

# kierkegaard II

*The Sequel*

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## — CULTURAL STUDIES IN CULTURAL STUDIES

In what follows, I want to discuss three audience responses to ‘Kierkegaard: The Movie’, a paper I delivered at the Cultural Studies Association of Australia’s annual conference in December 2001, and to show where those responses led me. The reason I am doing so is that I am more and more convinced that our theories of ideology suffer a fundamental flaw. They fail to incorporate the richest source of data that we, as humanities academics, have at our disposal: the fact that we are all teachers. What richer source could we have for studying the transmission of ideas and beliefs than our own social practices? I am referring not only to the classroom, but also to our conferences, and even to our collegial visits to the pub. Wherever it is that university people garner new ideas and directions, *that is where we will be most likely to learn about the mechanisms of cultural and indeed political transmission.* So rather than set forth a new reading of Harold Garfinkel’s *Studies in Ethnomethodology*, I want to show how my new reading was generated in response to the comments and critiques I received when I delivered my first Kierkegaard paper.<sup>1</sup> The result will seem chatty and informal and that is precisely my point because I’m using the data most ready to hand to further the study of where ideas come from: people like us.

The main polemic thrust of my Kierkegaard paper was the claim that a radical academic politics would, regardless of left or right and other such group affiliations, involve opening a space for new types of intellectual practices. I argued that cultural studies would have a real (as opposed to just postulated) political impact if it saw as its charter to provide space and facilities for otherwise unthinkable projects. For you cannot teach students how to learn

by simply giving them facts. You have to provide an example of learning for them to emulate. In other words, teachers need to show the same radical openness to the new that they wish to inculcate in their students, or they are simply not teaching. They can do so by supporting, and allowing themselves to be engaged by, crazy projects: their own, or those of others. That's the world I want to be in. So if, in 'Kierkegaard II: The Sequel', I seek to document how I learnt from enacting 'Kierkegaard: The Movie', and to think through how one learns from cultural events more broadly, it's with the aim of expanding the space of learning in general.

So, whatever happened to 'Kierkegaard: The Movie'? I was on a panel of Derrideans who all said nothing. I guess he hasn't written a book on surfing yet. Not that I wanted to minimise Derrida's influence on us in the Cultural Studies Association of Australia, but rather to redirect it. For the truth of the matter is that the definition of revolution ('taking ideas as things to act first, and then theorise later') and poetry, which I garnered from Kierkegaard's *Repetition*, and then sought to apply to Wagner while learning to surf in Cornwall, is less a critique of Derrida's *Of Grammatology* than an explanation of its power.<sup>2</sup> *Grammatology* doesn't tell us what deconstruction is, it just does it, smashing whole fields of knowledge with one and the same exploding kernel of post-Saussurean insight, letting the ideas, terminologies, systematisations and so forth come later. *Act the idea first, theorise it later* (as perhaps happens with all learning).

Which brings me to my first respondent, who said something very similar. Amanda MacDonald drew an analogy between the model of experimental practice set forth in my 'Kierkegaard: The Movie' and her experience of foreign language teaching. Amanda argued that the teaching of language is fundamentally concerned with changes of mechanical practice and habit. It forces a radical altering of one's habits of bodily association (sound production, hearing, recognition of visual markers) and of intellectual association (new lexicons and new grammars by which to link them), which is why learning a foreign language is both so traumatic and so utterly liberating. It forces you to see if repetition is possible, to change your daily motions, and so serves to generate new ideas and experiences.

Something in the links Amanda offered between foreign language, trauma and change inspired me to bring my experience of learning and teaching into the space where I usually consider politics and culture. I was interpellated to think through a new frame. And to remember that it was while I was studying Beginner's French, a first-year subject, and round the time of those two dance parties that I had my first experiences of gay sex—I have no idea why I just wrote that. I was twenty-eight, already with MA and teaching experience. I turned up as a student for my first class, an intensive summer-semester course, only to discover that it was in fact located in the very room in which I had, only two months before, taught my last class. I had been tutoring in a course devoted to the history of piracy. As you do. Suddenly

I was on the other side of the desk. And getting ideas. All the while rehearsing for *RED RAW: A Revolutionary Dance Party*. And dragging up all my old Russian too. Experiencing ‘the transformation of metaphor into metamorphosis’ as the Russian linguist and émigré Roman Jakobson once phrased it, describing the practice of Velimir Khlebnikov, the great pre-revolutionary Russian poet.<sup>3</sup>

Stephen Muecke, to turn to my second interlocutor, responded to the paper by suggesting that I had made a shift from the melancholy perspective of my earlier book, *From Here to Tierra del Fuego*, which had sought to know the truth of the past of that little South American island and from there the rest of the world, through a massive Marxist apparatus.<sup>4</sup> Having launched that very book a few months earlier in Melbourne, Stephen proceeded to suggest that I was now treating truth, or whatever functions in its place, as something that has its home in the future. This suggestion suddenly crystallised in me the realisation that I had indeed, as I grew older, shifted intellectual trajectories. Immediately I thought up an analogy of my own. My work on Kierkegaard suddenly, because of Stephen’s usage of the word ‘future’, seemed directly linked to what I had been reading about Charles Sanders Peirce, whose most compelling semiotic theses are, as Jakobson once remarked, concerned with the future-orientation of meaning.<sup>5</sup> As Peirce put it, the ‘meaning of every proposition lies in the future’.<sup>6</sup> This is because, now in Jakobson’s words, ‘the sign opens a path toward the indefinite future, that is, it anticipates, it predicts, things to come’, and it does so on the basis of its prior history of meaning.<sup>7</sup> Yet that past history can never fully saturate a future one:

it is clear that the frame law is only a condition for all possible future occurrences [while] it is in the context of each occurrence that the verbal invariant of the verbal sign—its general meaning—acquires its new, particular meaning. The context is variable, and the particular meaning of the word undergoes renewal in each new context.<sup>8</sup>

And how is this all about learning? The analogies offered by both Amanda and Stephen, in response to my work, in fact served to generate new projects, this one included. And I have been led to suggest, simply following the chain of my own associations, that these interlocutions (for it is the reality of change that I am attempting to track here, the creative power of dialogue) had both a prospective and a retrospective power. In hearing other people’s analogies and metaphors for creative learning I was being directed back into my own most intense experiences of newness, those times (and everyone has them) where one experiences the ‘transformation of metaphor into metamorphosis’. Now I want to disrupt this narrative to introduce the event that really sparked this writing.

The third of my respondents, John Frow, suggested I think again. The problem, he argued, with founding an academic discipline on the model of creative practice—the problem with, in my words, ‘making Cultural Studies the experimental, which is to say, the radical wing of

the humanities', the problem with acting the idea first and theorising it later—is the problem of institutionalisation.<sup>9</sup> John didn't refer to the Russian Revolution but rather the recent 'happenings' of the 1960s. He said that my Kierkegaard paper had reminded him of Harold Garfinkel and ethnomethodology, the branch of sociology which Garfinkel named and so invented. Garfinkel encouraged his students to perform experiments in the disruption of everyday life. In one such experiment, students were asked to go home and act as if they were lodgers. The reactions, variously intense and hostile, that this experiment elicited served to reveal the precarious, and in many ways invisible, sets of rules and regulations governing such seemingly natural practices as the family home. Such experiments, John said, were obviously quite powerful and new, but the attempt to institutionalise them within the bureaucratising context of university life was not a success. By the early 1980s Garfinkel's ideas had been heavily criticised and were already becoming outmoded and unfashionable. How, John asked me, do you institutionalise creativity, something inherently anti-institutional?

Is it possible to institutionalise creativity? Or is it only ever an accident in a world of reification and retrospectivity? My gut feeling, during that question session, was that there was more to say on the topic, but I couldn't find it.

I was a bit disconsolate at the fact that my triumphal and revolutionary paper had a bit of a hole in it. One of the things about doing experimental (fictocritical) writing is that people don't often criticise it; they usually just ignore it. Perhaps some of the reason for writing in a crazy way is to put yourself beyond critique. As in the dumb reason. It's only the real critics like Frow who will cut through the crap and treat your message for what it was: an attempt to speak well. Thank God for our critics.

#### — THE DISCONTINUOUS INSTITUTE

So, how do you institutionalise creativity, something that undermines, and makes subject to time (we have no idea what the future is!) places that don't want to be? A few months after the conference, I stumbled across a paper by Harold Garfinkel titled 'Practices for Following Rules and Applying Instructions' in *The Penguin Modern Sociology Reader* (ed. Peter Worseley, 1970, second-hand).<sup>10</sup> The *Reader* features on its cover two long-hairs (a tall male with beard and scarf, and a woman with a fringe that comes down over her eyes) alongside a close-cropped pinstriped business type. All three are Anglo. Next to the pin is a short Indian male in a hound's-tooth suit, with wide lapels and huge sideburns. *The Penguin Modern Sociology Reader* is clearly out of date. Not to mention bizarre. For it features Garfinkel. And he is out there. In his talk on rules and instructions Garfinkel argues that one finds 'in the complex of ordinary, mundane accounts that there are practices for locating monsters but there are also practices for burying them. There are practices for refusing the existence of exceptions.'<sup>11</sup>

A monster, it turns out, is a way to stuff up a list of rules and instructions. It's the exception the rules have not covered. So if we are playing tick-tack-toe, and the rules say that 'two persons play', a monster invokes questions such as 'Any two persons? When, today? Tomorrow? Do we have to be in sight of each other? Can we play by mail? Can one player be dead?'<sup>12</sup> It rapidly becomes apparent that no set of rules or instructions, whatever the game, whatever the genre, or even institution, is ever exhaustive. As Garfinkel puts it, with some glee: 'My classes can tell you that creating such problems is the easiest thing in the world'.<sup>13</sup>

Though that is an understatement. The following strikes me as pure genius. I mean, imagine teaching your students to generate this little monster:

We sit down to play a game, and we decide that it is going to be chess. You make your move. I take all of the pieces off the board, and I shake them up, and this may go on for three or four minutes. Then I finish, and I put the pieces back on the board. The positions have not been changed. Now the thing that is apt to happen is that the person grabs my arm and asks, 'What are you doing?'

I answer, 'I'm playing chess.'

'You're not playing chess.'

'Show me in the rule book where it's not permitted.'

'All right, it's not in the rule book. It's still illegal. Why are you doing it?'

'I find that whenever I do it, I win.'

'Oh, what a load of bunk that is. Nevertheless, don't do it.'<sup>14</sup>

Such scenarios serve to reveal the 'located character' of the rules of chess, the fact that they only make sense amid a further set of (invisible) rules and instructions.

#### — WEIRD SCIENCE

Wild teaching. Wild implications. Of course, it didn't survive. The sort of work Garfinkel promoted was clearly very dependent on his own personal lunacy. Once you lose such a performer, you only with difficulty find others who are capable of a similar logical anarchy. Garfinkel's is an example of a successful, but brief, personality politics. For as institutional policy, it was doomed to fail. People don't like to change their habits. However officially radical, people, both within the university and outside it, are basically conservative. They do not want the shock of the new to disrupt their breakfast. And lunch. And dinner. For three weeks. Of your kids pretending to be lodgers. I may sound a bit conservative myself here, but I am trying to come to terms with Frow's critique, with my own personal experience, and with the actual fate of Garfinkel's work.

The trajectory I have been sketching led me from this particular chapter in *The Penguin Modern Sociology Reader* to *Studies in Ethnomethodology*, Garfinkel's most famous work. From

there I turned to the various appreciations, debates and critiques to which that 1968 book led and I'll use some of these texts to outline further the issues Harold Garfinkel raises for the questioning of ideology. A brief case study of the fate of Garfinkel's work, and the way it was received in collegial spaces similar to ours, should help us to appreciate the possibilities for enacting similarly crazy programs among us. This will also provide me with the space to hone my answer to John Frow's question: how do you institutionalise creativity?

So what is ethnomethodology? Garfinkel begins *Studies in Ethnomethodology* by invoking Emile Durkheim's foundational characterisation of sociology as the discipline that studies 'the objective reality of social facts'. He proceeds to assert that this facticity should not simply be taken as a given. It is an 'ongoing accomplishment' of the various agents in a society and needs to be analysed as such.<sup>15</sup> The research Garfinkel then sets forth aim to investigate how the facts that make up social order are accomplished. That is, he analyses how people render their lives 'accountable' to the various systems of authority they encounter, and how they theorise what they are doing in the process. He does so through the study of cases like that of Agnes, an intersex who wishes to pass as a woman.<sup>16</sup> Garfinkel treats Agnes's gender as her own performative 'accomplishment', and in the process allows room for agency in a category that other sociologists would simply regard as a given part of being born into whichever particular society. This focus on the performance of one's facts is evident in the name Garfinkel gave the discipline, using the analogy of ethnobotany. Just as an ethnobotanist studies the practice of plant labelling and usage within a given indigenous group, an ethnomethodologist studies the practice of social-fact labelling and usage, that is, the methodology through which a given group approaches its world. For Agnes is at once a representative of a particular ethnos, the intersex, and a 'practical scientist', who necessarily theorises what she is doing, if only so as to succeed at it.

The research Garfinkel's new discipline led to was fundamentally descriptive, indeed, programmatically so. Garfinkel advised researchers to assume an attitude of 'ethnomethodological indifference' to the larger political and social categories (for example, class and capital) of traditional social inquiry, all the more to open their eyes to lived categories of their subjects. This 'indifference', as I will show, has been cause for substantial critique. That noted, there is a clear, albeit unstated, political edge to ethnomethodology. As Michael Lynch, one of Garfinkel's students, remarked, the very labelling of the discipline served to problematise 'the division of labour between the social scientist and native practitioner'.<sup>17</sup> After all, the focus on a subject's 'performances of method' could drive one to entertain a certain theatricality, or performativity, in one's reading of the social scientist's factual 'accomplishments' as well.<sup>18</sup> In other words, it could lead you to 'question the very foundations of social science knowledge'.<sup>19</sup> Maybe it is just one knowledge, among others. Maybe there are scientists everywhere.

Clearly there are parallels to cultural studies here, in the multiple ontologies such a program implicitly postulates for the various groups in society, and the desire to regard those groups' epistemologies as equally significant to those of the academic sciences. That said, how many of us actually teach our students to dump the *Cultural Studies Reader* and instead learn about the "seen but unnoticed" background of common understandings' all around them, by engaging in the 'demonstrations' that will serve to reveal it? How many of us stand behind the lectern encouraging students to bargain in shops with fixed prices, to perform the lodger experiment on their parents (who might complain to the university about the disruption this causes), or to insist, the next time they speak to a friend, on explanations for the meaning of common phrases?

Let me turn to some of Garfinkel's critics. Sarah Bailyn, in the course of her recent article 'Who Makes the Rules? Using Wittgenstein in Social Theory', seeks to show the problems, both philosophical and political, innate to Garfinkel's approach to 'rules'.<sup>20</sup> Bailyn reminds us of Garfinkel's desire in founding ethnomethodology to avoid the sort of sociology that presented its subjects as 'judgemental dopes', that is, as little more than mouthpieces of the social structures (for example, working class culture) and orders into which they are born. Given it is continually achieved, 'social order is contingent in ethnomethodology. It is not a foregone conclusion.'<sup>21</sup> Yet for Bailyn, who writes in the poststructuralist tradition of the latter Wittgenstein,<sup>22</sup> Garfinkel's desire to show his subjects' agential role in performing the social facts of their existence is defeated by the conformist view of society his work actually presupposes:

Garfinkel's famous 'breaching' experiments for example, *depended* on the participants sharing assumptions ... which the students would then deliberately break in order to see what happened. These experiments would not have worked so well among participants who did not share assumptions with one another.<sup>23</sup>

Bailyn compares her own lived experience of the multiplicity of social orders to highlight Garfinkel's 'lack of consideration of subgroups in society, or meanings which are not shared amongst all members of a community'.<sup>24</sup> Going to the bank, for instance, involves her in a multiplicity of rule systems and language games, whereas

the homeless man sitting outside the bank, hoping people will give him a few coins while their wallets are open, clearly does not have the same meaning/use of the banking system. It does not form part of his social structure in the same way.<sup>25</sup>

Embracing Wittgenstein's call to avoid treating concepts like 'society' as metaphysical entities, Bailyn suggests that the 'background of common understandings', which Garfinkel claims

his experiments reveal, is one such metaphysical entity.<sup>26</sup> The very idea of ‘common understandings’ collapses the multiplicity of societies within any one society into the fantasy of one. Garfinkel simply assumes that what his experiments reveal to be true of one member of his society will therefore apply to all members of that society. This assumption effectively does make ‘judgemental dopes’ of them all.

Bailyn’s critique certainly seems valid and appropriate to the evidence she cites. But her citations are selective. It’s not simply that she ignores the accounts, in *Studies in Ethno - methodology*, of people like Agnes. She ignores moments within the very chapters she cites. For the Garfinkel who invokes the ‘background of common understandings’ in Chapter 2, ‘Studies of the Routine Grounds of Everyday Activities’, proceeds, in that same chapter, to analyse the ‘retrospective-prospective’ nature of meaning.

In one of his famous experiments, Garfinkel encouraged his students to interrogate their subjects on the meaning of common phrases that arose in the course of conversation. Consider the following transcript:

On Friday night my husband and I were watching television. My husband remarked that he was tired. I asked, ‘How are you tired? Physically, mentally, or just bored?’

(Subject) I don’t know, I guess physically, mainly.

(E) You mean that your muscles ache, or your bones?

(S) I guess so. Don’t be so technical.

(After more watching)

(S) All these old movies have the same kind of old iron bedstead in them.

(E) What do you mean? Do you mean all old movies, or some of them, or just the ones you have seen?

(S) What’s the matter with you? You know what I mean.

(E) I wish you would be more specific.

(S) You know what I mean. Drop dead!

By derailing a typical exchange, the transcript forces us to think through what usually take place. Usually, one sutures over potential idiosyncrasies and exceptions on the grounds that the subject saying *this* (‘All these old movies’, which if taken as a universal judgement is obviously absurd) must have actually meant to say *that* (‘the ones I’ve seen’). By interrogating their subjects on matters of meaning that we would usually either assume or allow to emerge in the course of the conversation, Garfinkel’s students demonstrated that in conversing, one is always ‘waiting for something later in order to see what was meant before’.<sup>27</sup> Common understandings are retrospectively imparted, for common understanding—meaning itself—comes from the future and after the breach.



That's why the students' victims get so annoyed: if you question them on the meaning of statements that seem perfectly obvious, you are actually forcing them to articulate their reliance on the fantasy (one of our deepest—"You know what I mean") that we all speak the same language. We rely on this fantasy simply so as to speak. Yet it's an 'accomplishment' much more than a given, and, in this respect at least, it's like any other practice of rule following.

Not only do we suture over breaches to create order, but the existence of a breach often in itself creates order. A breaching experiment often causes its subjects to articulate, and at times create, rules that might not actually hold sway, or be so widely held, otherwise. We see the order of a set of 'common understandings' in the past precisely because it's just been shattered, which is how we are tricked into thinking it was 'always-already' there. How else are we to understand the extraordinary power of Derrida's *Of Grammatology*, which functioned not merely to shatter the corpus of structuralist knowledge, but to promulgate a set of rules as to how that corpus should be read? Since Derrida we have had one structuralism, which, ranging from Claude Lévi-Strauss to Roman Jakobson, has been seen to suffer in all its parts from the same fundamental flaw Derrida diagnosed in it. Assessments of 'the structuralist era' come from the future, and after the breach, where they turn into habits and purport to truth.

To talk of 'the post-structuralist order' in these terms brings out the real issue raised by Bailyn's work: there are places and periods in which creative acts, such as Garfinkel's breaching experiments or Chisholm's dance party or Derrida's *Grammatology*, serve to elicit the new, and there are those in which they give birth, if they arise at all, either to repression or—perhaps through the sheer multiplicity of language games on offer—to indifference. Myself, I think the move in Bailyn's article from the supposedly closed systems of Garfinkel and Anthony Giddens (her other subject, whom she attacks with greater accuracy) to the concluding picture of a society of multiple and incommensurable practices (not just meanings, à la Derrida, but practices, usages of meaning) is really very powerfully creative. It destroys 'society' and certainly changed my day, however much I think it mangles old Garfinkel's (though in a way it really just resurrects his project, which is why I like it). Clearly it's possible within poststructuralism actually to be poststructuralist.

Jakobson writes profoundly of the diminution of creative possibility in his 1930 essay 'On a Generation that Squandered its Poets', which should one day be regarded as the founding text of a science that analyses the possible conditions of creative acts.<sup>28</sup> Eulogising the recently suicided poet Vladimir Mayakovsky, Jakobson sets forth the clearly suicidal strains within Mayakovsky's writing, tendencies apparent even before the revolution, and the creative repression it eventually heralded ('Hadn't I better just/ let a bullet mark the period of my sentence').<sup>29</sup> He sets these strivings alongside an account of the increasing impossibility

within a Stalinising Soviet Union for those of the 1890s generation to live out their creativity ('a generation', now in 1930, 'allotted the morose feat of building without song ... impoverished in the most real sense of the word').<sup>30</sup>

#### HOW DO YOU INSTITUTIONALISE CREATIVITY?

I'll turn to the second critique of Garfinkel's work that I want to consider, that of Anthony Giddens. Giddens discusses, and eventually dismisses, ethnomethodology in the course of his 1993 *New Rules of Sociological Method*.<sup>31</sup> Unlike Bailyn's, his critique shows an awareness of the multiple rationalities Garfinkel did indeed postulate for the various *ethnoi* he studied.<sup>32</sup> Yet Giddens accuses Garfinkel of totalising the field all the same. The problem, Giddens writes, is in ethnomethodology's assumption that it is the desire for accountability that drives social actors:

identifying rationality with 'accountability' cuts off the description of acts and communications from the analysis of purposive or motivated conduct, the strivings of actors to realise definite interests ... 'doing bureaucracy', 'doing nuclear physics' ... '[d]oing a social practice' is much more than rendering it accountable, and this is precisely what makes it an *accomplishment*.<sup>33</sup>

Again this seems quite a valid critique. One could attempt to obviate it through psychoanalytic theory, taking accountability as a reference to the superego and the reality principle intertwined in it. For 'the prohibition of incest is nothing other than the sine qua non of speech'.<sup>34</sup> In speaking, one simultaneously passes into the register of prohibition that provides expression (as the inverse possibility which any prohibition brings to light), and indeed being, to desire. That is, the act of speaking reality (say, in a scientific report) is at once a rendering of accounts to the superego, and the various social agencies in which we incarnate it (for example, one's scientific peers), and at the same time shot through with 'purposive or motivated conduct', shot through with 'strivings ... to realise definite interests', shot through, that is, with unconscious desire. One could argue in this way, and so ignore Lacan's own comments as to the metaphysical stupidity of psychoanalytic theory, its subordination to, and often detrimental effect upon, the practice of intersubjective intervention, which practice in the psychoanalytic clinic in fact involves. Myself, I prefer to follow Lacan here.

I accept Giddens's critique. What I want to do, on the contrary, is focus on the agenda that drives it. I want to focus on the set of 'accomplishments' that Giddens wants to preserve from Garfinkel's radical levelling of scientific and practical rationalities under the rubric of 'accountability'. One is the accomplishment of the scientist. From asserting the inadequacy of the concept of 'accountability' to comprehend agential strivings, Giddens moves straight to an analysis of ethnomethodology's inadequacy to validate any sort of knowledge claims. Given,

Giddens argues, that ethnomethodology can itself be seen as just one more theatre for the performance of accountability, shouldn't one assume an attitude of 'ethnomethodological indifference' to its claims too (just as one assumed it in relation to the class and capital accounts of orthodox sociologists?).<sup>35</sup> Its findings will then seem no more valid than that of any other practice, something one might want to celebrate in postmodern style. But how do you then deal, as Giddens now asks us to, with the status of mathematical knowledge, the fact that it's pretty hard to relativise, or for that matter deconstruct, ' $2 \times 2 = 4$ '.<sup>36</sup> It is interesting to see Giddens move so swiftly from a critique of 'accountability' qua agential strivings to a critique of it qua truth-functional knowledge claims, something he wants to allow not merely in mathematics, but—by reflection from the mathematical case—in the social sciences too. The swift move is interesting, for the critique of accountability in terms of purposiveness seems a clear reference to the realm of politics. Guessing at the agenda driving Giddens here, I think he is actually getting at the political function of objective analyses of social facts like class, capital, structuration and so forth. I think he is suggesting that in abandoning truth-functional knowledge, ethnomethodology effectively abandons politics. I may be wrong in my reading of Giddens, of course, for these connections are by no means explicit.

As for the explicit critique, it seems quite a valid one. It raises issues. Only those issues have less to do with Garfinkel than with poststructuralism more generally. For the funny thing is that, however much Giddens's critiques might relate to some of Garfinkel's programmatic statements, when you look into the archive of ethnomethodological investigations you realise that these researches are actually very empirical, and in that sense thoroughly truth-functional. Take the 1981 article Garfinkel wrote with Michael Lynch and Eric Livingston, 'The Work of a Discovering Science Construed with Materials from the Optically Discovered Pulsar'.<sup>37</sup> It focuses on the 'quiddities' of the astrophysical discovery it observes, and gives appendices, where you find taped conversation transcripts, logbook entries and even the scientific article in which the discovery was originally communicated.<sup>38</sup> It's hardly Jean Baudrillard. As Gerald Holton remarks, in his comments on the paper at the conference where the Pulsar paper was delivered, 'Garfinkel ... first and foremost ... is an empiricist, a type regarded by some as dangerous'.<sup>39</sup> Garfinkel's colleague Michael Lynch, who also characterises himself as an empiricist, argues that the

*claim* in ethnomethodology—a fallible and arguable claim in particular cases—is that the phenomena studied furnish interpretative insight as well as materials for investigation.<sup>40</sup>

In other words, it's an empiricism that discovers not merely social facts in the texture of others' realities but conceptual categories as well. Though it stays within the knowledge protocols (for example, *fallibility*, Popper's criterion for scientific respectability, which is even

regarded as conservative by some) of academic practice. We are hardly tumbled into the epistemological 'abyss' Giddens derives from all this. Of course he is relying on Garfinkel's rather fuzzy theoretical statements as to accountability and ontology, rather than the record of what research the latter has actually done.<sup>41</sup> That Giddens has really missed the point here is apparent the moment you consider that much of the critique of Garfinkel has focused on his very empiricism, a charge made on the grounds (very familiar to cultural studies academics, who tend, by and large, to empiricism too) that he concerns himself only with trivial and insignificant realities.<sup>42</sup>

At this point I want to shift from an account of Giddens's criticisms to a consideration of the real question they bring to the table. How can the representation of objective reality constitute a political project? What can empirically based ethnomethodological accounts of the methodology by which coroners perform their investigations after 'sudden, unnatural death' (to mention one of Garfinkel's queerer studies)<sup>43</sup> have to offer the very serious, difficult and protracted struggles over issues at the centre of contemporary political life, both in Australia and worldwide? In asking this, I am implicitly asking about the politics of this very paper of mine, this cultural study in cultural studies, with its account of the practice and production of cultural studies: the teaching, the conferences, the formal and informal interactions (and how do you institutionalise creativity?). What's the point? How can a description of reality in itself be revolutionary? How can it help to institutionalise creativity?

The radicalism of this proposal (that a description of reality might itself foster change and creativity) is—like Benjamin's 'Theses on the Philosophy of History', like Bailyn's essay cited above, like Garfinkel's work—predicated on the belief that first, a multiplicity of worlds/readings are in existence at any given moment, and second, they can be translated into the present.<sup>44</sup> So Garfinkel would remind us, in arguing that the

formula 'The objective reality of social facts is sociology's fundamental principle' is heard by professionals, according to occasion, as a definition of association members' activities, as their slogan, their task, aim, achievement, brag, sales pitch, justification, discovery, social phenomenon or research constraint.<sup>45</sup>

Garfinkel reminds us that there are innumerable possibilities—call them ontologies—within the reality of that very formula. All are real, and any one can be translated into the present. That's the 'objective reality' of the concept 'objective reality', its own historicity as a concept that has been historically received in all these different ways. I have tried to broach a similar phenomenon, in my cultural study of a cultural studies conference and its effects (one of which was my incitement, by John Frow, to read all this Garfinkel). One can see the discipline as a radical practice of dialogue, exchange and learning, precisely by choosing to focus on the facts of its conferencing activities, rather than the ideas in its journal

publications, which are pretty meagre in comparison. By focusing on a particular one of its realities ('a past deferred to the future', a 'revolutionary utopia'), one attempts to sway the discipline to become more that way inclined. For describing reality can be a very political act indeed. It can be a form, as Gramsci reminds us, of governmentality.

So, Garfinkel's focuses our reading of the objective reality of rules and instructions on one of that field's particular realities (namely, their procedures for dealing with exceptions—'All right, it's not in the rule book. It's still illegal.')

and not on others (for example, the contributions rules make to the causes of peace and harmony, which are real, and can be documented). In doing this, he opens our eyes to the cracks within the various systems all around us. He effectively gives us a primer for how to stuff up lists of rules and instructions, even though he never directs us to do so. He gives us an example. That can be an enormously political thing to do.

Look at the preface to *Studies in Ethnomethodology*, where we read (recalling the 'monsters', 'practices of etc', 'ad hoc' ...) an acknowledgment that 'David Sudnow worked to the limit of his patience to improve the writing'.<sup>46</sup> Why don't commentators notice these things? Instead we have Bailyn criticising the inadequate poststructuralism of Garfinkel's work, Giddens attacking its inadequate structuralism. They can't both be right. Or rather, they should realise that they can. In Giddens's case, it's right there on the page. Attempting to recount Garfinkel's views on the difference between the practical and the academic scientist, Giddens on one page describes how Garfinkel regards scientists as practitioners of a 'context free' rationality (which is why their findings are irrelevant to the majority of the populace, who always speak contextually).<sup>47</sup> A few pages later we read Giddens's critique of Garfinkel's eradication of the difference between practical and academic science, with no reference to the prior and contradictory characterisation of his views. In truth, both characterisations are valid. It is amusing to read Giddens add the caveat that ethnomethodology 'cannot readily be evaluated as a whole' because of the clear differences between the views of its founder and those of his followers.<sup>48</sup>

Reading Garfinkel to the letter will involve returning to the exegetical process I enacted above. That is, it will serve to return me to the historical trajectory of this paper, which is on its way to an answer to Frow's question (but how do you institutionalise creativity?), an answer that will involve a focus on the letter of that very question. But first, before returning to the world, a comment about science—which might, as I've begun to suggest, be nothing other than just such a return.

What cultural studies lacks is a conception of itself as an experimental science. I've suggested some parallels between cultural studies and ethnomethodology above. What emerges from this, for me, is the comparative quietness of the work we produce, which seems to conceive itself as the analysis of (social and theoretical, but don't confuse the two!) texts, much more than the transmission, performance or elicitation of knowledge. The same

with our treatment of theory. There was an interesting discussion on the CSAA website a number of years ago, where various people were asked to name what they saw as the canon of cultural studies texts. Some provided such a list, some rejected the validity of canonisation altogether, and some discussion ensued.<sup>49</sup> The question of our role in the transmission of knowledge certainly emerged here, which I think is a healthy sign. But it's tame compared to Garfinkel. His way of problematising the canon was to make problems for it, by encouraging his students to take the canon of phenomenological literature and to 'misread' it, without specifying what that might mean.<sup>50</sup>

By encouraging people actually to produce knowledge in the world, Garfinkel might be seen to be departing from the realm of academic knowledge. One could read this along the lines of the opposition set up in Marx's ever-quoted eleventh thesis on Feuerbach: 'The philosophers have only *interpreted* the world, in various ways; the point is to *change* it'.<sup>51</sup> So we could say that Garfinkel, in inducing his students to misread the canon, dared to leave traditional knowledge protocols behind. We could say that he exchanged knowledge for action. For obviously politics and knowledge are opposed. Louis Althusser reads matters otherwise: 'The theoretical revolution announced in thesis XI is in reality the foundation of a new science'.<sup>52</sup> Althusser's lead encourages me in my suspicion that actually, that famous eleventh thesis needs to be read in the light of the second, which holds—a far more profound formulation—that 'Man must prove the truth, i.e. the reality and the power, the this-sidedness of his thinking in practice'.<sup>53</sup> In other words, our thinking must involve intervening in the world in such a way that reality itself evidences its proof and power. Which is basically what Lenin did in Russia. 'For me', he is reported to have told a visitor, just after the revolution, 'theory is only a hypothesis, not the Holy Scripture; it is a tool in our daily work'.<sup>54</sup> Of course, any physical scientist would say the same. Such a forward-thinking orientation is reflected in Althusser's thesis, in 'Lenin and Philosophy', that 'science is the real itself, known by the action which reveals it'.<sup>55</sup>

Science, in such formulations, is less a corpus of stable knowledge, than a set of interventionist practices whose validity is known, indeed proven, by their effects. This has all sorts of implications for how we understand not merely Garfinkel's, but Althusser's work too. A scientific act of teaching, to translate one such implication, is one that takes aim and manages to convert an entire generation to thinking through its own agenda. That would be 'the reality and power', the scientific status, of Althusser's teaching and collegial work with Alain Badiou, Etienne Balibar, Jacques Derrida, Michel Foucault, Pierre Macherey, Jacques Alain Miller, Jean Claude Milner and others, work whose effects we still feel now in the contemporary Australian university. If such formulations of science seem extreme in their pragmatism and worldliness, it is worth remembering that Peirce, who was no revolutionary, would understand scientific investigation under the same pragmatic rubric: as an

interventionist practice that is judged by its effects on the world. Of course, one might want to consider just how far to extend this definition of 'science' (again, keeping in mind Bailyn's caveat against metaphysicalising concepts such as 'creativity', 'rules', 'science' and 'society') and which histories one will attempt to deactivate in the process. But compare the opposite, the diminishing of science evident in comments such as those of Forgacs, who argues that the English reception of Gramsci was long delayed, for Gramsci only ever appeared in the form 'sanitised by Althusserian scientism'.<sup>56</sup> Isn't it rather science itself that is sanitised in such formulations, which fail to see just what a political power a scientific attitude has the potential to be? But then, as Althusser put it, 'Lenin is *intolerable* to academic philosophy' and perhaps always will be.<sup>57</sup> And this is not without reason. Empiricism is not only, to repeat Holton's words, 'regarded by some as dangerous'. In certain hands, it is dangerous!

Isn't that why Garfinkel himself seems so sanitised in critiques of his work, which focus on an abstracting account of his 'ideas' and simply ignore the to-the-letter account of their practice? He's too full on. That's why ethnomethodology fell into disrepute and dismissal, to the point where Lynch finds himself, in an article aimed at celebrating the status of *Human Studies* as 'Ethnomethodology's Unofficial Journal', compelled to spend most of his editorial defending the discipline from critiques such as Giddens's, or dismissals in Nicholas Abercrombie, Stephen Hill and Brian S. Turner's *Dictionary of Sociology*, which gives the fact that ethnomethodology 'deals with trivial subjects' as the first in the reasons for its decline.<sup>58</sup> Is it really the *triviality* (of, for example, observing and exegiting police interrogation practices) that brought ethnomethodology into disrepute? Isn't the real reason that this sort of inquiry is not trivial enough? It's way too full on! This sort of stuff is dangerous. People might get ideas. They might start experimenting on our lives too. We have no idea what the future is. *Act the idea first, theorise it later.*

I am suggesting that what was truly radical in ethnomethodology was its scientific method. You are not meant to act scientifically when it comes to the sort of subjects Garfinkel treated. His approach was (according to unwritten institutional rules, but whose? and do they really hold?) simply wrong, a misreading of science. To be scientific in such instances *is* to be creative. Q: So how do you institutionalise creativity? A: You start by realising that actually it didn't disappear. There in the text of Frow's own response to my paper, recalling and reading it now to the letter, was a description of precisely what I have claimed Garfinkel's critics try to make disappear: this crazy empiricism, these radical examples.

#### — THE UNCONSCIOUS INSTITUTE

In conclusion, Garfinkel stresses that there is no corrective dimension to any of his researches. He is *not* trying to show people ways in which they could make their following of rules

and instructions easier by eradicating all the ‘monsters’. For the fact of the matter is that if you excluded such strategies, ‘this would leave you without an enterprise’.<sup>59</sup> He simply offers a picture of how rules and regulations function for the people who follow them. He’s simply being scientific. I am attempting a similar project here, in tracing the evolution of a set of ideas from the giving of a paper, to the responses it garners, and on to the new ideas these lead to. That’s my way into the theory of ideology. Of course, the critique of triviality could still apply to this very essay, just as to Garfinkel’s works, just as to cultural studies in general. For none of these projects seem to concern the great questions of power: nation, state, capital. Nor do cultural studies of Kylie Minogue fans (of which I am one). Now let me answer John Frow’s question: how do you institutionalise creativity, something inherently anti-institutional? How do you avoid just doing another Garfinkel, and fading into obscurity? How can you maintain, and make standard, the sort of newness I wanted to champion in that earlier paper, ‘Kierkegaard: The Movie’?

My breakthrough with Garfinkel—about a year or so after ‘Kierkegaard: The Movie’, percolating through Amanda’s and Stephen’s comments, resonating with John’s critique, and then bouncing off an accidentally discovered and out-of-date edition of one of Garfinkel’s own essays—was my realisation that Garfinkel may well have failed to institute ethnomethodology within the academy, but there I was reading about it. There was John Frow telling me about it, passing on the example. Even my canonical *Dictionary of Sociology*, which lists all the problems with and critiques of Garfinkel, still couldn’t help passing on the wonderful lodger experiment. I started to realise, after a year of thinking intermittently about Garfinkel, that actually, he had managed to form an institution. We’d all ‘let it pass’. Who’s to say that that is not an institution, something that stands through the passage of time? After all, you’re reading about it now. It’s just been republished. Officially. Just as it was when Frow spoke about it in public to forty people in Hobart, others of whom might have gone and chased it up too. It bribes its way into publication through us. Officially. As a favour perhaps. And it could become new. Every time.

As such, Garfinkel’s work constitutes an institution, a *discontinuous institute*, whose existence, through repeated retellings, serves to stage by providing an example for intellectual creativity.

A: You institutionalise creativity first by realising that it’s happening or could happen all around you, it’s just a matter of focusing. Realising the relative freedom of living in the future, which does not exist, you promote those realities over others. You translate them. You do it by asking good questions—and here I assume the existence of spaces open enough to allow this, something which is by no means guaranteed and has to be fought for.



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1. Harold Garfinkel, *Studies in Ethnomethodology*, Prentice Hall, New Jersey, 1968.
2. Jacques Derrida, *Of Grammatology*, trans. Gayatri Chakravorty Spivak, Johns Hopkins University Press, Baltimore and London, 1976; Soren Kierkegaard, *Repetition: An Essay in Experimental Psychology*, attr. Constantine Constantius and others, trans. Walter Lowrie, Princeton University Press, Princeton, 1943.
3. Roman Jakobson and Krystyna Pomorska, *Dialogues*, trans. Christian Hubert, Cambridge University Press, Cambridge, 1983, p. 137.
4. Paul Magee, *From Here to Tierra del Fuego*, University of Illinois Press, Champaign, 2000.
5. Roman Jakobson, 'A Few Remarks on Peirce', in Stephen Rudy (ed.) *Selected Writings, Vol. 7: Contributions to Comparative Mythology, Studies in Linguistics and Philosophy, 1972–82*, Mouton, Berlin, 1985, pp. 248–53.
6. Charles S. Peirce, 'The Essentials of Pragmatism', *Philosophical Writings*, Dover, London, 1955, p. 261.
7. Jakobson, in Jakobson and Pomorska, p. 92.
8. Jakobson, in Jakobson and Pomorska, p. 92.
9. Paul Magee, 'Kierkegaard: The Movie', unpublished paper, *What's Left of Theory*, Cultural Studies Association of Australia Annual Conference, University of Tasmania, Hobart, 4 December 2001.
10. Harold Garfinkel, 'Practices for Following Rules and Applying Instructions', in Peter Worseley (ed.), *Modern Sociology Reader*, Penguin, London, 1970, pp. 658–65.
11. Garfinkel, 'Practices for Following Rules', p. 659.
12. Garfinkel, 'Practices for Following Rules', p. 658.
13. Garfinkel, 'Practices for Following Rules', p. 659.
14. Garfinkel, 'Practices for Following Rules', p. 660.
15. Garfinkel, *Studies in Ethnomethodology*, p. vii.
16. Garfinkel, 'Passing and the Managed Achievement of Sex Status in an Intersex Person, (Part 1)', *Studies in Ethnomethodology*, pp. 116–86.
17. Michael Lynch, 'Ethnomethodology's Unofficial Journal', *Human Studies*, vol. 25, no. 4, 2002, p. 486.
18. Lynch, p. 487.
19. Lynch, p. 487.
20. Sarah Bailyn, 'Who Makes the Rules? Using Wittgenstein in Social Theory', *Journal for the Theory of Social Behaviour*, vol. 32, no. 3, 2002, pp. 311–29.
21. Garfinkel, *Studies in Ethnomethodology*, p. 66; Bailyn, p. 316.
22. I realise this is an unusual and anachronistic phrasing, but the latter Wittgenstein does it better than anyone else.
23. Bailyn, p. 317.
24. Bailyn, p. 317.
25. Bailyn, p. 327.
26. Garfinkel, *Studies in Ethnomethodology*, p. 44.
27. Garfinkel, *Studies in Ethnomethodology*, p. 41.
28. Roman Jakobson, 'On a Generation that Squandered its Poets', in Krystyna Pomorska and Stephen Rudy (eds), *Verbal Art, Verbal Sign, Verbal Time*, Minnesota University Press, Minneapolis, 1985, pp. 111–33.
29. Vladimir Mayakovsky, cited in Roman Jakobson, 'On a Generation,' p. 125.
30. Vladimir Mayakovsky, in Jakobson, 'On a Generation', p. 125; Jakobson, 'On a Generation', p. 132.
31. Anthony Giddens, *New Rules of Sociological Method: A Positive Critique of Sociological Method*, second edition, Polity Press, Cambridge, 1993.
32. This is not, mind, to dismiss the force of Bailyn's critique of Giddens's metaphysical totalisation of the concept of 'rules', which seems entirely valid, p. 326.
33. Giddens, p. 46.
34. Jacques Lacan, *The Ethics of Psychoanalysis, 1959–60: The Seminar of Jacques Lacan, Book VII, 1959–60*, ed. J.-A. Miller, trans. with notes Dennis Potter, Norton, New York, 1992, p. 69.
35. Giddens, p. 47.
36. Giddens, p. 49.
37. Harold Garfinkel, Michael Lynch and Eric Livingston, 'The Work of a Discovering Science Constructed with Materials from the Optically Discovered Pulsar', *Philosophy of the Social Sciences*, vol. 11, no. 2, 1981, pp. 131–58.
38. Garfinkel, Lynch and Livingston, p. 132.
39. G. Holton, 'Comments on Professor Harold Garfinkel's Paper', *Philosophy of the Social Sciences*, vol. 11, no. 2, 1981, p. 159.
40. Lynch, p. 489.
41. Giddens, p. 47.
42. For example, see Nicholas Abercrombie, Stephen Hill and Bryan Turner, *Dictionary of Sociology*, Penguin, London, 1984, pp. 83–4.
43. Garfinkel, *Studies in Ethnomethodology*, pp. 11–18.

44. Walter Benjamin, 'Theses on the Philosophy of History', *Illuminations*, Fontana, London, 1973, pp. 245–57.
45. Garfinkel, cited in Giddens, p. 48.
46. Garfinkel, *Studies in Ethnomethodology*, p. xi.
47. Giddens, p. 41.
48. Giddens, p. 45.
49. 'Does Cultural Studies Have a Canon?', <<http://www.staff.vu.edu.au/CSAA/canon.html>>.
50. Lynch, p. 490.
51. Karl Marx, 'Theses on Feuerbach', in Christopher John Arther (ed.), *The German Ideology, Part One, With Selections from Parts Two and Three, and Supplementary Texts*, trans. unknown, International Publishers, New York, 1970, p. 123.
52. Louis Althusser, 'Lenin and Philosophy', *Lenin and Philosophy and Other Essays*, trans. Ben Brewster, Monthly Review Press, New York, 1971, p. 38.
53. Marx, p. 121.
54. Vladimir Ilich Lenin, in Ronald W. Clark, *Lenin: The Man Behind the Mask*, Faber and Faber, London and Boston, 1988, p. 149.
55. Althusser, p. 38.
56. David Forgacs, 'National-Popular, Genealogy of a Concept', in Simon During (ed.), *The Cultural Studies Reader*, second edition, Routledge, London, 2001, p. 210.
57. Althusser, p. 31.
58. Abercrombie, Hill and Turner, p. 83.
59. Garfinkel, 'Practices for Following Rules', p. 662.