Renormalizing science? Postanalytic inquiry for post-normal times^{*1} Philippe Sormani

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Abstract

In 1993, *Scientific Practice and Ordinary Action*, Michael Lynch's landmark volume on ethnomethodology and social studies of science, was published at Cambridge University Press (Lynch 1993). In the same year, a journal named *Futures* published 'Science for the Post-Normal Age' (Funtowicz and Ravetz 1993), a seminal essay in the field of science policy expertise by now. Thirty years on, Lynch has masterfully edited Garfinkel's *Studies of Work in the Sciences* (Garfinkel 2022), while the qualifier 'post-normal' has become a recurring trope for tension-riddled societies, if not sociology as a tricky project itself (e.g., Thorpe 2022). Taking its cue from these publications, genealogies, and coincidences, this paper pauses on Lynch's legacy in ethnomethodological studies of scientific work, while reflexively explicating its distinctive contribution to science and technology studies (STS). As a reflexive explication, the paper engages with 'Lynch on science' in the light of prior readings and reviews, as well as in view of new articulations of ethnomethodology, STS, and sociology (e.g., Marres 2023). In 1993, Lynch introduced 'postanalytic inquiry' as scholarly inspired praxeology, while offering a subtly deflationist critique of 'normalizing science' (i.e., scientism at large). So what now? Instead of indulging in renormalizing science and social science, this paper articulates three readings of postanalytic inquiry in ethnomethodology—against, with, and beyond—that is, perhaps, Lynch's 'radical ethnomethodology' (2016), if not anyone's, for post-normal times.

The asterisk in the title marks the contingent and provisional character of my *Festschrift* contribution. The paper is contingent on reading Michael Lynch's extensive writing, notably on science at work and ethnomethodology in practice (e.g., Lynch 1993, 2001, 2022a, b), before and after inviting and first meeting him as a scholar at an international workshop on 'scientific work *as* ordinary action,' organized at the University of Fribourg, Switzerland (GRIPS 2007). The contribution is also provisional, insofar as—in addition to my sustained reading of Lynch's work on science (Sormani 2014, 2016, 2022a)—the contribution sketches out a topical recontextualization of his *'postanalytic approach*' (Lynch 1993, ix, italics in original). For whatever it's worth, I owe this recontextualization idea to recent policy analysis, education research, and epistemology teaching appointments, thanks to Ângela Guimarães Pereira, Farinaz Fassa Recrosio, and Esther González Martínez, respectively. Wes Sharrock deserves credit for having nudged me into exploring laboratory work in the first place. Finally, I thank the editors of this *Festschrift* for inviting and commenting upon my contribution.

INTRODUCTION

'[...] a niche for ethnomethodology or something like it reappears wherever and whenever formal analysis is done' (Lynch 2015, 613)

'Professor Lynch!'—I could not help but pronounce this formal greeting, after I had spotted (who appeared to be) Mike Lynch at a market stand in a well-known Swiss town. In fishing gear, and before setting out on a family trip, Mike was in town to discuss the Garfinkel volume he recently had edited and introduced as *Studies of Work in the Sciences* (Garfinkel 2022). Pending that discussion and lacking an invitation, I figured, I might as well attend to the day's business at hand: a reporting meeting of the Swiss National Science Foundation, on the 'digital transformation' project on which I recently had been (self-)hired, and which required 'some ethnomethodology.' To properly lay out the background of this paper, a second anecdote is in order, however. Take September 13th, 2023. It also happened to be a day of coincidences: *Social Studies of Science*, a leading journal in science and technology studies (STS), published our introduction to a special issue on 'artificial intelligence' (Jaton and Sormani 2023), climate scientists yet again warned of 'Earth [being] 'well outside safe operating space for humanity'' (Carrington 2023), and a new family member (of mine) was born into this tricky world on that very day².

As the two anecdotes suggest, there are at least two contrasting ways in which the 1960s slogan 'the personal is political' still applies to today's working academic, in terms of 'research policy' or 'world politics,' for short—both rooted in everyday circumstances somehow. Now and then, Harold Garfinkel, with whom Lynch had studied for his PhD in the late 1970s and collaborated for the investigation of the (natural) sciences (e.g., Garfinkel et al. 1981, 1988; Lynch 1985; Lynch et al. 1983), has been identified as a 'sociologist for sociologists,' concerned with research matters and disciplinary politics, more so than socio-political issues, let alone world-political ones (e.g., Losing Earth, Rich 2019). In the light of Garfinkel's biography and academic career, not to mention 'post-humanist' developments in STS, the contrast may be misleading, and the connections between the two, three, or more kinds of (cosmo-)politics underappreciated (e.g., Blaser 2016; Rawls and Turowetz 2021). However, Garfinkel's Studies in Ethnomethodology (1967) surely invited its sociological reception in methodological and epistemological terms, not in primarily political, socially critical, or ecologically reframed ones. For one thing, Garfinkel had invented 'ethnomethodology' as the sociological study of 'everyday methods of practical action and practical reasoning used by members of society to make sense and produce order in their daily lives' (Burns 2015, 1). For another,

Garfinkel and his students dissolved the supposed chasm between common sense and scientific/professional reasoning, unpacked the taken-for-granted existence of society, developed detailed descriptions of

² From 2002 to 2012, Lynch had been serving as Editor of *Social Studies of Science*. How did STS develop before, during and after this period? For a thoughtful answer to this question, see Lynch (2014).

commonplace and specialized organizations of practice, and specified how society members assemble and sustain the sense that they live, act, and mean in a shared orderly world (Burns 2015, 1).

This multifaceted research agenda, if sociological and empirical, remained and remains rooted in (social) phenomenology, 'treating method and form as phenomena' (Langsdorf 1995). To probe 'ethnomethodology's niche,' as alluded to above, is then (and to stick with Langsdorf's wording) to 'report on some [formal] content as constituted in our social actions, in accord with mundane, 'conservative' practices-and without intrusion from the sociologist's method or interests' (1995, 184). Scientific Practice and Ordinary Action, Lynch's landmark volume from 1993, takes its cue from this phenomenological interest, while adding a sociological twist. The phenomenological interest is articulated as ethnomethodology's study of the 'genealogical relationship between social practices and accounts of those practices' (Lynch 1993, 1; italics in original). In turn, the sociological twist—a twist in 'epistemic sociology' (more of which below)—is indicated as ethnomethodology's purpose, namely: to engage in 'respecification' (Garfinkel 1991). In particular, Lynch (1993) advocated a detailed reexamination of the topics conventionally associated with science-topics such as 'observation,' 'representation,' 'measurement,' 'proof,' and 'discovery' (xi-vii)—in the light of how they happen (or not) to be 'locally and practically relevant' (xi, note 1)—in the light of laboratory work, for example³.

Taken together, that phenomenological interest and this sociological purpose renew the (potentially) heuristic tension, the tension between explicating social practices with respect to their vernacular accounts or technical terms, on the one hand, and engaging with the academic literature(s) about the domain considered, yet prior or subsequently to the encountered working situation, on the other. Consequently, one might ask with Howard Schwartz:

how does one see, know and describe everyday reality in new, interesting or scientific ways while keeping that reality phenomenologically intact as the selfsame world one has started out with? (Schwartz 2002, 117).

Against this background, I have come to read, rework, and review Lynch's extensive work on 'science in practice' over the last twenty years, partly at least. At first, I argued *against* Lynch's 'postanalytic approach' and its seemingly intellectualist trappings (Sormani 2014, 2016). More recently, I have sided *with* Lynch's programmatic approach, if indirectly, in reviewing *Harold Garfinkel: Studies of Work in the Sciences* (Sormani 2022a). This Garfinkel volume, edited and introduced by Lynch (2022b), articulates a post-Kuhnian praxeology of proto-sociological interest, challenging both disengaged ethnomethodology (hence the prefix 'proto-,' hinting at the key requirement of practical immersion) and scholarly disinterest (hence the adjective 'sociological,' alluding to disciplinary pertinence). In this paper, I will revisit and elaborate on these two readings, before sketching out a third one *beyond* the program's usual contextualization, including mine—that is, perhaps, Lynch's 'radical ethnomethodology'

³ The emphasis on 'work,' rather than its habitual place or conventional attributes, dovetails with Garfinkel's 'studies of work' program (see Lynch 2022a).

(2016), if not anyone's, for 'post-normal times' (Thorpe 2022). In conclusion, I will touch on recent 'renormalizing science' exercises in sociology, STS, and conversation analysis (CA), while leveraging Lynch's four-decade deflationist critique of 'scientism' for unprejudiced (re-) articulations of said fields⁴.

1. AGAINST: TACIT IMPORT OF CONSTRUCTION ANALOGY

saic difference between whatever I was typing on my computer, the computer I had been granted access to as 'Sigmund' on the laboratory shop floor, and the two-store experimental facility that had just exploded in my back, with its measurement unit shooting out of it like a rocket (Sormani 2014, 94-5). In the course of observing physicists at work, I had collected various kinds and numerous instances of such 'difference reminders.' For a start, experimental physicists cared more about STS' than STS"—that is, 'scanning tunnelling spectroscopy' (Fischer et al. 2007), rather than 'science and technology studies.' In the vein of this and similar observations, Respecifying Lab Ethnography (Sormani 2014) took issue with Lynch's (1993) scholarly arguments in and around STS", arguments that I perceived as largely oblivious of the (up-)skilled engagement with technical practices of 'normal science' (e.g., STS') as required of lab members, if not the reflexive ethnographer. Wouldn't any next ethnomethodological study of scientific work—I wondered with Garfinkel (2002)—require precisely such tricky engagement? Before returning to the explosion episode, perhaps the best 'unmotivated observable' (Garfinkel 2022) that I could glean from my field notes, a brief outline of Lynch's (then) '*postanalytic* approach to the study of scientific practices' (1993, ix) is in order⁵.

As its subtitle suggests, the book offered an exegetical intervention in the burgeoning field of 'social studies of science,' while both drawing upon and developing 'ethnomethodology' for the purpose (Lynch 1993). Thirty years on, the volume remains a must-read for anyone interested in social studies of science and technology, their joint historical development and major sociological controversies, as well as ethnographic approaches and ethnomethodologi-

⁴ Readers may have noticed my slippage from 'approach' to 'programmatic approach' and then 'program'—all in singular. The slippage marks the continuing influence of Lynch's legacy in STS, ethnomethodology and related domains, including on my own work—but see also Alač (2011); Bjelić (2003); Bjelić and Lynch (1992); Bogen (1999); Button et al. (2022); Coopmans and Button (2014); Greiffenhagen and Sharrock (2011); Hoeppe (2014); Hutchinson et al. (2022); Ivarsson and Lindwall (2023); Mair et al. (2020); Mondada (2014); Sherman Heckler (2011); Vertesi (2015); Watson (2023); Ziewitz (2017). On the viability of 'proto-sociology' and its main variations, see Quéré (1994).

⁵ Recently, John Heritage has opposed CA and 'post-analytic ethnomethodology' (Heritage 2018), an opposition that Lynch (2018) takes objection to. On my reading, his 1993 book was *not* directed against CA per se, but 'applied CA' more specifically. Indeed, the book refused applying CA's methodology *a priori* (i.e., real-time recording, detailed transcription, interaction analysis) and offered it a contingent place among other 'normal (social) science' methodologies instead (e.g., participant observation, historical analysis, documentary study, 304–5)—that is, a place contingent upon the phenomenon of '(scientific) practice' under scrutiny, as well as upon Lynch's (1993) research program, I should add.

cal alternatives in (and to) the field. Lynch's insightful exegesis articulates three intellectual developments: the programmatic definition of 'scientific contents' per se as a legitimate topic of sociological inquiry, the conceptual relevance of Ludwig Wittgenstein's remarks for dissolving causal pictures of 'rule following,' and the improbable venture of ethnomethodological inquiry as an alternative approach to scientific, technological, or otherwise epistemic work 'in the wild.' Against this background, Lynch's programmatic intervention offers a plea for a 'postanalytic approach' to such work, taking the form of an 'epistemic sociology' (Coulter 1989). That is, and instead of legislating on epistemology's traditional themes (e.g., 'truth,' 'objectivity,' 'method'), Lynch's approach, and particular 'take' on Garfinkel (1993, 5, note 9), suggests turning those themes into empirical topics, 'iterable epistopics' (275-87), investigable for how they are dealt with in worksite practice (e.g., lab work). As its qualifier 'postanalytic' suggests, the approach invited a 'retrospective relation to already accomplished analyses' (312), including prior historical, philosophical, sociological, and/or interactional analyses (e.g., CA). Throughout, a deflationist motive characterizes the approach, as it takes its cue from Wittgenstein's remarks and reminders to explicate the immanent logic of social practices (e.g., the internal relation between rule and conduct), rather than to promote explanatory schemes oblivious of situated practices and their mundane ordering yet again, be it in the name of a 'strong program' (Bloor 1976), a 'new sociology' (Latour 1992), or a 'superscience' (Lynch 1993, 313). A practice-informed critique of 'scientism,' as a recurring yet dubious philosophy of (social) science, would result from this approach, while taking as many expressions as to be expected from close engagement with this treacherous hydra (e.g., Lynch 1991a, 1991b, 1999, 2009).⁶

What could possibly be wrong with Lynch's deft exposition? Let us return to the explosion episode to illustrate a first—to put it bluntly—intellectualizing risk. The episode offered a stark reminder of the prosaic difference between shop floor priorities and potential scholarly interests (recently, I've added 'dodgy software,' as the practical reality contrasting with 'AI' discourse, to my growing collection). A closer look at the conceptual articulation *internal* to those practical priorities, in contrast to theoretical sociological interest(s), further exemplifies the phenomenological issue, a lingering 'Schutzian problem' (Sormani 2014, 241). The explosion episode made evident the critical yet tightly *circumscribed* relevance of '(social) construction' in experimental physics as ordinarily pursued. The experimentalist had pressed forward with a 'quick measurement,' despite an unresolved 'calibration problem,' thus blowing up his facility and throwing him back to its 'partial (re-)construction' (91–5). Conversely, constructivist studies of science (e.g., Latour and Woolgar 1986[1979]), instead of describing facility construction as a *particular* phase of techno-scientific inquiry, *assumed* its overall conduct and eventual facts to be 'constructed' (Lynch 1993, 102; see also Francis and Hester 2004).

⁶ If wrong-headed, 'scientism' is a multi-headed beast, ranging from 'the slavish imitation of the [mistaken] method and language of [natural] science' (Hayek, Popper) to 'science's belief in itself' and the 'meaning of knowledge [exclusively] defined by what the sciences [allegedly] do' (Habermas), all quoted in Frisby (1976, xiii-xiv). On and against 'scientism' in philosophy of science, natural and cultural, see Sorell (1991). On and against the other part of the same coin, literary forms of 'anti-scientism,' see Lynch (2000a).

Intriguingly, this assumption marks a category mistake, dodgy analogy, and research program in one!

Lynch's 'postanalytic approach' in turn suggested *re*-examining 'construction,' or any other (quasi-)epistemological topic, as dealt with, discussed, or disposed of in laboratory practice. Why? In Lynch's outline '[t]oward an investigation of primitive epistopics' (1993, 299-308), the academic literature of social studies of science (i.e., STS'') provided an important motive for ethnomethodological inquiry (300). So had the theme of 'social construction' for Lynch's study of laboratory work, given its academic traction at the time, at least for introductory purposes (Lynch 1985). In turn, my reflexive ethnography of experimental physics at work (Sormani 2014) set out to recover manifestly self-organized research practices, prior to social scientific indexation ex cathedra, and their immanent conceptual articulation *in situ* (i.e., STS'). In this vein, I came to wonder how Lynch's approach could avoid the risk of tacitly importing the construction analogy to practice description—if not any other topic of scholarly interest, yet of *practical irrelevance* to scientists at work. That, then, might have been wrong with Lynch's exposition⁷.

2. WITH: PRODUCTION PHENOMENA VS INVIDIOUS COMPARISON

Thirty years on, *Harold Garfinkel: Studies of Work* in the Sciences (Garfinkel 2022), Lynch's masterful edition of Garfinkel's writings and lectures on 'scientific work' from the 1980s, invites an instructive reappraisal of *Scientific Practice and Ordinary Action* (Lynch 1993). At the time, the latter book was addressed to STS(") scholars, prospective ethnomethodologists, and advanced students in the humanities, yet to be provided with a 'different angle' on their home disciplines (308). Perhaps due to this purpose and academic public, the book arguably overemphasized the heuristic interest of scholarly exposition for grasping 'scientific practice' in its ordinary pursuit. 'Institutional amnesia' was identified, at least among French sociologists, as a second intellectualizing tendency of the book. The book, it seemed, advocated description of scientific practice *as* 'ordinary action,' *as if* scientists were primarily or exclusively orienting to epistemic puzzles of disciplinary interest, regardless of institutional accountabilities, organizational frames, and power struggles (e.g., Quéré 2002)⁸.

In what sense, then, does *Studies of Work in the Sciences*, the recently published Garfinkel volume, edited and introduced by Lynch (2022b), offer us a new 'take' on his earlier 'postanalytic approach' (1993)? In two respects at least, I will argue, both of which bring out the

⁷ This, at any rate, was my critical question and candid argument in and for *Respecifying Lab Ethnography* (Sormani 2014). In conclusion, I challenged both the premises and application of Lynch's (1993) 'postanalytic approach' (e.g., Sormani 2014, 235, 238–44). On the risk of 'scholastic fallacy' in sociological reasoning and language philosophy, see also Quéré (2021, 2023) and Krämer (2010).

⁸ French? Well, Durkheimian (or Bourdieusian) might be a better qualification. Following his review essay, Louis Quéré co-authored a Durkheimian critique of Garfinkel's 'studies of work' program, if indirectly (Quéré and Terzi 2011), and similarly to Greiffenhagen and Sharrock's (2019) Wittgensteinian critique. For a complementary Schutzian critique, see Dennis (2004), and Lynch's (2004) rejoinder.

deeply Garfinkelian inspiration of Lynch's continuing work on science, inviting ethnomethodologists to side 'with,' rather than argue 'against,' his postanalytic stance.

2.1 Postanalytic inquiry as ethnomethodological corrective

In one respect, Lynch's scholarly exposition from 1993 (re-)appears as an *ethnomethodological* corrective to 'respecifying the natural sciences as discovering sciences of practical action,' primarily or exclusively so, the program first drafted in a co-authored essay (Garfinkel et al. 1988) and now published as the first part of *Studies of Work in the Sciences* (Garfinkel 2022). Hindsight is of particular interest here. In the 1988 essay, Garfinkel and his nominal co-authors (Lynch 2022b, 6) sketched out the (then relatively recent) past, present, and prospect of ethnomethodological inquiry into the natural sciences and mathematics, in and as their 'discipline-specific work-site details' (1). In the process, the author(s) compared the two major studies laying claim to this project at the time: Lynch's Art and Artifact in Laboratory Science (1985), Livingston's The Ethnomethodological Foundations of Mathematics (1986)⁹. Both studies are identified as 'exemplary ethnomethodological' ones (27). Only Livingston's study, however, is praised for the 'unique adequacy' of its (ethno-)mathematical findings, praxeological and pedagogical. By contrast, Lynch's 'analytic ethnography'—due to its scholarly interest and rhetorical qualities, rather than its technical acumen in the target domain (i.e., axon sprouting as studied in a neurobiology lab)—would stand in need of ethnomethodological respecification—that is, as an

[...] analytic ethnography, *not* as flawed or ersatz ethnomethodology, *but as a condition for finding and explicating dissatisfactions that in turn provide the respecifying studies* [listed in the essay's appendix] *with an agenda* (Garfinkel 2022, 27; italics in original).

However, a complementary essay on 'respecification' was soon published and signed by Garfinkel (1991). This essay—in fact, a retitled version of a prior intervention at the ASA (Garfinkel 1988)—rearticulated ethnomethodology's immersion in technical practice *with* its sociological purpose: the situated specification of technical practice in, if not *as*, its 'social order'—that is, an empirical endeavor of critical interest to social and human sciences, too (Button 1991a, b). Hence by 1991, and by virtue of Garfinkel's own reframing, the primary focus on, if not exclusive pursuit of, technical practice *qua* technical practice appears as reductive praxeology (see also Garfinkel's 2002 emphasis on 'hybrid studies' in physics *and* sociology). In other words, 'unique adequacy' and 'scholarly exposition' came to stand in a heuristic tension, a tension constitutive of ethnomethodological inquiry that Lynch's (1993)

⁹ Eric Livingston had also been studying with Garfinkel for his PhD, before collaborating with both, Garfinkel and Lynch (resulting, for example, in the famous 'Pulsar paper,' Garfinkel et al. 1981).

book accentuated, and the reedited Garfinkel (2022) essay reminds us of, by way of marked contrast three decades later¹⁰.

Manifestly, ethnomethodological *res*pecification requires at least two *related* yet *different* terms in tension—no 'invidious comparison' against a unilateral benchmark 'from without' (be it one exemplary study, trick, or ideal). As the subtitle allusion to 'production phenomena' suggests, this still leaves open the problem of relevance though (i.e., as to why and how this or that inquiry is engaged in), problem to which I shall return when it comes to 'recontextualizing' postanalytic inquiry in the third part of this paper¹¹.

2.2 Post-Kuhnian praxeology of proto-sociological interest

In another respect, the recently published *Studies of Work in the Sciences* (Garfinkel 2022) prove of particular interest, and that is, in relation to lingering 'institutional amnesia' and its Durkheimian critique alluded to above.

In this respect, it bears worth remembering that *The Structure of Scientific Revolutions* (Kuhn 1970a) first homed in on 'normal science' in its disciplinary pursuit, if not dogmatic practice, largely equating institutional accountabilities with epistemic priorities. As Randall Collins concisely put it, 'Kuhn is not only a Mertonian, but he is a Mertonian *sans* sociology' (Collins 1983, in Zammito 2004, 321, note 48). In his lectures from the early 1980s, Garfinkel took inspiration from Kuhn for probing 'discovering work' in the natural sciences (see Garfinkel 2022, Part II). In particular, Kuhn's notion of 'paradigm as shared example' (2022, 127) and '[customized] law-sketch' (2022, 128, 176), as explicated in his 1969 postscript to *Structure* (Kuhn 1970b), provided an important source of inspiration for Garfinkel's lectures, as well as his later notion of 'Lebenswelt pairs' in the sciences (Garfinkel 2007; Garfinkel and Liberman 2007). Lynch's (1993) approach to scientific practice as 'ordinary action,' some-

¹⁰ And, we should highlight, thanks to Lynch's remarkable editorial work! For recent reviews, see Hoeppe (2023); Smith (2023); Sormani (2022a). This is not to claim that Lynch's (1993) book itself was exclusively concerned with Garfinkel's EM and its variations, as both my brief appraisal in the prior section and more extensive reviews suggest (e.g., Dennis 2004; Quéré 2002; Sormani 2014).

II In the early 1990s, Garfinkel and Lawrence Wieder introduced the notion of 'asymmetric alternates' to characterize sociological inquiry in terms of the mentioned tension (Garfinkel and Wieder 1992). They contrasted formal approaches, articulating operational definitions, models, and concepts with lived practices, enabling sociological inquiry via those very definitions, models, and concepts. Yet the endogenous organization of lived practices, while providing the tacit basis for sociological inquiry, couldn't be recovered from its formal features. This asymmetry characterizes many other practices of inquiry, lay or professional, including 'wayfinding' (Macbeth 2024) with the help of maps, yet not reducible to maps, digital or analog. Again, these terms in tension are related, while differing from each other in kind—thus making 'invidious comparison' pointless (e.g., map precision vs skilled navigation). In the vein of Husserl's and Schutz's phenomenologies, the 'difference assumption' between (social) scientific theoretical abstraction and mundane practical realities runs through Garfinkel's oeuvre (Sormani and vom Lehn 2023, xiv–xv). As a heuristic tension, it has inspired different directions of EM analysis (Sormani 2019), despite or precisely because of arguable incoherence(s) (Greiffenhagen and Sharrock 2019).

how regardless of its institutional accountabilities other than epistemic and/or embodied (if we stick to its 'French' reception, *aka* Durkheimian reading), took its cue from this recently recovered lineage, Kuhnian and Garfinkelian, in addition to its deflationist motive of Wittgensteinian inspiration¹².

That being said, the term 'institutional amnesia' does not need to stand proxy for arguably reductive sociology, epistemic or other, but can also be treated—that is, 'misread' (Garfinkel 2002)—as a *counter-factual praxeological maxim*: set out on your ethnomethodological study of scientific work, regardless of its conventional institution or your professional credentials in the discipline considered—in short, 'go for the most difficult first!' (see Sudnow 1978, 36). In so doing, any reflexive ethnographer or prospective ethnomethodologist is likely to be confronted with a large range of instructive contingencies, including (some of) the critical contingencies that practicing scientists are confronted with at work, among which the 'institutional accountabilities' they face, in and due to the scientific practice(s) they engage in (in this vein, see also Lynch et al. 1983; Sharrock and Anderson 1982). In *Respecifying Lab Ethnography*, I described this experience as the 'ethnographer's paradox':

The paradox by and large consisted in the ethnographer becoming confronted through his [or her] own 'subjective', substantial, and practical engagement in the domain with its most objective and formal, if not theoretical requirements [...] (Sormani 2014, 247).

By way of interviewing scientists, Garfinkel in the 1980s set out with a stance similar to the highlighted maxim (see also Bjelić 2003; Livingston 2008), as he asked scientists to elaborate on the 'contingencies of the day's work' and their 'actual experiences in [their] lab[s]' (2022, 43), while discovering his own technical incompetency *and* its heuristic interest in the process (if only for pursuing interview inquiry, if not participant observation). 'Losing the phenomenon,' 'Wasting time,' 'Making an experiment work'—these were only the first three contingencies characteristic of lab work that Garfinkel listed and reflected upon, as 'locally lived constraints on the instructable reproduction of the [intended] phenomenon' (23) of disciplinary inquiry. Indeed, both Garfinkel's initial list, which includes sixteen further work-site contingencies, and his insightful elaborations appear wickedly open-ended: a 'descriptive litany' (24). Ironically, the list ends with a principled critique, a critique voiced by Phil Agre, the computer scientist in the room: 'You missed the point! What are the contingencies for?' (ibid.).¹³

¹² The latter motive finds its clearest expression in Lynch's (1992a, b) debate with David Bloor (1992) regarding Wittgenstein's remarks on 'rule following' and their sociological implications (Lynch 1993, chap. 5). Alternatively, Wieder's (1974) reflexive ethnography probed Durkheim's 'social facticity' as an everyday phenomenon, including potentially lab work and 'discovering science' *in situ* (Sormani 2014, 2022b).

¹³ A graduate student in computer science and engineering at MIT in the late 1980s, Phil Agre attended one of Garfinkel's talks at the time (see Garfinkel 2022, 24, editor's note 2). Agre went on to articulate an internal critique of 'artificial intelligence,' critique for which he remains perhaps best known today (e.g., Agre 1997). For an instructive intellectual biography, see Masís (2014).

2.3 A case in point: contingencies in programming and pedagogy

According to Garfinkel (2022), Agre himself came up with a principled answer to his critical question, an answer formulated from the vantage point of his programming practice, namely: the listed work-site contingencies provide 'constraints on the truth of the matter' (24). Truly, and outside of Agre's programming practice, the 'truth of the matter' remains an allusive idea(l), even or especially when related to his ongoing project: a 'computational representation of practical action' (55). Dwelling on this allusiveness, Garfinkel's essay advocates a methodological shift (24), while acknowledging a 'consequential dispute' (55). Let us consider each in turn, shift and acknowledgment, before turning to a recent example of programming practice and its classroom contingencies. The example is taken from an educational setting, a potentially 'perspicuous' one, which also will allow us to reexamine 'postanalytic inquiry,' if not recast its epistemic outlook¹⁴.

That being said, the *methodological shift* advocated by Garfinkel in response to Agre's idea(l) remained principled and allusive, too. For one thing, he posited that prospective ethnomethodologists of '(scientific) work' would home in on the 'praxeological validity of instructed actions' (24; italics added). Researchers were invited to study the practical realization of actions, their instructed character, and its manifest success, how and why an 'action' recognizably came to fit its 'instruction' (e.g., a recipe finding its culinary expression in a savory dish). Task-specific 'relevance,' work-site 'teachability,' and the 'constraints' tied to a task's teaching in situ were moved into focus—in opposition to Agre's abstract 'truth constraints,' at work 'nowhere ever available' (24) as such. Yet, in Garfinkel's essay, the advocated shift happened to be paradoxical, too. The essay requires of the ethnomethodological analyst to engage in the production of the phenomenon, to describe and teach its production methods 'from within,' as Livingston's study of mathematics did and is presented to have done (28–29). On the other hand, Garfinkel's essay, as the interview inquiry it reports and relies upon, flouts this prospect by virtue of its very form, as his 'summary of [ethnographic] dissatisfactions' amply suggests (47). The discursive articulation of sociological reasoning, or philosophical argument, remains deceptive. As Garfinkel put it elsewhere, 'you'll strive to make it right. And those [rhetorical] skills will assure that you'll miss the point' $(2024, 22-23)^{15}$.

Yet Garfinkel did not simply dismiss Agre's initial question on praxeological grounds, *but also acknowledged the 'potential of a consequential dispute*' (2022, 55; italics added), a dispute 'bearing specifically on the nature of discovering work in the natural sciences' (ibid.). What does this specific pertinence look like? To this question, Garfinkel's essay again gives a paradoxical, if provisional answer. On the one hand, Agre's question, when embedded in an

¹⁴ On 'perspicuous settings,' see Garfinkel (2002, 181–82), and on their contrastive interest for postanalytic inquiry, see Lynch (1993, 300–01). 'Contrastive interest' alludes to the heuristic interest of distinctive everyday situations—contrived or encountered—for illuminating a received scholarly concept.

¹⁵ Conversely, as an 'achieved topic of order,' any discursively available topic (or 'epistopic,' Lynch 1993) proves of praxeological interest only, if paradoxically, when it 'disappears and becomes instead an achieved *phenome-non*' (Garfinkel 2024, 27), a 'radical phenomen[on] of order' (Garfinkel 2021, 4) (e.g., a competent disciplinary practice, typically tacitly achieved, if not recognizably productive).

ethnomethodological study underway, is said to offer a starting point not only for charting 'new phenomena' (55) (e.g., practitioners' 'truth of the matter' *in situ*), but also for questioning received views ex cathedra:

For example, for the bench scientists and ethnomethodologists, evidence is no longer interestingly Evidence speaking in generalities and universally, but is evidence locally achieved and locally occasioned, witnessed, recognized, and understood in and as of a particular science distinctively in work-site specific coherence of detail* (Garfinkel 2022, 56).

This critical prospect anticipates the respecification agenda, as exposed in *Scientific Practice and Ordinary Action* in view of tracking 'iterable epistopics' (Lynch 1993), if incidentally articulated as 'epistemic sociology' (Coulter 1989). On the other hand, Garfinkel's interest in the 'natural sciences,' and the 'nature of discovering work' in particular, begs the question of computer programming, its distinctive practice(s), and constitutive particulars—the domain from which Garfinkel, as it seems, borrowed Agre's question in the first place. The open question, then, was and is perhaps best read as yet another research invitation to ethnomethodological inquiry, having computer programming and its distinctive contingencies probed and problematized (be it in their own terms, with respect to the 'natural sciences,' or both). So will the remainder of this paper, eventually attempting to move 'beyond' postanalytic inquiry—if that is possible at all!¹⁶

'Why Mars, with Mobots, Now?'—this is the working title of a companion paper on a pedagogical scenario for computer programming at school (Sormani et al., forthcoming). The abbreviation 'mobots' stands for 'mobile robots' and, in the case at hand, these took the form of small vehicles, equipped with two wheels and multiple sensors, an embedded computer, differential drive, as well as LED lighting (see Appendix I). Pitched as a 'Mars Mission,' the pedagogical scenario required students to program these mobots at a distance, for the programmed vehicles to follow a path on the Mars surface (as schematically shown via a YouTube connection) and, eventually, to have a Mars-based power station repaired remotely (see Excerpt 1). To have this scenario put into practice, an *intricate ensemble of instructed actions* had to be articulated and achieved *in situ*. 'Interactive programming' and 'scenario interfacing' (to gloss two aspects of this ensemble) were both drawn upon to have the task achieved, then and there. How? Let us pause on a programming episode where 'getting to the point' proved to be the practical priority, requiring the timely mastery of classroom contingencies, and thus offering an apt opportunity to respecify Agre's question (if not Garfinkel's complaint) in the light of situated practice¹⁷.

¹⁶ On Garfinkel's uses of asterisks (*), see Liberman (2007) and, with respect to 'detail,' Garfinkel (2022); Macbeth (2022). For prior studies of programming work, see Button and Sharrock (1995) and, more recently, Brooker (2019); Saha et al. (2023). From a Latourian perspective, see Jaton (2021).

¹⁷ Garfinkel's complaint about rhetorically, if skillfully, 'miss[ing] the point' (2024, 22-23).

A provisional transcript of the episode reads as follows:

Excerpt 1: The point in question: 'getting on a white spot,' intermediary task of 'Mars mission'

- I1. 2: Instructors
- S1, 2: Students

((S1 and S2 of group red sit in front of their control screen, I1 and I2 are making rounds))

((clicks on mouse, launches program))

- 19 I let you proceed, I will go and help |group yellow. 11:
- 20 S1:
- 21 S2: ((lays his head on the desk))
- 22 S1: ((takes his hand off the mouse, observes anticipated mobot move on screen)) #1
- 23



#1 ((observes and waits for anticipated mobot move on "Mars surface" to the right))

24		(15s) ()
25	S2:	((gets out of his chair, walks towards open door, runs out of classroom))
26		(3 s)
27	S1:	so we all need to get on a white spot (<i>un point blanc</i>) before-
28		<pre>((stands up and addresses I1 supporting group yellow))</pre>
29	l1:	yes.
30	12:	that's it.
31	S1:	okay.
32	I1:	when we all have reached a white spot, we can continue the mission.
33	S1:	okay.
34	l1:	((supervising group yellow))
35	12:	((approaching 'S1))
36	12:	so, 'here are we are waiting?
37	S1:	°yes.° () and then I will wait until the others-
38	12:	and now you've already launched it? ()
39	S1:	°yeah°
40	12:	oh, it's there,=>it's already there.<
41	S1:	voilà, I am on a spot.
42		((addresses I1
43		supporting group yellow))
43	l1:	yeah. sector B
44	12:	super. super.
45	11:	((starts applauding, so does yellow group)) #2 ((applaud together,
46	S1, I2:	((applaud)) as mobot reaches
47		#2 white spot))

Judged by standard practice in contemporary CA, and its 'multi-modal' update in particular (Deppermann 2013), the above transcript offers a rudimentary version (for transcription conventions, see Appendix II). Yet, and already in the light of that rudimentary version, the protagonists involved—two students, two instructors—seem to have organized their interaction intelligibly: not only its transcription turned out to be possible, but the transcription also allows us to analyze the episode (as part of an unfolding activity, as line numbering starting at 19 suggests). In particular, the action the students engage in, if instructed (line 19), appears as both screen mediated and verbally formulated.¹⁸

After having programmed the envisaged mobot move (i.e., before the transcribed episode), students use the computer screen as a control screen to monitor the mobot's actual move(s) in terms of the program (see lines 20-23, #1). Moreover, student action, awaiting the launched program to move the mobot, is verbally formulated. The verbal formulation articulates their instructed action, and delegated mobot performance, in terms of the routine pattern of classroom interaction, respectively a local variation thereof: an initiation-realization-evaluation sequence. Not only does the instructor (I1) *initiate* the task at hand (line 19), but in the course of its *realization* (20-39) the student (S1) also double-checks whether the task engaged in is the correct, and collectively required, one: 'so we all need to get on a white spot [?]' (line 27). Eventually, its *evaluation* takes a collaborative form, culminating in joint applause (lines 40-47). The students of this group did 'get to the point,' a white spot on the schematically represented Mars surface (#1). Yet, and contrary to what Agre's question suggested, it is the very specification of the contingencies required to be mastered for this task (i.e., as the 'intermediary task' at hand) that allow us, through its emerging particulars, to engage with the purpose of the activity underway (e.g., as part of a 'Mars mission'). The task emerges as one 'problematic possibility' among others. So does scenario interfacing, to whose reflexive explication we shall now turn.

3. BEYOND: REFLEXIVE EXPLICATION OF PROBLEMATIC POSSIBILITY

'Why we send Robots to Mars' (Vanarse 2023), this headline echoes the sales pitches of rocket entrepreneurs (e.g., Musk 2017). It also recalls post-colonial critique, if not chilling dystopia, and an ecological critique by the late Bruno Latour (2021). In a school context, a Mars mission surely provides students with an alluring scenario, having them 'program together,' enabling both computer programming and peer collaboration with mobots at a distance. Doesn't such training in collaborative '21st century skills'—the last century's 'soft skills'—disable 'Anthropocene awareness' though (Sutoris 2022)? And what kind of 'skilled community' is thereby assembled? Further description of interactive programming, as sketched out in the

¹⁸ In elaborating on 'instructed action,' as Doug Macbeth notes, 'Garfinkel was speaking of work that is prior to things like IREs [initiation-response-evaluation sequences] and directives' (2014, 304). In the next section, I will turn to (some of) this 'prior work' via a reflexive explication of practical engagement in the 'Mars mission' at hand. This move will also recontextualize Lynch's 'postanalytic inquiry' and tentatively displace its problem horizon—that is, potentially 'beyond' the epistemic register of instructed action and its praxeological validity.

previous section, might allow one to explicate additional "conservative' practices' (Langsdorf 1995, 184), conservative of the 'Mars mission,' assumed and enabled as a closed toy world in common. Yet indulging in that descriptive pursuit indeed begs the question of why *that* pedagogical scenario was or should be chosen, as one 'problematic possibility' among others, as a practical decision on its local relevance and perceived interest—in short, we're back to a, if not *Das Problem der Relevanz* (Schütz 1982)! ¹⁹

'So we all need to get on a white spot [?]' (line 27). What kind of 'instructed action' informed this question? Our practical involvement in scenario interfacing affords us with an instructive source to reexamine the episode analyzed so far:

- How was the task jointly achieved, as a particular *Gestalt* and recognizable step of the 'Mars mission,' its background (in this respect, see also Lynch and Eisenmann 2022)?
- And how, in and through that step achievement, was *that* scenario performed, in and as the relevant situation (i.e., as one 'problematic possibility' among others)?

To address these two questions, I will take my cue from the double interest of ethnomethodological description, empirical *and* pedagogical, as and when it tackles 'tutorial problems' (Garfinkel 2002). Drawing on the mentioned companion paper, I can share the following provisional observations, which—as the two questions above suggest—hint at the move 'beyond' postanalytic inquiry²⁰.

Empirically, engaging in the task proved doubly instructive. Task engagement not only involved the task-specific selection of a program sequence, but task-specificity and program sequence also elaborated each other in the process. Together, instructor (II) and students (SI, S2) figured out which mobot sequence to program (a 'right turn,' combined with a 'move timer'), while the selected task (to arrive on 'a white spot') took shape through this very process (as the selected one, 'to the right'). Indeed, only after having suggested a left turn to students (to reach the free spot on that side, #1), yet also noticed their already programmed 'right turn' sequence, the instructor would let them proceed (line 19). Their intended action, in short, appeared sufficiently 'instructed' for the pending task. This, however, proved not to be the case for all students, as the instructor's following utterance suggests: 'I will go and help group yellow' (line 19). Indeed, scenario interfacing was not limited to interstitial decisions on adequate program sequences. Throughout, it required numerous contingencies to be dealt

^{&#}x27;In order to study the problem of relevance in the domain of perception, we should remind ourselves [...] that *each perception includes the problem of selection*' (Schütz 1982, 44; italics in original, translation by the author). This problem of selection or choice among alternatives (*Auswahl*) concerns participants' orientations to one *Gestalt* rather than another *in situ*, thus appearing as one among other 'problematic possibilities' (Husserl) *in vivo*. This selective orientation or 'problem of relevance,' according to Schütz, was overlooked by Gestalt psychology as it tended to assume its Gestalt, rather than explicate the situation out of which it emerged—or indeed was '[coherently] produced' (Garfinkel 2021, 6–7)—as this or that 'alternative' (Schütz 1982, 52–3).

²⁰ Put succinctly, 'descriptions are provided for and 'readable' interchangeably as pedagogies' (Garfinkel 2002, 101). 'Tutorial problems,' in turn, emerge as part of technical self-instruction by the ethnomethodological analyst(s), progressively disclosing 'members' discipline-specific methods' (Garfinkel 2002, 145, chap. 4).

with, including 'making space in the curriculum,' 'having the internet connection set up and running,' 'deflecting skeptical student questions,' and so on. Taken together, the contingencies encountered proved of multifaceted interest, not least of which pedagogical—be it prior, during, or after the (re-)examined episode²¹.

Pedagogically, the encountered situation opened up an 'ethnomethodological niche.' None of the formal instructions (i.e., the 'Mars mission' documentation) did or could anticipate practical contingencies in situ (Tuesday morning, in the classroom, with teenage students, five minutes late, wobbly internet, etc.). Nor would a subsequent formal analysis recover them, presenting experimental psychology results of student performance under different conditions (Chevalier et al. 2022). As an intricate ensemble of instructed actions, the 'Mars mission' was composed of two principal activities, glossed as 'interactive programming' and 'scenario interfacing,' respectively. The scenario required students to engage in programming. Interfacing was primarily left to instructors. Occasionally, students called into question the scenario underway, as one student challenged our toy world, 'but Sir, we're not on Mars there!' (as shown in screenshot). In other words, this student challenged the pedagogical scenario as an 'unproblematic field' (Schütz 1982, 53)—that is, as the tacit field against whose unquestioned background the mobot programming activity was to be jointly pursued, its steps achieved, monitored, and evaluated, and its aim pursued (i.e., the 'remote repair of a power station'). Student interjections could (and did) challenge this *implied ecology of praxeological validity* in several respects, regarding its interactional routine, curricular interfacing, and/ or specific aim (i.e., 'remote repair on schematic Mars'). Most of these interjections proved 'tutorial' in one way or other, be it to have scenario interfacing improved or inspire a scenario change altogether, thus variably leading to 'figure and ground' interchange (Gurwitsch 1964, 110-112), potentially recontextualizing 'postanalytic inquiry,' and eventually displacing its problem horizon, too (see Appendix III)²².

CONCLUSION

Me: 'What were you just thinking?' Hans: 'What does the world need now?'

Together with two colleagues, Hans and Peter, I was sitting on the terrace of a closed restaurant, named *Ende der Welt* ('End of the World'). The restaurant went by the same name as the small valley we had just hiked up, and whose name had caught our attention, if only to bring our two-day Mountain seminar on ethnomethodology, sociology, and related matters to a respectable close. The day before we got caught up in fog. The well-indicated path had facilitated intense discussion of our recent and related publications (e.g., Eisenmann and Mit-

²¹ On such locally encountered 'constraints on [...] real-time* teachability,' see Garfinkel (2022, 24). As a classroom event, they led to 'figure and ground [being] interchanged' (Gurwitsch 1964, 110).

²² Just how are the observations, reported in the preceding two paragraphs, related to my teaching engagement? Pending the mentioned paper, I defer to Garfinkel's remarks on 'inspectably so' (2002, 211–12) and invite readers to consult Appendix III as part of that engagement.

chell 2022; Mlynář 2022; Sormani 2023), yet without leading to a clear conclusion. On the terrace, Peter and I ended up discussing 'research policy' relating to funding, positions, and projects, as I noticed Hans' silence. As the above exchange suggests, his searching answer to my direct question brought the conversation instantly back to 'world politics,' while taking into account our ironically unseemly location, the *Ende der Welt* somewhere in the Swiss Alps²³.

'What does the world need now?'—Hans' question, a sober variation on a Jackie DeShannon song (1965), can surely be put to Mars missions, and indeed it has, and probably should be. Aren't they indeed just another exercise in 'extrapolative' science fiction, extended extractivism, and/or extraterrestrial ethnocentrism? As we walked back to our base camp, Hans added a vocal complaint about sociological writing: 'why start out with anecdotes, rather than arguments!?'. To conclude, I would like to reflect on Lynch's 'radical ethnomethodology' (2016), if not anyone's, for 'post-normal times' (Thorpe 2022) and, for this purpose, address Hans' question in terms of his complaint first. After all, the complaint concerns this *Festschrift* contribution, too—if only for its two opening anecdotes, and the concluding third one, just offered in the same register.

Why anecdotes first? A short answer can be given with John H. Zammito, a historian of philosophy and social science, as he comments on stultifying abstraction in mainstream science and technology studies (STS"): 'hyperbolic 'theory' threatens especially the prospect of learning anything from others that we did not already presume' (Zammito 2004, 275). Among many other things, anecdotes in turn constitute a starting point for and/or incidental expression of an ethnography underway. To write it up, there might be many 'styles' (Van Maanen 2011), each of which lends itself to reflexive explication and ethnomethodological analysis, whether its observed field and target phenomena turn out to be an institutional reality (Wieder 1974), embodied science (Bjelić 2003), or academic text (Watson 2009). Not only are anecdotes, given their narrative form, 'packaging devices for culture' (Schegloff 1997:98), but they also lend themselves, and the 'culture' they articulate, to be 'unpackaged' (analyzed, studied, critiqued)²⁴.

The longer answer brings us back to sociology's 'what's new problem,' as articulated by Howard Schwartz in his (2002) essay, the tricky tension between (social) scientific approach and everyday phenomenon. Garfinkel's notion of 'respecification' (1991) accentuated the tension. First, the implied Schutzian contrast between practical relevancies and theoretical interest(s) multiplies 'what's new problems' against the background of many constituencies (of scholars, sociologists, scientists, professionals of various kinds, etc.). Second, the notion itself can only *allude to* an empirical solution (or conceptual dissolution). Indeed, everyday activities are assumed to already obey their own ordering work and practical intelligibility *in situ*, including their reportable character in anecdotal terms (i.e., prior to scholarly interpretation, not to mention 'hyperbolic 'theory'')²⁵.

²³ On the 'end(s) of ethnomethodology' and inspiring leads for respecification, see also Pollner (2012a, b).

²⁴ On the 'interactional unpackaging of a 'gloss' in conversation, see Jefferson (1985). On the embodied (re-) engagement with and technical 'unpackaging' of Galileo's pendulum, see Bjelić (2023).

²⁵ On Garfinkel's 'respecification,' related approaches, and their heuristic interest(s), see Button (1991a, b); Lynch and Bogen (1996); Sormani (in press) and the remainder of this conclusion.

One thing is to introduce and accentuate the tension, as Garfinkel crucially did, another one is to preserve and probe it, in and through distinctive studies of ethnomethodological interest, empirical and/or conceptual. Arguably, this latter move encapsulates Lynch's 'radical ethnomethodology' (2016) as a postanalytic program, as well as the *special interest of its double radicality* in and for 'post-normal times' (Thorpe 2022).

'Against, with, and beyond'—the three particles allowed us to chart the 'double radicality' of postanalytic inquiry, probing both social theory and situated practice, in and through some of the successive readings and contrasting reviews it invited, including my own. Most intriguingly, perhaps, it was the practical pursuit of empirical research *in situ* which, in and through a particular case (i.e., a 'Mars mission'), allowed us to renew topics and change scenarios, while explicating alternative foci as 'problematic possibilities' (e.g., interactive programming for energy extraction vs a broken ecology's renewed relevance). The contingencies encountered allowed us to problematize the activity's pedagogical purpose (*pace* Agre's question), while its sociological interest emerged—in potentially 'new, interesting or scientific ways'—in the very light of situated engagement (*pace* Schwartz's problem). The reflexive engagement in situated practice, here or elsewhere, highlights also the subversive heuristics of Lynch's (1993) respecification agenda. If I could at first invoke 'practical irrelevance' against Lynch's 'postanalytic approach' (1993) to science and technology, then this invocation was also possible *due to* the approach's own programmatic, theory-deflationary agenda. If 'institutional amnesia' could be highlighted as one of the program's (social-)theoretical deficits, then this critical possibility could also be turned *against* that arguable deficit, into a counter-factual maxim of empirical research. The expression 'double radicality,' then, stands for Lynch's unprejudiced articulation of two inquiry moments, instructive practice and conceptual critique, rather than (say) 'mandarin ethnomethodology' (Maynard and Clayman 2018)²⁶.

Back to Hans' question, reformulated: what is the special interest of Lynch's leitmotiv manifestly self-critical, not self-defeating—in and for 'post-normal times' (Thorpe 2022)? In 1993, a journal named *Futures* published 'Science for the Post-Normal Age' (Funtowicz and Ravetz 1993), a seminal essay in the field of science policy expertise by now. In a nutshell, Funtowicz and Ravetz argued that social change under the aegis of scientific progress—due to its detrimental consequences on the environment, humankind included—had ushered in a 'post-normal age' post-World War II. Not only would this contradictory situation confront policy-makers, citizens, with a new range of 'wicked problems,' but disciplinarily authoritative 'normal science' wouldn't generate, let alone guarantee, practice-relevant expertise, let alone political legitimacy (hence their plea for a broader 'public expertise' model, ibid.). Thirty years on, the qualifier 'post-normal' has become a recurring trope for tension-riddled societies, if not 'cannibal capitalism' *tout court* (Fraser 2022), prone to multiple crisis with no promising end in sight (e.g., melting glaciers, burning forests, displaced people). Incidentally, sociology itself (re-)appears as a tricky project, its formal methods projecting realistic models—proxies

^{26 &#}x27;Mandarin ethnomethodology' is a polemic shorthand for EM/CA based on scholarship, shorthand which unfortunately I cannot further unpack at this point. For a prior charge of 'crypto-radicalism,' put to ethnomethodological 'studies of work,' see Pollner (1991).

for 'normalcy' (Thorpe 2022:12)—yet missing out on lived realities, not to mention the 'everyday world as problematic' (Smith 1987)²⁷.

Let us conclude on, instead of starting out with, another *argument* then. Arguably, and in response to 'post-normal times,' there are at least two major stances of sociological interest, 'perversely conservative' and/or 'heuristically subversive.' As indicated, these are not necessarily mutually exclusive, and certainly not empirically balanced (Ivarsson 2023, 178). In his recent book, UCSD-based sociologist Charles Thorpe devotes an entire chapter to the first kind of stance, a chapter entitled '*hypernormalization* in post-normal times' (Thorpe 2022, chap. 1; italics added). The chapter, in essence, discusses the intriguing paradox of 'putting on a 'normalcy show' in a broken world,' while drawing together historical, sociological, psychological, and economic considerations for the purpose (Garfinkel, Goffman, and Gouldner included). In current sociology, taking on this stance amounts to redoubling the 'normalcy show,' *nolens volens* or despite better knowledge. For its conspicuous result, a garden variety of 'renormalizing science' exercises, Thorpe has a polemic passage in store:

[Mainstream] sociology's very means of detaching its knowledge from particularistic and everyday perspectives, through positivist methodology, also prevent sociology from doing other than reflecting the prevailing appearances of things. Its anti-septic scientism is also its anodyne middle-class professional complacency and unseeingness (Thorpe 2022, 4).

In its Marxian bluntness, this introductory passage lacks Lynch's 'nuance in action' (to adapt Coulter's 1989 title), and certainly his unprejudiced articulation of praxeological inquiry and conceptual critique (with respect to 'American sociology,' see Turner 2014). Yet to conclude this Festschrift contribution, and to celebrate Lynch's legacy, the passage does its job remarkably well, if only to keep (sociological) argumentation open as 'ethnomethodology's niche,' too. What's the point of reifying 'scientific expertise' in the singular, for example? Or what's the point of formalizing CA into 'information exchange' schematics? Or, for that matter, why venture an 'epistemological fusion' of CA and (say) experimental psychology? Taken together, these questions invite a 'heuristically subversive' stance in the face of proliferating 'academic hypernormalization.' Over and again, Lynch's subtly deflationist critique of 'normalizing science' has demonstrated why and how we should gladly accept that invitation, even or especially as 'party crashers' (e.g., Lynch 1991b, 1999, 2000b, 2000c, 2008, 2013, 2015; Lynch and Bogen 1997). On this festive note, at any rate, I conclude my *Festschrift* contribution. If the conclusion leaves open our introductory concern—'(cosmo-)politics' and its everyday moorings-then so much the better. It only suggests that there is ample reason, certainly in 'post-normal times,' to double down on radical ethnomethodology.

²⁷ For an inspiring, if frugal, Latourian reflection on 'how to turn politics around,' see Marres (2023). For a recent discussion in German sociology, see Lessenich and Scheffer (2024).

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APPENDIX I 'MARS MISSION' MOBILE ROBOT (MOBOT)



Image credit: Photograph taken by the author of 'Thymio' robot, an 'open-source educational robot,' see www.thymio.org

APPENDIX II TRANSCRIPTION CONVENTIONS

(1s)	Pause timed in seconds
=	No discernible interval between adjacent utterances, or activities
(go ahead)	uncertain hearing or seeing
((does))	Described activity, typically non-verbal
<u>so</u>	Emphasized stretch of talk
>so<	Faster stretch of talk
°so°	Quieter stretch of talk
?	Rising intonation, as in a question
,	'Continuing' intonation
	Falling intonation, as at the end of a sentence
[so	Overlapping utterances and/or activities
[this	
[((does))	
so	Utterance and activity overlapping, one sign per participant
((does))	
#1	Placement of screenshot, one number per screenshot

APPENDIX III 'WHY NOT VENUS?'—NOTES ON, FROM WITHIN, AND FOR AN ALTERNATIVE SCENARIO

Eventually, we were led to ask 'Why Not Venus, Next Time?' – rather than, as it were, 'Why Mars, with Mobots, Now?' (Sormani et al., forthcoming). The classroom exploration of the former question may renew student reflection on the 'greenhouse effect' next time, if not 'computing within limits' (Nardi et al. 2018), rather than the collaborative pursuit of 'energy extraction' yet again, as the following passage regarding the late 1970s research agenda of climate scientist James Hansen suggests:

[...] [At NASA] Hansen turned [...] to *Venus*. Why, he tried to determine, was its surface so hot? In 1967, a Soviet satellite beamed back the answer: the planet's atmosphere was mainly carbon dioxide. Though once it might have had habitable temperatures, it appeared to have succumbed to a runaway greenhouse effect. As the sun grew brighter, Venus's ocean, believed to have covered the planet by an average depth of eighty feet, began to evaporate, thickening the atmosphere, which forced yet greater evaporation—a self-perpetuating cycle that finally boiled off the ocean entirely and heated the planet's surface to more than 800 degrees Fahrenheit. At the other extreme, *Mars's* threadbare atmosphere had insufficient carbon dioxide to trap much heat at all, leaving it about 900 degrees colder. *Earth* lay in the middle, its Goldilocks greenhouse effect just strong enough to support life (Rich 2019, 30; italics added).

In the bulk of this paper, I paused on the allusive yet paradoxical character of Garfinkel's (2022) essay on the natural sciences. Why? For one thing, Lynch's recent edition of Garfinkel's essay allowed us to side 'with' his postanalytic inquiry, while articulating EM's interest in (computer) science in terms of its incidental respecification of prior scholarly analysis – that is, regarding idealized 'truth constraints' in and on computer programming (i.e., via Agre's critique, and Garfinkel's complaint). For another, the practical engagement in the target domain, through a pedagogical scenario and its tentative implementation, allowed us to move 'beyond' postanalytic inquiry. Instead of a received scholarly literature, as Lynch's postanalytic 'first step' would suggest (Lynch 1993, 300), our focal engagement cast into relief its *practically relevant* background to begin with – that is, the defeasibly implied ecology for pedagogical interaction in situ, as the encountered contingencies and my phenomenological reflections intimated (e.g., via Schütz's 'selective relevance' reminder, and Gurwitsch's 'ground to figure' reversal). Is that to say that we are back to 'against' again, a renewed opposition to postanalytic inquiry? No, as I suggest in the conclusion to this paper. In any event, it might be worthwhile to pause on its (quasi-)Garfinkelian thrust, if only to highlight the paper's pedagogical interest for an alternative scenario next time (e.g., 'why not Venus?').

In his later writings, despite or because of their 'trickster' style (Pollner 2012b), Garfinkel insisted on practical immersion and self-instructive practice as EM's key requirement and worldly orientation:

[t]here is no way in the world that you can learn the craft of Ethnomethodological inquiries that are directed to respecifying the topics of order, as achievements of ordinary society, just by reading about the results of EM studies. You must take on a problem and over a course of weeks work on it. [...]' (Garfinkel 2002, 168).

In this paper, I reported on a 'Mars mission,' as the intriguing classroom problem we engaged in, to have students in turn engage in remote mobot programming. This situated practical immersion allowed us to reexamine, if not respecify, familiar 'topics of order' in ethnomethodology/CA (e.g., IRE sequences, instructed action, praxeological validity). Hence an exercise in 'postanalytic inquiry' was documented. Beyond that, practical immersion also proved generative of new topics and renewed pedagogical vistas, if not 'post-normal' reframings (regarding implied ecology, a climate-out-of-kilter, 1970s climate science, etc.), thus inviting a topical recontextualization of 'postanalytic inquiry' in two ways. First, computer programming and its practical instruction were probed as new *empirical topics*, topics that have by and large escaped Lynch's epistemically oriented research program to date (but see note 16 above). Second, our tentative engagement in (computer) programming instruction invited a *conceptual* reframing, a reframing that recovered the 'implied ecology' of classroom computing and its praxeological validity (i.e., the contingently implied, yet contentiously closed toy world of Mars). With these contrasting orientations in mind, let us return to our student question: 'So we all need to get on a white spot [?]' (line 27).

What kind of 'instructed action' informed this question? A first answer focused on the 'praxeological validity' of the required mobot move(s). The description in the bulk of this paper, if provisional, indeed examined interactional routine as adapted to the task at hand (i.e., an 'initiation-realization-evaluation' sequence, to have a 'programmed mobot reach a white spot'). How to 'get on a white spot'? In Garfinkel's terms, *that* 'issue [could] get settled' (2022, 23), at least for the 'Mars mission' participants *in situ* (i.e., if and as they 'played along'). In turn, our practical involvement in the task, as part of the scenario sustained in the classroom, not only allowed us to list some critical contingencies to be managed for successful task achievement (e.g., 'five minutes late'), but those local contingencies also foregrounded the very background against which the 'Mars mission' and its toy world appeared as a pedagogical opportunity for computer science, notwithstanding the arguable nature of its implied ecology – that is, arguably, a 'technovision of the world, designed to produce workers for a global system of neoliberal capitalism that abides by the gospel of infinite growth and sees the natural environment as a resource to be exploited' (Sutoris 2022, 20–21)²⁸.

To cut a long story short, design choices, as they appeared in situated interaction, remain to be questioned (e.g., 'toy worlds' as trendy models? 'White spots' as valued steps? 'Right turns' as preferred options? Morally, practically, politically so?). Open questions, in turn, afford anyone with further 'pedagogical opportunities' (Chevalier, personal remark). Or, as Garfinkel put it elsewhere, 'once you catch on to the objects that these [e.g., a display of two white spots] can be provided for as the appearances of, then there's no end' (Garfinkel 2021:8) – including, say, the 'intersectional politics of planet selection' *in situ* – in and as, if not against, this or that pedagogical scenario *in vivo* ('Venus' vs 'Mars,' 'venture' vs 'mission,' 'left' vs 'right,' or 'green liberal' instead, etc.).

²⁸ For a genealogy of this 'technovision' via 20th century 'closed world/cold war' politics, see Edwards (1997).