest that ethnomethodology and SSST have in science, and the bridge between the two that Lynch may seem to represent, is, like beauty, only skin deep. Although SSST is increasingly adopting the traditional sociological topics of gender, race, and class in its investigations of the social organisation of science and technology, its predominant grounding has been a *social constructionist theory in social science*.

There are important points of difference between ethnomethodology and social constructionism that make the two incompatible. For ethnomethodology, what is vernacularly describable as science is organised and achieved in courses of practical action of theorising, experimenting, writing up, and other routine matters that make up ‘the work of science’. Social constructionist studies, however, continue the familiar sociological interest in professionally determining the ‘meaning’ attached to science and technology and making ‘discoveries’ about the status of scientists’ and technologists’ practices. In this paper, I want to return to the early study by Lynch of laboratory science and contrast it with the study by Latour and Woolgar in order to draw out how ethnomethodology and social constructionism stand in relationship to one another. However, there is also another purpose. Lynch’s study was ground-breaking for ethnomethodological studies of work and for ethnomethodological studies of science. Latour and Woolgar’s study is no less ground-breaking in and for SSST. However, if they are read together, Lynch can be found to be exemplifying what an alternative sociology can amount to, while, despite the radical claims associated with SSST, Latour and Woolgar preserve sociology as business-as-usual. Although not designed to throw down a gauntlet to SSST, Lynch’s early study is suggestive of a different path that SSST could have followed to that of constructionism—the path that was taken by Latour and Woolgar’s early examination.

Obviously, decades have passed since these studies were conducted. Both were early implementations of their respective core interests in either ethnomethodology or social constructionist theory. It might well be the case that if they returned to their respective studies the au-

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2 Lynch acknowledges that Knorr-Cetina (1981) was also conducting an ethnographic study of a protein chemistry laboratory during the same time period Lynch and Latour and Woolgar conducted theirs.

thors might want to amend, enhance, or in some other way re-evaluate them. However, such retrospective considerations are beside the point for this present consideration. A contrast between the two studies provides an opportunity to reflect upon the constructionist path trodden by SSST and how an alternative path was, and still is, possible.

## WHOSE POINT OF VIEW?

Lynch's and Latour and Woolgar's studies were both conducted in laboratory settings. Both studies described how the actions and interactions of the scientists they observed as they went about the scientific business of their laboratories were marbled through with practices of reasoning common to the organisation of everyday action and interaction. Both studies make visible how these everyday matters do not figure in scientific descriptions of the results of the work of the scientists. However, the two studies viewed the settings and the actions and interactions observed from two different points of view. Latour and Woolgar's viewed the settings and the actions and interactions from the point of view of *sociology*. Lynch viewed them from the "*actor's point of view*". These different viewpoints result in divergences in the description of the matters and issues that the studies had in common, and importantly, how SSST could engage with scientific renditions of 'facts' and 'discovery'.

Turning to Latour and Woolgar first, they dispense with the idea that science is concerned with making discoveries about objects that exist independently of the procedures used to reveal them: "we do not conceive of scientists using various strategies as pulling back the curtains on pre-given, but hitherto concealed truths. Rather, objects (in this case, substances) are constituted through the artful creativity of scientists." (Latour and Woolgar 1979, 129). Latour and Woolgar are, in effect, challenging a received version of science. While scientists, themselves, may account for their activities as making discoveries about physical facts that have an independent existence—that they are 'out there', awaiting discovery—such an account also coheres with the way in which science is portrayed in everyday accounts, and often in popular representations of science in various forms of media outputs. Indeed, Latour and Woolgar recognise this when they go on to describe how this view of science is so culturally embedded that it caused them difficulties in describing scientific activity *without* giving the impression that science is concerned with making discoveries. Throughout their book, Latour and Woolgar joust with this understanding of science. They pit a theory that science is the constructed product of social processes, such as inscription, against the understanding of 'discovering sciences'. As constructions, facts are not 'things' that can be discovered, and consequently, the natural sciences cannot be conceived of as 'discovering sciences'. Rather than 'facts' awaiting discovery, 'facts' are social constructions.

Latour and Woolgar are not denying that objects, in their case, substances, do not have solidity. They are not arguing that physical objects do not exist independently of descriptions of them—that we live in a vague physical world that does not become concrete until described, and which then melts away and is concretely reformed when described in other ways. Rather, they intend to convey that the *meaning* those "solid" objects have is socially constructed. We should, however, refer to *meanings* because in chapter three they describe the construction of

a fact, TRF(H)<sup>3</sup>, and stress that what this fact is, is dependent on what it differently means to various and different networks of scientists.

However, Latour and Woolgar's depictions of science regarding 'fact', 'discovery' and 'meaning' bring them into conflict not only with a received version of science but also with their use in natural language. In the everyday world, we do have the concepts of 'fact' and 'discovery' and natural language users are able to apply them in understandable ways in accounts and descriptions of their own and other's actions. Thus, we can say that we discovered that our partner was unfaithful, and we know that this description will be understood. We can also say, that even though we did not know it at the time, we later found out that all along it was the fact of the matter, and also have *that* description understood. That it was the case is not dependent on finding it out or retrospectively constituting it as a fact. It was a fact waiting to be discovered, and if it was not discovered it does not mean that it was not a fact. Simply, 'fact' and 'discovery' are natural language concepts which are understood and used by competent members who in their use display their mastery of natural language.

To describe the discovery of an unfaithful partner as a construction would be to describe it as some sort of fabrication, as lacking substance. However, it might turn out that 'in fact' there was no unfaithful activity, somebody was just saying so to cause trouble, or in some other way it comes to light that our partner did not, in fact, commit the actions they were thought to have engaged in. But none of this is to the point. The point is that facts can be discovered, and facts can be shown to be otherwise. That something that was not known has now been revealed, or something we thought we knew has been shown to be false, are perfectly intelligible and understandable descriptions for competent language users. There is, consequently, a residual phenomenon in Latour and Woolgar's theory: everyday understandings of fact and discovery, everyday understandings that are deployed in accounts of unfaithful partners and accounts of science by scientists and by lay members. When scientists say they have discovered something—a substance, for example—that account is understandable to any competent language user. Everyone who hears on a news broadcast that astronomers have discovered a new star can understand that account, as they understand the account that the police discovered drugs in a warehouse at a particular location. Facts and their discoveries, along with facts that are no longer so, are a commonplace.

The traditional way in which such common-sense understandings, as residual matters, are accommodated in the social sciences is to criticise them as incorrect, ideological, partial, truncated, imprecise, ill-informed and the like. They are thus put to one side. And this is what Latour and Woolgar do when they pit their constructionist theory against everyday uses, in both science and the everyday world, of the words 'fact' and 'discovery'. The "actor's point of view" is being replaced by the sociologist point of view: members say they are doing, and think they are doing 'that', but what they are really doing is 'this'. Scientists' own understanding of what they are doing is 'wrong' *even though this understanding may be, in part, an organising feature of their activities*.

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3 TRF is short for Thyrotropin-Releasing Factor and TRH refers to Thyrotropin-Releasing Hormone. Latour and Woolgar run the two together in acknowledgement of the way in which different communities relate to either "Factor" or "Hormone".

This stance towards “the actor’s point of view” pervades Latour and Woolgar’s study; the following is an apt example.<sup>4</sup>

The bioassay is not merely a means of obtaining some independently given entity; the bioassay constitutes the construction of the substance. Similarly, a substance could not be said to exist without fractionating columns ..., since a fraction only exists by virtue of the process of discrimination. Likewise, the spectrum produced by nuclear magnetic resonance (NMR) spectrometer ... would not exist but for the spectrometer. It is not simply that phenomena *depend on* certain material instrumentation; rather, the phenomena *are thoroughly constituted by* the material setting of the laboratory. The artificial reality, which participants describe in terms of an objective entity, has in fact been constructed by the use of inscription devices. Such a reality ... takes on the appearance of a phenomenon by virtue of its construction through material techniques. (Latour and Woolgar 1979, 64, italics in the original).

Latour and Woolgar starkly dismiss the *participants’* understandings—those working in the laboratory—and replace their ‘reality’ with Latour and Woolgar’s ‘reality’. On this account, participants’ talk about what they are doing, their accounts of their actions, become inconsequential for understanding the organisation of their actions. A consequence is that how the scientists’ point of view regarding ‘facts’ and ‘discovery’ play into the production and organisation of actions and interactions in the laboratory becomes inconsequential for understanding and describing them. Latour and Woolgar pluck the concepts of fact and discovery from out of the contexts of their ordinary uses, redefine them on the basis of a sociological theory and then reapply them to the world they are examining. In their redefined state everyday understandings are then found wanting. Their impoverished descriptive state is, however, only the product (dare we say the *constructed* product) of Latour and Woolgar’s sociology.

There is a curious consequence of this procedure for Latour and Woolgar’s study. It is grounded in ethnographic observations of the actions and interactions of scientists in a laboratory setting. However, these actions become masked from view by the sociological position adopted by Latour and Woolgar. Scientists’ actions display an orientation to the way in which fact and discovery figure in science. For example, the scientists are not engaged in occult practices, they are not attempting to conjure up substances through incantations. Rather they display an orientation to ‘scientific method’ in their activities. This is not to deny that interwoven into those practices are everyday common-sense methods of reasoning that may be required to enact scientific methods. It is also not to deny that the application of scientific method may rely upon methods of common-sense reasoning, whose use is ignored or expunged from scientific accounts. However, the scientists *do* display an orientation to an adherence to scientific method for discovering physical facts in their actions. For example, Latour and Woolgar (1979, 58–59) describe a scientific procedure that was undertaken in the laboratory which involves the administration of a “control” substance, the “measurement” of the effects it resulted in, in order to establish a “baseline”, to be used to compare the effects that another substance might have, and then a further procedure to establish if that substance

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4 The terms that follow in the quoted passage - bioassay, fractionating, spectrometer- are technical terms describing scientific tools and processes.

has “caused” the effect. Simply, whatever else may be said about the scientists’ actions and procedures described by Latour and Woolgar, those actions display an orientation, by the participants, to ‘scientific method’ which they understand they are using to discover a fact about the substance that was introduced.

To stress again, to so describe these actions of method and procedure is not to cleanse them of any common-sense methods they may involve, neither is it to take a position on science. The point is that an orientation by participants to fact and discovery through a method of science is displayed in how they, in part, organise the actions described by Latour and Woolgar. However, by evaporating the very idea that facts are out there awaiting discovery, Latour and Woolgar lose sight of the way in which this very understanding is an organising feature of participants’ actions, and how their account of their actions is, in part, a feature of the organisation of those actions. If the actions and interactions of scientists in laboratory settings is the grounding for Latour and Woolgar’s study then how they are organised can only be dimly seen, or murkily seen, if a feature of that organisation—how participants talk about, account for, and make their actions recognisable to one another—is disregarded because it has been set aside as ‘wrong’.

In stark contrast, Lynch’s examination does display an interest in how participants talk about, account for, and make their actions account-able and recognisable to one another. This is not to say that Lynch sets out with the intention of providing a contrast with a constructionist position; he describes how he was unaware at the time he wrote up his study that Latour and Woolgar had also conducted a study in a laboratory setting. However, Lynch’s study provides an alternative to the traditional sociology of Latour and Woolgar which overlays how members organise their actions as account-able with an account generated from a selected social theory. Lynch does not pit the sociologists’ depiction of ‘reality’ against participants’ descriptions of ‘reality’, he is not engaged in arguing about what science is or is not. If anything, Lynch is indifferent to such arguments. In Lynch’s study the laboratory is an everyday setting in the sense that it is a place of work, and what is being done within it is the settings’ members’ work.

His description of scientists’ accounts of artefacts illustrates a significant point of difference between the two studies. For example, like Latour and Woolgar, Lynch describes how instruments used in the laboratory were elements in the constitution of the entities they found, which might be taken as saying that they were used to ‘construct’ them. And like Latour and Woolgar he describes how the role of instruments in the discovery of artifacts was taken for granted by scientists.

In lab science the manner in which the world of, for example, neural phenomena, is actualised as an instrumental accomplishment remains unspecified in talk of axons, dendrites, glial cells ... i.e., a world of “natural constituents”. Such naturalistic references do not specify how the detailed presence of those entities to description -their concrete visible shapes, textual contrasts, size, extension, i.e. their tangibility is a technical accomplishment in laboratory work. It is as though the complex of theories, techniques and instruments which were involved in disclosing such entities become transitively invisible in the way a clear pane of glass is unnoticed by the gaze that sees through it. (Lynch 1985, 82–83).

Although similarities might be found with Latour and Woolgar regarding the relationship between instruments<sup>5</sup> and entities or substances, there are important differences. While Latour and Woolgar assert that the material arrangement of instruments constructs substances which do not exist independently of them, Lynch is making the point that the character of concrete entities is technically accomplished in the work of the scientists. This emphasis provides for a focus on what that technical work consists of—how scientists play their theories, techniques and instruments into their laboratory activities. Simply, the possibility of preserving scientists' own understanding of their actions as a dimension of their organisation is provided for. Thus Lynch, rather than ignoring scientists' accounts of their actions, turns to these accounts in order to understand how their actions are made accountable to the practices of science. He describes the scientist's point of view:

the instrumental complex is formulated as *methods*... a series of procedures to be purposively executed as a means to an observational and experimental result. In methods talk, the "technique" or "instrument" is referenced as a kind of practical object—an instrument with a functional order or parts placed at arms length and viewed as a material thing with functional implications. (1985, 83, italics in the original).

Lynch is not merely parroting the accounts formulated by participants; he is, rather, interested in how they relate to the actions of the scientists in the lab. Thus, he explains that:

accounts of artifacts will be presented as exhibits of contexts in which practioners' accounts yield detailed specifications of the visibility of laboratory practices in a way that is unique to their discovering programs. That is, artifact accounts will be shown to be a particular way in which laboratory work is made visible ... a way which is embedded in specific topics of ongoing research projects, and which is reflexive to the setting in which the research is accomplished. (1985, 83–84).

Unlike Latour and Woolgar, who displace scientists' accounts of their own actions with a sociological account, Lynch makes visible and investigates the scientists' own understandings of what they are doing, as they are making discoveries about facts, for how it is an organising feature of their actions. Scientists' accounts thus become a rich resource for understanding what science consists of as the embodied actions of scientists: "artifact accounts are appropriated as disclosures of the 'unwitting' work of laboratory science". (Lynch 1985, 84).

In investigating the *work* of scientists—how they account for their actions as science and how they make those actions recognisable as doing science—Lynch is able to accomplish two analytic ends: one which is also firmly in Latour and Woolgar's sights, while the other is precluded by their constructionist position. The first is that science is a socially organised undertaking; the second is to address how science is organised as a distinct undertaking.

First, Latour and Woolgar proceed from a constructionist position that social processes and science and technology are 'a seamless web'.<sup>6</sup> Lynch also treats science as socially organ-

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5 Note that Lynch not only mentions instruments but also "theories" and "techniques" in this constitutive process.

6 See Hughes (1986).



ised. The difference between the two studies is, however, what they make of this obvious fact.<sup>7</sup> For Latour and Woolgar this is presented as their *discovery*. To a certain extent, this might make sense because scientific papers that describe a discovery or a process seldom reference the socially organised features of their work. However, proposing the entwined relationship between the ‘social’ and the ‘scientific’ as a discovery has two consequences. First, it ignores the fact that scientists know that already. For scientists, the social organisation of science is, in innumerable ways, embedded in their work, a given that they orient to, and use, as an everyday, routine matter. For example, grant applications are a common feature of scientists’ work. In writing them, scientists orient to the moral, economic, political, and other social constraints espoused by the grant authorities, and importantly when writing up their interim and end reports for the granting bodies, they show how their work has been done to satisfy these constraints.<sup>8</sup> A second consequence of presenting the social organisation of science as a ‘discovery’ is that it becomes the end point of the investigation. Thus, Latour and Woolgar assert that facts are constructed through a social process of inscription. Apart from naming the inscription devices that is all they have to say—it is their discovery and no more needs saying.

Lynch, however, displays that he is fully aware of the fact that scientists understand that science is a socially organised undertaking, and this allows him to enquire into the question of how scientists play the social into the organisation of their work. That science is socially organised is not the end point of his study but an entry point into the work of science. For example, he describes how work in the laboratory displayed an orientation to the social entity of a *project*.<sup>9</sup> One constraint that working on projects imposes is that of time: projects involve a timespan, the release of further funds is dependent upon the completion of a phase in the project, grant deadlines must be met, and such like. In Lynch’s study, it can be found how laboratory staff organised their work in an orientation to the constraints of a project. Thus, he describes how laboratory staff would juggle their work across projects; how they would maximise their time by working on aspects of another project instead of being idle while waiting for one experiment they had put into operation to complete its cycle; they would multi-task by doing one task in such a way that it also contributed to another task yet to be done, and they would prioritise, interrupting some task they were involved in if the demands of another project were more pressing. Simply, presenting the social organisation of science as a discovery and ignoring how, in their practical work activities, scientists orient to this everyday fact of their laboratory life closes the door on enquiring into how scientists conduct their work of science as socially organised, or at best, in slightly opening it, Latour and Woolgar catch a dim peek of what may be involved. Lynch fully opens the door.

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7 Obvious in the sense that scientists themselves know full well that practical reasoning and ad hoc methods are interwoven into scientific method. And obvious in the sense that science is, when all is said and done, done by humans in social relationships with one another.

8 The author has first-hand knowledge of such matters, not only having, in conjunction with computer scientists, written successful grant applications, but also having been a member of panels making decisions about applications for funding made by scientists, and reviewing their results.

9 Also see Button and Sharrock (1996).



The second aspect of examining the laboratory as a workplace and how scientists organise their work within it is to provide the opportunity to understand how that work is made recognisable *as* science. The question that can then be asked is ‘what is the work of science’. Garfinkel has described how there is a missing interactional ‘what’ of the work in many studies of domains of action. To blandly state that science is socially constructed does not dissolve science; it remains as a recognisable domain of activity that is differentiable from other domains of activity such as, for example, the practice of law. We just *do* talk of science and law as separate undertakings. While both domains may be socially organised, and while there may be social practices that cut across both, for those involved, and within society at large, they are differentiated spheres of activity. While a scientist may give evidence in a trial, nevertheless, they are giving evidence *as* a scientist; they are not acting as advocates. There is a missing interactional ‘what’ of the work of science in Latour and Woolgar’s study; Lynch makes aspects of that ‘what’ visible.

An example of the consequences of this difference is found in the way in which the two studies consider the fact that much of science is conducted in the conversations that go on between laboratory members. Both studies present this fact as an example of how science is socially organised, and both studies present transcripts of conversation<sup>10</sup> which they analyse to ground their descriptions. Lynch’s sequential examination of what he calls “shop talk” reveals a sequential device through which, in their talk, the participants *modified* their accounts of objects in pursuit of agreement. Lynch shows that disagreement and argument in shop talk is part and parcel of the work of science, and how that work is partly organised and achieved *as* scientific work in the modification of accounts in the pursuit of agreement. He describes how participants modified their accounts in the face of disagreement in courses of conversation characterised by sequential structures of assertion, counter assertion and reassertion. Sacks (1987) describes how generally the *pursuit of agreement* in conversation is organised within a preference system. However, Lynch makes visible how the pursuit of agreement in shop talk, as organised through assertion, counter assertion, and reassertion, is used for scientific purposes, how this sequential structure is used to reach agreement within, and for, the purposes of the laboratory setting.

The point to be taken away from this description is not that, in their conversations, participants construct the object. Rather “re-descriptions [in the face of challenges] of objects, in pursuit of agreement, do not necessarily depart from the ‘objective’ features of objects.” (Lynch 1985, 202) Indeed, Lynch describes how in courses of assertion and reassertion scientists can make discoveries about objects; in their shop talk, assertions and reassertions, can be used by the scientist to cast ‘new light’ on objects; their conversation provides “resources for disclosing *an* objectivity in its elaboration of reference” (Lynch 1985, 203). Lynch is at pains to describe how it is that in their conversations the laboratory participants proposed “objectivities”, argued about them and verified them. The modification of accounts of objects in conversation does not so much erode the objectivity of description; rather the objectivi-

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10 Latour and Woolgar tell us that they were not allowed to make an actual transcript of the conversations they present because of laboratory protocol. Instead, they constructed their transcripts from the notes they made of the conversations.

ty of an account is achieved and verified in pursuit of agreement. Lynch is thus showing us how courses of conversation, in which accounts of objects are argued over and modified, are as much the work of science as is experimentation. Rather than taking a position on what scientists are doing in their conversations as creating facts, which, as we will see below, is the position adopted by Latour and Woolgar, Lynch makes the actions they accomplish in their conversations, pursuing agreement, inspectable for how participants organise them and how through that organisation they achieve an oriented-to objective character of an object and their account of it.

Latour and Woolgar are also interested in what they call microprocesses such as gestures or talk, but gloss them as being in the service of “fact creation”. They state that a received version of science suggests that there is a divide between scientific reasoning and the reasoning that takes place in non-scientific settings. However, they found, through an inspection of the scientists’ conversations they had in hand, that it was difficult to identify what they describe as “purely” descriptive, technical, or theoretical discussions. They write: “the mysterious thought process employed by scientists in their setting is not strikingly different from those techniques employed to muddle through daily encounters.” (Latour and Woolgar 1979, 166)

Despite the pejorative use of “muddle” to characterise everyday practical reasoning, there may be a seeming alignment with Lynch’s description of how scientists display an everyday conversational practice of reaching agreement. Latour and Woolgar describe how, in their conversations, scientists can create or destroy facts which might chime with Lynch’s description of the way in which objects are modified over the course of a conversation. However, the similarities are a surface matter.

Latour and Woolgar argue that the objective status of scientific reasoning can be questioned because organising methods used in non-scientific settings can be found in scientists’ conversations. However, if we examine their analysis of the conversations they present we find bland characterisations of whole conversations, rather than, as we find in Lynch’s analysis, the descriptions of actions done in those conversations. For example, conversations are globally described as displaying that a variety of interests are in play, and somehow, these contending interests result in fact creation and fact dismantling. However, Latour and Woolgar do not explicate the ‘somehow’; they do not show us how, in the actual organisation of actions done in and as conversation, facts are indeed constructed. Instead, they provide vernacular formulations of what the participants are saying, and merely state, assert, that in saying these things participants are creating and dismantling facts. Unlike Lynch, who does show us how, in their conversational actions, scientists are doing the work of science, Latour and Woolgar do not describe *how*, in the organisation of what the scientists say, they are creating and dismantling facts. Simply, Latour and Woolgar do not draw out from these conversations just how the scientists are organising their conversation as fact creation and fact dismantling; unless that is done that description is merely an assertion.

Lynch, on the other hand, bores down into the details of the conversational exchanges he examines. In doing so he encounters how a conversational practice of reaching agreement is used by scientists as the work of science. That is, from the scientists’ point of view they are doing science *in* the modification of objects through the course of their conversation. In boring down into the details of their conversation, Lynch is not merely asserting this; rather,

he makes visible how the conversational practice of reaching agreement is being organised through distinctive structures that display an orientation to the setting in which they are used. The general conversational practice of pursuing agreement is, thus, tailored for the laboratory setting as sequences of assertion, counter assertion, and reassertion. It is through these actions that the participants figure out what an object is. To merely assert that participants are creating and dismantling facts is to coral the conversations through the imposition of a theory that stands outside of the way in which participants organise their actions and interactions. Lynch, however, makes visible how participants achieve the oriented to status of objects as objective through their endogenous practices. Latour and Woolgar, in contrast, use the fact that everyday practical reasoning figures in science as a way of questioning its *objective* description of objects. Again, Lynch is not taking a position on science, he is not contesting scientists' understanding of science. Rather, he is attempting to make visible how scientists' actions are account-able, orientable to, as science. He makes visible that, in part, the methods through which shop talk is organised is the work of organising a description as characterizable and orientable to as objective. In other words, how scientists make the object objectively account-able.

## CONCLUSION

Why turn to two books which are four and five decades old in a volume that celebrates Lynch's life-long work, especially since he subsequently developed what might be considered more important contributions to both SSST and to ethnomethodology? There are two reasons. First, both books contribute to a foundational move in SSST: the ethnographic study of the actions of scientists. Along with Knorr-Cetina (1981), they were the first studies to enter laboratory settings in which science was being done in practice and enquire into how science is done in the raw, so to speak. Both studies can be viewed as supporting a way in which SSST developed a confrontational position to the received version of science: science is not the objective, technical, methodical, logical undertaking it is portrayed to be in scientific documents and popular renditions. By way of contrast, both studies make visible the socially organised character of science which is obscured in scientific and popular accounts of science. However, if the two studies are compared we find two very different understandings of what this obvious fact means for studying 'science in action'. Latour and Woolgar's study fully supports a confrontational stance to the received version of science and in this respect has been viewed by many as a radical alternative to received versions of science. Lynch's study is, however, a radical alternative in another sense. It is a radical alternative to the traditional sociology embedded in Latour and Woolgar's study and in social constructionist theory in general.

The divergences between Latour and Woolgar, and Lynch that I have attempted to describe regarding how to treat scientists' accounts of their own actions, one to critique them and replace them with an alternative account, the other to make visible their role in organising the practical actions found in the laboratory, speak to a wider divergence in the social sciences in general. Garfinkel and Sacks (1970) refer to ethnomethodology as providing an alternate

to the constructive<sup>11</sup> analysis of social action by directing attention to the formal structures members instantiate in situ as they order and achieve their actions and interactions. Thus, actions are done so as to be account-able, they are done so as to be recognisable as those actions. It is this ethnomethodological understanding that underpins Lynch's study, and that he returns to in his on-going corpus of clarifying work. An implicit overarching concern within his study is to make visible the structures through which action is endogenously organised so as to be recognisable as doing science. The question addressed is what the work of science is? What is it that scientists do, and how do they organise what they do so that it is account-able as science.?

Latour and Woolgar adopt a traditional standpoint to their subject matter. They re-describe the actions as ordinarily done and ordinarily understood (even if they are extra-ordinary, or 'special' doings) as meaning something else through the imposition of a sociological theory. Though Lynch may be viewed in SSST as 'one of us', even an exemplary 'one of us', and even though his work may be picked over to shore up a constructionist position, I find embedded in his studies, descriptions of the work of science that can be used to cast a light on fundamental flaws in a constructionist position. *Art and Artifact* (1985) examples how SSST could have trodden an alternative path that would have indeed been radical, but a radical alternative to traditional sociology.

The second reason I have turned to this very early study by Lynch is that it is one of the few studies that implements Garfinkel's programmatic statements regarding the ethnomethodological study of science.<sup>12</sup> This reason is an ancillary to the first. I have gestured at, but not attempted to systematically draw out, how *Art and Artifact* illustrates how a 'domain' of specialised work can be examined in order to view its constitutive methods. However, reading the book again after the passage of some considerable time since I first read it I am struck by its ground-breaking character for ethnomethodology. It has been previously argued (Button, Lynch, and Sharrock, 2022) that the program of ethnomethodological studies of work initiated by Garfinkel (1986) has stalled. *Art and Artifact* rewards re-reading as a resource to draw from if ethnomethodological studies of work is to be resuscitated.

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11 Not to be confused with constructionist. Garfinkel and Sacks were referring to the methodological and theoretical tools deployed in sociology and the social science at large through which the essential indexicality of natural language expressions is displaced by objective expressions—a task that is itself obstructed by the essential indexicality of natural language.

12 See Garfinkel (2022).

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