MAKING, BREAKING AND FOLLOWING RULES: THE IRVINE CASE

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ABSTRACT

James March's highly influential article on organisational learning underpins the studies of exploration and exploitation collected in this issue. What is less well known is that March's article, which is based on a computer simulation of collective and individual learning, reflects a real-life experiment in exploration and exploitation that he, in large part, designed and conducted when he was the new 'boy Dean' of the School of Social Sciences in the University of California at Irvine between 1964 and 1969. This paper tells this story and then uses it to critique March's original model. It argues that March's model, which was probably the first simulation of an organisation learning, worked to constitute rather than model the phenomenon of organisational learning. The Irvine story is also important because it provides the context for what constitutes knowledge in organisation theory, and because it highlights the personal trauma and distress that can accompany the creative play of exploration.

Keywords: Exploration; exploitation; organisational learning; James March; ethnomethodology; organised anarchy

Managing 'Human Resources' by Exploiting and Exploring People's Potentials
Research in the Sociology of Organizations, Volume 37, 27–54

ISSN: 0733-558X/doi:10.1108/S0733-558X(2013)0000037005

INTRODUCTION

In 1991, *Organization Science* published a seminal contribution to management and organisation studies, namely James March's article on 'Exploration and Exploitation in Organizational Learning' (March, 1991, hereinafter E&E). This paper provided the basis for a major stream of research into, *inter alia*, the twin phenomena of exploration and exploitation. What is largely forgotten is that March's paper, which is based on a computer simulation of collective and individual learning, reflects a real-life experiment in exploration and exploitation that he, in large part, designed and conducted when he was the new 'boy Dean' of the School of Social Sciences in the University of California at Irvine between 1964 and 1969. The story of the School of Social Sciences (hereafter SSS) is a particularly good case of the difficulties associated with fostering an ethos of exploration within a context where exploitation is also privileged. It is an unusual attempt to escape from, subvert and yet work within institutional powers, rules and structures. It is also a case study of creativity, work, power and play that might illuminate similar experiments. And because the story is recounted largely by the individuals involved, it highlights what ambidexterity can mean at the personal level.

But this story is more than just another case of ambidexterity, exploration and exploitation. Not least, it is a story about James March, who is such an influential figure in management and organisation studies, and the context in which the ideas associated with him emerged. If we accept that knowledge is situated (Lave & Wenger, 1991), then context is implicated in theory and theory development, which means that important theoretical contributions to the field, such as March's distinction between exploration and exploitation and his seminal paper on organisational learning, need to be understood contextually. And the context for the experiment is fascinating. The experiment occurred in southern California during the 1960s, a period of intense social, political, technological and cultural upheaval. Understandings of social science were also in flux at the time, and SSS was a significant node in the development of new and influential streams of research, such as situated learning, ethnomethodology and conversation analysis (as well as the mathematical modelling paradigm). It was also an important battle-site where advocates of different epistemological positions confronted (and avoided) one another.

The story is also an interesting study of the interplay between the practice of organising and models of organising, since March was both a practicing manager/leader in SSS and an influential theorist of organisation. Social

phenomena are inherently reflexive – since knowledge of social phenomena always works to constitute particular social phenomena – but this reflexive dimension must have been heightened in SSS where models and practices of organising were being co-produced dialectically. This interplay between the practice and the model of organisation is of particular interest because there are remarkable parallels between the SSS story and the model of organisation on which the E&E paper is based. In brief, E&E is examines the relationship between exploration and exploitation through a computer program simulating 50 random individuals coming to a collective and shared understanding of reality. SSS is not a model but a real story about fifty individuals being put together as a group that individually and collectively engaged in exploration and exploitation and out of which emerged some shared understandings of reality.

The paper begins by summarising the E&E paper and its influence. It then presents a description of the SSS experiment, before proceeding to reflect on how the two experiments inform each other, and, in particular, how the real-life experiment provides a basis for critiquing and re-evaluating E&E, and for deepening our knowledge of exploration and exploitation.

E&E

March's 'Exploration & Exploitation' article is one of the most influential in the management canon. It is now the seventh most highly cited article in the field (based on a Google Scholar search in December 2010) and its annual citation count is increasing year-on-year (Fig. 1). Central to the article is a distinction between *exploration*, which 'includes things captured by terms such as search, variation, risk taking, experimentation, play, flexibility, discovery, innovation' (E&E, p. 71) and *exploitation* which 'includes such things as refinement, choice, production, efficiency, selection, implementation, execution' (*ibid.*). March begins his paper by pointing out that managers face a crucial trade-off between the high-risk exploration of new possibilities and the low-risk exploitation of existing certainties, a trade-off that might be explicit, when new investments are being evaluated, or implicit in organisational forms and norms.

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March then simulates this tension using a simple, but ingenious computer program (written in BASIC), which is presented in schematic form in Fig. 2.[1]

First, there is an external reality that is independent of individual beliefs. This 'reality' has m dimensions, each with value of 1 or -1 with independent

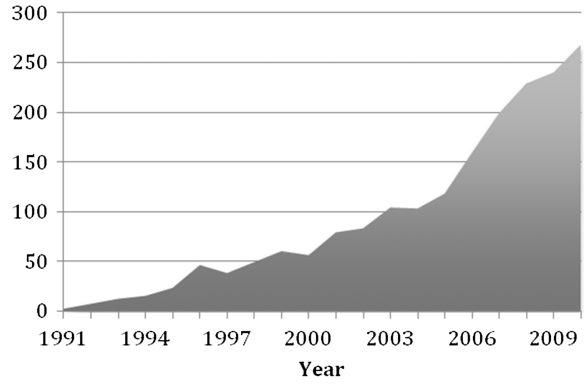

Fig. 1. Annual Count of Citations for March (1991) in the Web of Science.

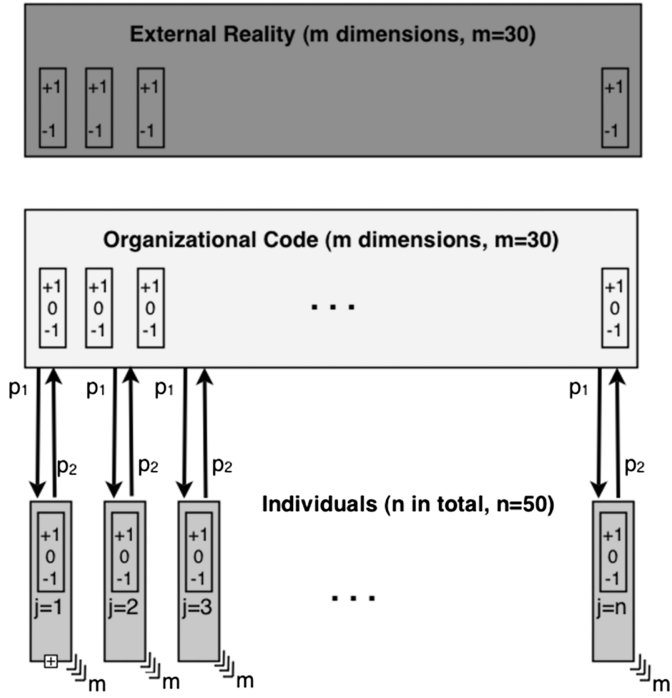

Fig. 2. March's Model in Schematic (after Bray & Prietula, 2007a).

probability of 0.5. Second, there is a group of n individuals each of whom has a belief (which can be -1, 0 or 1) about each reality dimension. In his model, March fixed m as 30 and n as 50, though the results seem to replicate for all m and n. Third, there is an 'organisational code' which represents the group's collective understanding of each reality dimension.[2] The initial conditions are that individuals have no knowledge (each belief in the set of n m-tuples is set randomly as -1, 0 or $+1$) and the organisational code is neutral (each element of the m-tuple is set at 0).

In each period the following changes may occur. If an individual's belief differs from the organisational code (the collective belief) then the individual will change his/her belief with probability p_1 (the effectiveness of socialisation). Crudely, p_1 represents *exploitation* as a probability that an organisation will influence an individual's knowledge (high p_1 means that collective norms, routines, etc. are strong). The organisational code can also change based on the beliefs of experts, namely 'those individuals whose beliefs correspond with reality on more dimensions than does the code' (E&E, p. 74). The probability that the beliefs of the code will be adjusted depends on the level of agreement between experts and on a parameter, p_2, that reflects the effectiveness of learning *by* the code. Again, p_2 crudely represents *exploration* as a probability that the collective will alter its presumed view of reality through learning from experts. Neither the code nor the individuals can observe reality directly, but the model does include data on the knowledge level of individuals and the code (the percentage of dimensions that an individual or the code hold as correct). These knowledge levels change as the organisational code adapts to the knowledge of the experts and as individuals conform to the knowledge of the organisational code. The organisational code can identify the experts, based on their overall individual knowledge levels, but it cannot identify which specific beliefs are true or false. An important insight suggested by the model is that a group can learn even in situations where individuals in isolation cannot (though this is a design feature of the model).

As successive iterations are performed, the knowledge level of the code and the individuals (the degree to which they match external reality) tend to converge until a stable knowledge equilibrium is achieved (which normally differs from reality). March examined the level of equilibrium knowledge in different scenarios of p_1 and p_2, and the speed it takes to converge to this equilibrium level. He found that equilibrium is reached earlier if there are higher rates of learning, and also that higher equilibrium levels were associated with *lower* individual learning rates (in other words, so-called 'slow learners' can mitigate the deleterious effect of groupthink).

In his paper, March extends this basic model to incorporate *personnel turnover* (p_3 being the probability that an individual may leave the organisation to be replaced by another individual who will have randomly distributed beliefs) and *environmental turbulence* (p_4 being the probability that a dimension of external reality will flip). Others have also extended and modified his model. For instance, Bray and Prietula (2007b) modelled a hierarchical structure within the group, Rodan (2008), Kane and Alavi (2007) and Miller, Meng, and Calantone (2006) incorporated interpersonal learning rather than learning through a collective code, while Rodan (2005) incorporated a number of different real-world organisational features. These, and others, have extended March's insights, but his original paper remains probably *the* critical contribution to the literature on organisational learning.

Notwithstanding the number of citations to March's paper, there have been relatively few substantive engagements with the details and assumptions of his model. Instead, the literature has focused on the tension between exploration and exploitation that he so elegantly articulated, on the possibility of 'organisational ambidexterity' (the ability to pursue exploration and exploitation simultaneously), and on the importance of 'slow learners' to counter the phenomenon of groupthink. In particular, there have been few, if any studies, that systematically contrast March's computer model of organisational learning with what happens in reality. The SSS experiment provides such a context for comparison and it is to this that we now turn.

THE SSS EXPERIMENT

The lost part of the E&E story is centred on a real-life organisational experiment that bears an uncanny resemblance to March's computer simulation. This section of the paper outlines the story of this experiment, which took place in the School of Social Sciences at the University of California-Irvine from 1964 to 1969. It is based on extensive interviews with many of those involved at the time or soon afterwards, including James March, Jean Lave, Duncan Luce, Arnie Binder, William Schonfeld, Mike Cole, Dean Neubauer, William Sharpe, Charles Lave, Julian Feldman, Michael Cohen, Kim Romney and John Payne. In total, some fifteen hours of interviews were conducted, taped and analysed. The research also draws on the descriptions and analysis of Johan Olsen (1970, 1976/1979), who was a visiting scholar in SSS at the time. In addition, the research draws on

correspondence with other individuals, and a detailed analysis of secondary and archival material. The factual accuracy of the story was validated by reverting to interviewees with my version of the narrative.

Since the story echoes key elements of the computer model, it provides a useful basis for examining the model and its assumptions. In addition, it is an interesting case study of exploration and exploitation in its own right. But it is also more than a case study. Much of the SSS story recounts a debate about the degree to which history and context matter in the construction of social scientific knowledge. The side of the debate that I favour holds that history and context should not be dismissed as mere description, but are instead constitutive of what we understand as social scientific knowledge. From this perspective, the SSS story is a vital part of the context out of which particular knowledge – such as theoretical understandings of organisational learning – emerged and is properly understood.

From 1954 to 1964, March was a leading member of an exceptional group of scholars based in the Graduate School of Administration (GSIA) at the Carnegie Institute of Technology. The history and influence of GSIA, which was an extraordinary hotbed of ideas and research, is well known and documented (Augier & March, 2001, 2002, 2004; Augier & Prietula, 2007; Crowther-Heyck, 2005, 2006; Gavetti, Levinthal, & Ocasio, 2007; Gleeson & Schlossman, 1995; Hosseini, 2003; March, 2007; Tadajewski, 2009). Six of the GSIA group, which fluctuated in number from 30 in 1955 to about 50 in 1964, received Nobel Prizes, while ten were elected to the US Academy of Sciences. The group, led by Lee Bach and Herbert Simon, emphasised inter-disciplinary research, deductive reasoning and mathematical modelling, commitments that provided much of the intellectual basis for the contemporary business school and for the SSS experiment.

In 1963, March (then aged 36) was invited to be the first Dean of the Social Science Division (in 1964 the university was divided into 'schools' rather than 'divisions') in a new campus that the University of California was building in Irvine, Southern California. At that time, the fledgling campus had neither staff nor students. In November 1963, March set out his vision for the Division in a letter he wrote to Ivan Hinderaker, Vice Chancellor of Academic Affairs. In this letter, he stated that the Division 'should be conspicuously experimental and innovative' with the burden of proof 'shifted to the existing system. I think there should be major innovations with respect to curricula in the social sciences, instructional methods, academic organisation, and staffing policies. The social science division should be viewed as an experimental laboratory rather than as

primarily a production facility'. His second point was that specialisation should be by problem areas rather than by traditional academic disciplines and that 'faculty should [have] substantial disrespect for traditional disciplinary identifications'. The third dimension of his plan was that the division should become a 'leader in the application of modern techniques for empirical investigation and theory building', which meant that the 'social sciences should be heavily laced with mathematics, statistics and computer methodology'. Finally, March was of the view that the division has to take some risks: 'There is no serious possibility of becoming a major institution with a conservative strategy'.

March's E&E model simulated a group of 50 individuals with random beliefs coming together and learning collectively and individually until such time as they shared common beliefs (not necessarily true) about the world. In other words, it simulated an organisation coming to be. In essence, the Irvine 'experiment' provided March with a similar opportunity to build and observe an organisation coming to be. For Mike Cole it was 'an experiment … .March was just letting it happen, to see what would happen'. William Schonfeld, who joined in 1970, concurred: 'It was as though you were doing a real-life experiment with human beings', and indeed the Course Catalogue (1969–1970) makes this clear: 'Undergraduate and graduate education in the School of Social Sciences at UCI involve participation in an experiment. The program, faculty and students differ substantially from conventional counterparts elsewhere' (p. 115). Dean Neubauer also saw it as an experiment:

> Irvine as a pedagogy was an experimental project; there's no doubt about that, and it was intoxicating … We used to call it, some of us, 'Jim's experimental universe'. We would tease him sometimes at dinner parties, 'How did you manage to get the University of California to give you this real-life laboratory?'

Elsewhere, Bill Sharpe 'characterised it to others as, "It was a really interesting experiment, but unfortunately all the rats died!"'

Even though March is obviously a central figure in the story, characterising SSS as 'Jim's experimental universe' overly personalises the story, not least because SSS drew on and reflected wider themes of the times. In particular, the post-war era was very much the 'golden age' of inter-disciplinarity (House, 1977; Sewell, 1989), with the SSS experiment following in the tradition of similar inter-disciplinary academic experiments such as GSIA (1950–1964), Yale's Institute of Human Relations (1929–1950), and Harvard's Department of Social Relations (1946–1966). Furthermore, the 'experiment' could only have taken place if there was a

general enthusiasm for such an endeavour. More broadly, what some of the informants identified as idiosyncratic individual behaviour may be better understood as manifestations of wider social phenomenon. For instance, Michael Cohen observed that March was especially interested in

> scrambling conventional organizational arrangements, to get people thinking in new directions. There were even some experiments done, where they shuffled the labels on everyone's mailboxes so that everyone was getting everyone else's mail. He was very interested in that period in what happens when the conventional patterns of behaviour get disrupted.

While such games exemplified March's interest in the positive aspects of play and foolishness (March, 1976/1979), they are probably better understood as examples of the 'breaching experiments' associated with the ethomethodological perspective that was emerging around that time, centred on individuals who joined March's group in Irvine, or UCLA, or both. Likewise, other parts of the SSS experiment, which informants sometimes attributed to March, may be better understood as local manifestations of broader movements that drew on, for instance, emerging ideas in cybernetics about self-organisation (Wiener, 1948/1961), or McCulloch's (1965) 'principle of redundancy of potential command' (which posits that command should pass to the region with the most important information), or influential critiques of society and formal organisation, such Marcuse's (1956) *Eros and Civilization*, Goodman's (1960) *Growing Up Absurd*, Mills' (1957) *The Power Elite* or Galbraith's (1958) *The Affluent Society*.

Interviewed in 1973 by Sam McCulloch, March observed that 'I had the instincts of having studied organisations, it would be fun to build one, the instincts of a missionary with respect to social science'. And build it he did. In 1964 he made his first appointment: his former graduate student, Julian Feldman (34) who had just published, with Edward Feigenbaum, the first collection of articles about artificial intelligence. Consistent with his philosophy of being innovative, distinctive and separate, March resisted using disciplinary titles for programmes or positions: 'And so we created these fancy titles. I had something like 'Associate Professor of Psychology and Economics or something like that. He [March] was a Professor of Political Science and Sociology.[3] Between the two of us we were covering four disciplines' (J. Feldman). Together they hired 15 faculty members during that first year, with the first group of 287 students arriving in the fall of 1965. In 1966 eleven more staff were hired and a further 375 students registered. Almost all of the faculty were young. Overall, some 45 staff were

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hired between 1965 and 1968 (E&E simulated a group of 50). In practice, virtually all the new hires were junior people: 'There were a lot of young junior people, energetic…' (the average age of the faculty in 1966 was just 26, with five women in the faculty of 30, which was unheard of at that time). Here's March's retrospective take on these hirings:

> mostly our basic strategy was to hire young people and to try to be ahead of the market and to take risks – to hire people who had a distinctive interest in playing with ideas; hard to tell, we didn't have very good testing devices for that, so basically we said we're going to run a strategy in which we'll have more failures that successes, but our successes will look pretty good.
>
> DK: It was a strategy of letting many flowers bloom?
>
> JM: Many flowers bloom, but we tried to have standards – flowers that don't bloom well, you weed out. But I think we were a little less successful in doing that. That was the strategy at least.

The hiring criterion was relatively straightforward, but unusual: "'Was this person interesting?" And I think clearly the dominant story was "we don't care whether you have the same kind of ideas as we have, as long as we can find your ideas interesting"' (J. March). Almost inevitably this eclecticism created a very high level of variety:

> As it developed, it turned out to be a number of people who I would now describe as social constructivists, [and] a number of people who turned out to be relatively pure mathematical modellers. There were some people who became committed ethnographers, and you are talking about a range – someone like Bill Sharpe [who subsequently won a Nobel Prize] at one end was creative and a little bit different, but a financial economist, and then you have people like Duane Metzger and Jean Lave at the other end and who were fairly creative, constructivists, postmodern anthropologists.[4] [J. March, interviewed in 2009]

According to Kim Romney, who joined in 1968, the variety that emerged was intentional: 'I don't know whether he made it explicit or not, but I think that he also wanted variety. High variance. You wouldn't have people coming out of the same mould'. William Schonfeld concurred: 'They did believe a lot in high variants; that's why you had a number of faculty who were quite gifted and a number of faculty who were not. They were looking for anybody who was different'. This desire for variety is an important part of the story, because high variety is an initial condition in the E&E simulation.

Another feature that SSS shares, at least to some extent, with E&E is that the group was initially undifferentiated. In his 1963 letter to Hinderaker,

March highlighted what he saw as the problem with the usual model of academic organisations.

> Academic organizations ordinarily combine inflexible central control with irrelevant local initiative. First, the structure is usually exceptionally rigid. The departments are substantially unchanging over time; they are the same from one university to the another. As a result, subunits tend to become inviolate, individual faculty members tend to be linked with a specific subunit in perpetuity, and the university as a whole becomes a loose alliance of migratory workers. Second, typical academic organization overuses 'legislative' techniques for decision making; it underuses staffwork, consultation and executive decision making.

In implementing an alternative, de-differentiated mode of organising, 'March had set it up so that there was far more equality among faculty, students and staff than in most universities' (K. Romney). He and others clearly emphasised socialisation among the faculty. In particular, the (non-academic) staff played a much more important role than was the norm in other universities, with some informants observing how 'unique' (K. Romney) it was to have staff, students and faculty socialising together. In the same vein, they had a policy that each member of faculty would teach one course a year with a colleague in a discipline the faculty member knew nothing about (Lave, 2009).

The eschewal of formal structure did give the appearance of chaos. According to William Schonfeld, who only arrived in 1970 after March had left, 'the only rule was, there are no rules'. Mike Cole, who arrived in 1967, says something similar: 'There were no rules, and it was as close to a blank slate in an institution as you're ever going to find'. In his interview, Arnie Binder emphasised the notion of disorganisation:

> 'disorganization' was a word that was used permanently … it meant that we were certainly never going to have departments here, above all; that we're never going to assign offices according to discipline. So you have to have the psychologists here and anthropologists in the next office, and so forth, and the interactions had to be so that there were no organizations by disciplinary focus above all … if they moved in a direction of what some would call 'responsible organization', he [March] would oppose it. [A Binder]

Mike Cole recalls that

> part of the disorganization was that it seemed like, you know what you're against but you don't know what you're for. Literally we would go in on Saturday to see what the hell we were going to do on Monday. And we would do that quite regularly.

Allied to the organisational decision not to have departments, March also worked to ensure that the physical environment supported this de-differentiation, as Dean Neubauer explains:

> The ways the physical environment affects the professional and psychological environment of the faculty was tremendously important. Jim had the great insight of mixing people up and he kept mixing them up. The person in the office next to you was not in the same discipline as you. This is sometimes called the water-cooler theory of organisation and was enormously important.

While de-differentiation might have been March's aspiration, his own presence as organisational designer and father-figure necessarily subverted that possibility. As he put it himself, 'I think some people would describe it as a benevolent despotcy [sic]!'

Structure Emerges and Disappears

March's 'experiment' was to put about 50 young academics together and then see how they might organise without replicating existing structures, or as Mike Cole recalls it: 'we created this rule that you cannot create an academic unit which was identifiable with an existing discipline [like sociology, anthropology or economics]'. Out of this mix, three groupings emerged, which, in 1967, came to be named as Program A, Program B and Program C. Each group's focus was deliberately vague, leaving space for the participants to decide the group's direction and orientation. And the divisions between the groups was never fixed; rather it was quite a fluid structure and the programs divided, recombined and took different names and characteristics over the years. Often there were more than three programs. AU :5

Program A, sometimes referred to as 'Formal Models' or, more officially, the 'Program of Mathematical and Computer Models in the Behavioral Sciences', followed through on the GSIA work, and the strict and unusual maths and computer programming requirements placed on the students was very much in harmony with this group's philosophy.[5] Arnie Binder, who helped form the group, recalls that 'March was never enthusiastic about that program because of its organisational implications, but eventually approved it, perhaps because of its mathematical emphasis and its distinct interdisciplinary nature'. Two-sector growth models, which were especially popular in the early 1960s (see Hahn (1965) for a useful review), were typical

of the type of research conducted by members of Program A. Similarly, March's E&E paper is very much in the tradition of Program A.

Program B was sometimes known as 'Language and Development' but its focus was also on culture and society. This group included what would be recognised as anthropologists, sociologists and social psychologists. It sought 'to provide sufficient understanding of complex cultural phenomena to produce significant cultural change [and to develop an] understanding of individual and small group behavior, as well as national, macro-level behavior' (March's report to Chancellor Aldrich, November 1968). While it avoided the sophisticated mathematical modelling of Program A, it still contained the 'the most scientistic, statistically oriented group of anthropologists in the country' (J. Lave). In retrospect, the best-known Program B academics were Jean Lave, who became well known for her work on situated learning (Lave & Wenger, 1991), Mike Cole whose research focused on cognitive development and cultural psychology, and the anthropologists Duane Metzger and Kim Romney.

Program C was 'a residual category for those faculty members in the Division who are not members of either program but hold appointments in the Division' (memo from March to Aldrich, June 1967). The records indicate that March himself was in neither Program A or B and so, by default, he was in Program C. For Jean Lave it was 'the set of those not belonging to any set' (Lave, 2009) and, as such, was a foil against attempts to reify structure. This is well articulated by Deane Neubauer who saw the A/B/C structure as

> the minimum structure allowable for people who needed structure by an administrative group which didn't want even that much structure … It served two functions simultaneously, on the one hand there's something here, so you have an answer when people ask you what you're doing, but on the other hand the differences between A, B and C were more apparent than real.

For some, the emerging structure was unimportant – Bill Sharpe didn't recall the A–B–C groupings at all – while for others, such as Jean Lave, it was more central, not least because it distinguished her and her Program B colleagues from the mathematical social scientists and their endeavours. For Dean Neubauer, the key difference between A, B and C was 'the degree to which you could insist on mathematical sophistication and the willingness to allow others to fake it'.

While the SSS group divided into sub-groups distinguished by their relative commitment to mathematical modelling, almost all members still shared a common ahistorical approach to inquiry. For instance, Duane

Metzger and Volney Steffler, both affiliated with Program B, championed the 'principles that all talk across disciplines must be in words of one syllable, or at least directly intelligible. No fair retreating into citations of scholarship in your own discipline. No appeals to historical contexts of contemporary work – that would be evasion' (Lave, 2009, p. 6). Those that took a contrary position – such as Inga Bell, who was a historical materialist social theorist – tended to leave. Ironically, while SSS was committed to mathematics as a universal language – with its ahistorical, abstract and decontextualised epistemology – some of Lave's subsequent work highlighted the situated, context-embedded nature of mathematical practice (Lave, 1988; Lave, Murtaugh, & de la Rocha, 1984). She now notes the irony of being 'a delighted, enthusiastic participant in its collective search for interdisciplinary unity via a mathematical language, empirical modelling and anti-historical, anti-social-theoretical stance, and [yet] end up today working within a historical, materialist theoretical problematic' (Lave, 2009)

One important group that existed outside the A–B–C structure was the ethnomethodolgists. The group was centred around Harvey Sacks, a phenomenological sociologist who joined in 1968, and included David Sudnow (who joined in 1967) and Gail Jefferson (who completed her PhD in Irvine in 1972). Sacks never subscribed to the idea of mathematical and ahistorical inter-disciplinary activity but his work was admired – if not necessarily comprehended – by many of the mathematical modellers because of its highly detailed, fine-grained empirical analysis, which, according to Jean Lave, they mistook as common theoretical territory. Sacks' group also included Harold Garfinkel and Emanuel Schegloff who moved back and forth between UCLA and Irvine as indeed did Harvey Sacks.[6]

If this loose structure was emerging, tensions were also developing, which culminated in fragmentation. In many ways, context drove this fragmentation. As March observed in 2010, the time and place were

> infused with talk of revolution, ethnic conflict, organic gardening, women's liberation, free spirits, and recreational drugs. I think that most people were substantially more concerned with such things than they were with the organization of the School (which was to a certain extent a side-show for the confusions, anxieties, and enthusiasms of the times).

One local manifestation of these anxieties was that those not in Program A increasingly argued that mathematical modelling, while intellectually elegant, did not engage sufficiently with the social and environmental problems of the time. By 1968, Arnie Binder had proposed a 'Program in Social Analysis' and March included this in his report of that year to

Aldrich. This program, which March presented as just a proposal, 'centers on the very problems which society presents for solution. e.g. 1. urban development, etc. 2. community mental health; 3. riots and unrest; 4. injury control in home and highways'. However, 'within 2 weeks he [March] withdrew [his support] and said he had just made a mistake in giving initial support' (Arnie Binder). Binder was clearly disaffected and worked to split off from the School of Social Sciences a new independent unit/program. This he eventually set up in 1970 after March had left Irvine. The program in Social Ecology grew over the years, and by 1981 it had 685 undergraduate and 57 graduate students.

March left Irvine in 1969 much to the shock of the group: '… the major incident is Jim leaving, and that was significant because the guy who brought us here and was our intellectual leader was all of a sudden saying that he didn't love us anymore … When the person who left, the kind of father figure, left, that created a disappointment … ' (C. Lave). 'His leading left a big vacuum' (K. Romney). 'One of the things that I was very unhappy about was March leaving when he did. I thought he'd stirred the pot and then just walked away from it. I mean I'm sure Jim has a different story about that. But then I followed' (M. Cole).

The experiment suffered from other stresses as well. First, while the university tolerated and even encouraged political activism and unusual behaviour, the wider community was much less sympathetic to that sort of thing. As Dean Neubauer put it, 'the notion of respectability was really important and having a bunch of freaks running around the School of Social Sciences was not the local community or the Regent's idea of what these universities should be doing'. Here, respectability meant conforming rather than experimenting. Neubauer also highlighted a 'fundament of conservatism' running through the school; this was a 'small "c" conservativism: what you speak is what you know, and what you know best is your own socialisation through your own graduate education, so there is a powerful tendency to reproduced that'. March was also of the view that, notwithstanding the time and place (southern California in the 1960s), the faculty were 'socially and politically relatively conservative'. This was perhaps because most of the young faculty were seeking to advance their careers in the rather conservative national academic structure. At the same time, this desire for career advancement sat uncomfortably with the group's hostility towards the academic status quo, and this then became a second source of stress. In particular, some of the group resisted publishing in journals because 'all academic journals of consequence are in the hands of the disciplines, so therefore publishing in one of them is bad, [and so] we

won't' (Bill Sharpe). This led to something of a crisis in 1968 when some members of the group were coming up to their 'up or out' tenure reviews.

A third external pressure was what Kim Romney referred to as 'the student identity problem: "What am I?" … One of the inherent tensions about inter-disciplinary stuff is that in the 50 s it worked because there was a surplus of jobs. But the moment the jobs get scarce then a student is penalised for not having a named specialty'. Whether students were applying to graduate programmes in other universities or for positions in industry, their ambiguous study programme was problematic:

> So then if you said, 'I'm inter-disciplinary and I know a lot of psych and I know a lot of social, and I want to work in this area', that wouldn't get you a job. There was a period where it was ambiguous for students and they were paying a price for the idealistic notion of inter-disciplinary work. (K. Romney)

> The reality was that we hadn't really thought through what our students meant to us as our product into the world … The outside world wasn't cooperative. And Santa Cruz had the same kind of problem. (D. Neubauer)

More generally, the post-war passion for inter-disciplinary research was waning around that time (House, 1977). In his detailed study of the 'Golden Age' of interdisciplinary social psychology, Sewell (1989) observes that it had 'largely vanished' by the mid-1960s because of, *inter alia*, the threat inter-disciplinarity posed to the traditional university department and the lack of funding for such work. In addition, the post-war enthusiasm for 'big science' was going a bit sour around that time, and there was a growing public suspicion of mathematical modelling, game theory, systems analysis and operations research because they were seen to be, in some way, fuelling the arms race, the Cold War, and military strategy in the Vietnam War. For instance, the RAND strategist, Herman Kahn, who had set out the idea of a winnable nuclear war in 1960, was caricatured in the 1964 black comedy film, *Dr Strangelove*, which helped implant in popular culture the notion of the mad scientist, dazzled by unreal if not surreal understandings of the individual and society (Boyer, 1996; Weiner, 1950). Of course Kahn was not advocating nuclear war, but merely contemplating, in a detached way, the notion of a winnable nuclear war. Others, with a more reflexive take on social theory, felt that this idea could make war more likely. Ironically, these different epistemological positions were reflected in Irvine, which housed the detached, ahistorical, acontextual epistemology of the mathematical modellers, the situated, historical, contextual, reflexive epistemology of the ethnomethodologists, and Program B sitting somewhere in the middle.

The anxieties associated with these wider issues coincided with March's departure as he himself acknowledged: 'There were a number of traumatic things associated with it [his departure], but not particularly my leaving. My leaving necessitated some decisions which articulated some of the differences that were suppressed by my presence, I suppose' (J. March).

In many ways, his leaving marked the end of the experiment. After an untidy appointment process,[7] Kim Romney was appointed Dean, which prompted Mike Cole to leave, and others also left around that time, if not for the same reason. Arnie Binder took the opportunity to create a new unit, called the School of Social Ecology, bringing staff and students away from the School of Social Sciences. While I have not collected much data on the School during the 1970s, my understanding is that it was a period of drift and emptiness. Some of the spirit and excitement of the 1960s remained, but the overwhelming sense seems to be one of disappointment, resentment, and loss. William Schonfeld, who arrived in 1970 when March was leaving, was appointed as Dean in 1982. He continued in this position for twenty years, putting in place a conventional social science departmental structure.

SSS AS CRITIQUE OF E&E

The SSS story provides an interesting lens through which to review the E&E simulation and the assumptions on which it is based. The differences between the two are summarised in Table 1.

The E&E model begins with a randomly selected group of individuals. Initially, the group is disorganised, with no *a priori* structure, and with each individual holding their own belief system. This original disorganisation and differentiation evolves inexorably into one homogenous group with a common, undifferentiated (but not necessarily 'true') belief system. March's 'high variance' strategy meant that this was approximated in SSS, to some degree, though it was obviously impossible not least because March's own position as Dean set him apart from others in the group. Notwithstanding this espoused 'high variety' strategy, the original group was quite homogenous – young academics, committed to an inter-disciplinary, mathematical-based study of social phenomena – that evolved, over time, into a heterogeneous mixture of individuals and sub-groups. In contrast to the E&E model, which evolved into a homogenous, stable undifferentiated group, SSS fractured into a heterogeneous mixture of individuals and factions that was quite unstable. Thus the initial and final conditions are almost polar opposites in E&E and SSS.

Table 1. E&E and SSS Contrasted.

	E&E	SSS
Initial conditions	Heterogeneous; high variance; random selection.	Homogeneous; medium variance; deliberate selection.
Final conditions	Homogeneous; stable; united.	Heterogeneous; unstable; fractured
Superior group	Axiomatically present	Contested
Belief system	Absent	Present
Radical change/ emergence	Impossible	Present
Ontology	Realist	Realist and Constructivist
Epistemology	Relativist	Relativist
Organisation–environment boundary	Clear	Ambiguous

The process of change or learning is also fundamentally different. Central to the evolutionary process in E&E is a 'superior group' of experts who have a truer understanding of reality than that held collectively, and the majority view of this 'superior group' works, stochastically, to change the collective belief system. In contrast, SSS was characterised by a number of powerful individuals, each with quite different beliefs (about reality). Importantly, these experts did not form a group and so there was no sense of a majority expert view, nor much evidence of majoritarianism at work. Instead, factions came to coalesce around these individuals, a phenomenon that was precluded from happening in the E&E simulation.

The E&E model inexorably moves to an undifferentiated belief system across the population, with every individual eventually holding identical beliefs. This absence of structure at the population level also extends to the individual level, where an individual's beliefs about each reality dimension are independent of his/her beliefs about other dimensions. In other words, there is no sense that an individual, within the model, might have a *system* of beliefs, where beliefs on one dimension might be related to beliefs on another dimension. Neither is there the possibility that beliefs might cluster into a meta-level structure. In contrast, in SSS we find clusters of individuals in the collective (Program A, B and the ethnomethologists) reflecting different belief *systems*. While one can never know the structure of an individual belief system, our own experience would tell us that far from being undifferentiated, individual belief systems have a meta-level structure that links beliefs about different domains of reality.

The E&E simulation can also be usefully contrasted with another experiment in organisation: a commune, known as 'the Farm', which existed for about two years in some old farm buildings on the campus. The Farm was administered by SSS and was probably the only commune in a state university. It both symbolised and reflected SSS. Like SSS (and E&E), it had about 50 members, though this number fluctuated considerably. At the outset, there was a degree of homogeneity in the commune: its members shared a common purpose with one another, with other communes dotted around California, and with 'alternative' organisations such as the Esalen Institute, founded in 1962. Some members were students but others were unconnected to the university; as March remembers, they were 'hangers on in one way or another … The people in it were not socially or politically adept; there were innocents … They were injured people. They hated protection'. The SSS commune only lasted a couple of years, similar to most other communes of the time. Jean Lave describes it thus:

> I am sad to say, but as most communes go, the really responsible people did all the work, they got fed up and left, and the less responsible people took over. At the end we were running a criminal enterprise, where drugs were being sold, lumber been sold; it was not okay, so we had to close it down. It was a messy business, and scary; the police were involved.

March concurs:

> One of the conspicuous things was they could never solve the governance problem. They could never figure that out. You wanted a system in which no one told anyone what to do, but on the other hand you wanted the garbage taken out, and they just never got around to figuring out that … I closed it down as one of my last acts as Dean.

But then, 'the university people who were involved evolved it to a little different structure – they started a school' (J. March). This school, which came to be known as the Farm School, experimented with alternative forms of elementary education: It was 'a Programme B kind of a thing … there was this experiment that was right on campus, an alternative education' (M. Cole). The Farm School has continued to the present day, though it is no longer connected with UC-Irvine. And so, with both SSS and the Farm, we find an initial homogeneity that, over time, disintegrates and yet transforms into something radically different. In contrast, the E&E model starts with a *heterogeneous* collective that is undifferentiated (in not having structure) and ends up with a *homogenous* collective that is also undifferentiated (in that all members hold identical beliefs).

E&E starts with disorder – a heterogeneous group with random beliefs about reality – that inexorably moves to order, where all members hold

identical beliefs about reality. This change from disorder to order is incremental, progressive and relentless. SSS presents quite a different picture of the interplay between order and disorder. Even though March's program ostensibly simulated the tension between exploration and exploitation the model always and necessarily converges on a stable, homogenous reality. SSS was quite different in that it was a clear attempt to move from order (the status quo of academia and society, or the homogenous group of young academics) to disorder (the experiment in anarchy), and then, hopefully, to a new order. The experiment was situated in, opposed to, and largely defined by the existing institutional orders. Disorder, then, was wilfully created, valorised and embraced. In contrast to E&E – where the collective continually moved away from disorder – SSS was a much more compelling case study of the dialectic between exploration and exploitation, and the risks and human traumas involved.

Another important difference is that E&E only allowed incremental change, while radical change (such as the departure of Jim March, the creation of the School of Social Ecology, or the death of Harvey Sacks) punctuated the narrative and the participants' meaning-making. If paradigm change is impossible in E&E, it was an intrinsic feature of SSS, most notably in the ironic emergence of radically different theoretical problematics – such as ethnomethodology or Jean Lave's (2009, 2011) progressive shift to a historical materialist position – out of an organisation designed to focus on mathematical modelling and computer simulation. In other words, E&E doesn't contain within itself any possibility of simulating the phenomenon of unintended consequences, which is, prima facie, important in learning.

Another way of thinking through the E&E model is to consider its implicit assumptions about ontology and epistemology. Specifically, it assumes a *realist* ontology (there is only one 'reality', which is exogenous to the group and is *not* socially constructed) and a *relativist* epistemology (knowledge of, or beliefs about, this reality varies between individuals). This philosophical sophistication may partly explain why the model has received so much attention. However, the ethnomethodologists in SSS, while accepting a relativist epistemology would almost certainly reject the model's realist ontology. Instead they would advocate – using a contemporary term – a constructivist ontology, on the basis that 'reality' is neither fixed nor endogenous, but historical, contextual, socially constructed, and situated in local practices. One suspects that they, and many of those in Program B, would see E&E as fatally flawed because its reality is insufficiently dynamic, reflexive and contextual. For instance, the model

glosses over the fundamental problem of how 'experts', who have a superior knowledge of an unknowable reality, could be 'objectively' identified in a social context. (In the E&E simulation, this is achieved by the computer algorithm, which can act as a type of *deus ex machina*). In the extended version of his model, March does allow the knowledge dimensions of reality to change but this is quite independent of processes internal to the group. And while the group in E&E exhibits dynamic learning for a while, this learning stops once the individuals converge on a shared view of reality. Again, the constructivists would be uneasy with this privileging of equilibrium and stasis.

This analysis suggests that the E&E model, notwithstanding its elegance, is deeply flawed. One possible way of progressing is to address these flaws by developing a more sophisticated model, and indeed this has been attempted. For instance, Miller and Lin (2010) have recently developed an agent-based model that seeks to overcome the limitations in March's model. The distinctive features of their more complex computer model are as follows. First, the environment is not objectively given and exogenous, there to be discovered by organisations, but is instead socially constructed (Berger & Luckman, 1966), enacted (Weick, 1969/1979), and hence amenable to organisational control (Cyert & March, 1963). Their environment, which includes a mix of exogenously fixed and potentially controllable elements, changes if the environment is amenable to change and there is sufficient consensus within the organisation about the change to be enacted. Second, they model organisations as having a 'dominant coalition' that leads the interpretive and enactment process. This coalition, which is a randomly selected subset of 10% of agents, has a collective belief system broadly equivalent to March's (1991) 'organizational code'. Third, they incorporate a spatial dimension in their model, on the assumption that individuals learn from their neighbours (after having, with some probability, learned from the code). Fourth, they model three different modes of learning: pragmatism (learning from the best performing neighbour), coherentism (learning from the neighbour with whom they share the most beliefs), and conformism (adopting the most common beliefs among the nearby agents). The added complexity of their model provides the opportunity to simulate and compare many different scenarios. One interesting outcome from the analysis is their finding that March's exploration-exploitation trade-off is 'a rather unique case associated with pragmatic learners and an uncontrollable environment' (p. 110).

On first look, Miller and Lin's model seems appealing, not least because it seeks to operationalise a constructivist ontology within a Marchian

simulation. However, comparing the model to the SSS experiment raises important questions that are not easily answered, and indeed problematise some key concepts. What precisely is the 'environment' that is being modelled? Is it the Vietnam War, the civil rights movement, the intellectual traditions of Carnegie, Yale and Harvard, the salary structures within the University of California? In thinking through the case, it's difficult to categorically distinguish between internal–internal and internal–external interactions. Moreover, there seems to be no sensible way to map individual beliefs onto an external, independent reality without getting into all sorts of philosophical conundrums and regressions. Even what might appear to be a straightforward belief – 'that is a cow' – is only straightforward if we adopt, as most of us do to get by in life, a form of naïve realism. But the problems with naïve realism are clear once we consider that the statement, 'that is a cow', takes on quite a different meaning if said by a beef rancher in Texas or by a Hindu in India. What this reminds us is that context and history matter. There is no 'there' or 'then' (indexicals in the language of ethnomethodology) in the type of modelling that E&E epitomised, nor indeed in much of the SSS experiment. By way of contrast, the central plank of ethnomethodology is the idea that social order is always and only an ongoing, local, situated accomplishment (and in no way exogenous).

The notion of a 'boundary' between the 'environment' and the 'organisation' is deemed to be unproblematic in both March's model and in Miller and Lin's more sophisticated version. However, it is an empirical problem as we can see once we try to operationalise such concepts in the SSS case. Where might such a boundary be located and how might its position be justified? It is difficult to look closely at the SSS case without concluding that, while the concepts of 'environment' and 'organisation' have intuitive appeal, their value seems to disappear once we try to identify and locate a boundary between the two. A key difficulty with such models is that they rely on systems theory, which privileges and reifies concepts like 'organisation', 'environment', 'internal', 'external', 'boundary' and 'equilibrium'. But systems theory has come under sustained attack, not least by the actor-network theorists who argue that the pure boundaries and essential constructs that systems theorists seek to impose invariably break, flow, change and transform. And if a boundary appears fixed, then this is the social phenomenon *to be explained* as an emergent outcome of network-building processes. This is why Latour asserts that the 'notion of systems is of no use to us' (Latour, 1988, p. 198), and why another actor-network theorist, John Law, dismisses functionalism and systems theory as 'the immodest sociology of order that came close to sterilising American social

thought in the 1950s and 1960s' (Law, 1994, p. 98). This disdain for systems is also echoed in the industrial networks literature as epitomised in the work of Håkansson who asserts that 'the environment is not a meaningful concept … more meaningful is the set of related entities' (Håkansson & Snehota, 1989, p. 191).

This leaves us with the perplexing question, 'Why is E&E so influential in organisation and management studies, if the model on which it is based is so defective?' Perhaps the best answer is that context and politics matter, as the actor-network theorists and their precursors, the ethnomethodologists, have consistently argued. If theory and knowledge are contextual, situated and historical, then the concepts of exploration and exploitation, which are intimately connected with March and his seminal paper, can *only* be understood through and with the SSS story. In this respect, E&E is maybe best understood as an emergent outcome of what happened in the SSS.

CONCLUSION

The SSS story is not merely an interesting tale; nor is it just a case study. Rather, it is fundamentally constitutive of the concepts of exploration and exploitation that are central to this special issue and to E&E. March doesn't use explicitly invoke the concept of ambidexterity in E&E, but it is implicit in his paper because his model is structured so that regardless of the mix of exploration and exploitation, organisational learning – as represented by the group coming to a shared, homogenous, stable understanding of reality – is inevitable. Thus, the model is framed around the notion that exploration and exploitation co-exist and that there is an optimum combination of both.

E&E is a simulation model in the tradition of Program A. The computer model on which the paper is built is devoid of context because context, within the Program A tradition, doesn't matter. Ironically, the paper that is E&E (as distinct from the model) can only be properly understood by bringing context back in. Once this is done, we can see E&E as not so much a simulation of organisational learning but rather as a talismanic contribution to a discipline that was still coming to be. The overall story is set in the 1960s when organisational researchers first began to seek a more 'synthetic' organisation theory, in contrast to the tradition up to then which was to study broader political and social processes but in an organisational context. Partly, if not largely, driven by the emerging requirement that business schools engage in scholarly research, organisations came to be distinct objects of inquiry rather than merely 'sites for understanding the

constitution and consequences of modern forms of power' (Lounsbury & Ventresca, 2002, p. 6). Interestingly, March was on the cusp of this transition, with three seminal works providing the foundation for this emerging science of organisation (Cyert & March, 1963; March, 1965; March & Simon, 1958). Even though March has always been sceptical of highly rationalised and instrumental understandings of organisational action – which he sees as embedded in ambiguity, uncertainty, culture and politics – his work has been fundamental in constituting the organisation as an object, and as an object of study. Once behaviour in an organisational context emerged as a distinct phenomenon and object of study – with routines (Cyert & March, 1963) and history (Lindblom, 1959) being seen as important aspects of such behaviour – and once organisations were understood as teleological systems (Simon, 1955), then it was no great conceptual leap to think of, not just learning in an organisational context, but the *learning organisation*. By 1991, when E&E appeared, there was an emerging literature on organisational learning, which was heavily indebted to the 'Carnegie School' (see Levitt and March (1988) for a review). March's E&E model is important within this narrative because it was probably the first simulation model of an organisation learning. And if a model exists, then the implication is that organisations do learn, albeit not exactly as the model depicts (it being just a model). What is not questioned, however, is the notion that organisations learn. Thus, in a rather subtle way, the *model* of organisational learning brings into being the reality of, not only the existence of organisations, but also the phenomenon of organisational learning itself.

Of course James March has little control over how his work is appropriated within a discourse, and thus he is rather ancillary to that phenomenon. It should also be stressed that while E&E can be identified with Program A, his oeuvre is certainly not confined to the Program A tradition. Indeed his other work questions many of the assumptions on which E&E is based (and necessarily based because of the constraints of simulation). For instance, elsewhere he stresses how problematic the commonsensical understandings of autonomy and coherence can be – whether this be at the level of the institution, organisation, group or individual – and how difficult it is, in practice, to distinguish between endogenous and exogenous social phenomena (March & Olsen, 1984). Yet management and organisation theory has largely ignored this aspect of his work, while it pays homage, as witnessed by the cascade of citations, to a paper based on a highly problematic model of reality.

This paper has argued that the SSS story is an important part of a larger political narrative about the construction of what we come to call

knowledge in organisation theory. But it is also more than that. It is a particular story about the joys and sorrows, the fights and factions, and the individual and collective disappointments and frustrations that accompany attempts at exploration in a context where exploitation is, sometimes unseen, also privileged.

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Davis & Marquis (2005); Jacques (2004); Levinthal (2010); March & Olsen (1989).

NOTES

1. March's 'garbage can' article (Cohen, March, & Olsen, 1972) – his next most highly cited paper – is also based on a computer simulation model.
2. One might also identify March's 'organizational code' with Lacan's 'Big Other' (Evans, 1996) or Baudrillard's 'code' (Ritzer, 1997, pp. 80–84).
3. In fact, March was professor of psychology and sociology.
4. Of course, labels like 'constructivist, postmodern anthropologists' would not have been used at the time, and may conjure up a false sense of the place. Instead, Jean Lave's description is probably better: 'We were all pre-postmodern structural functional run of the mill anthropologists.'
5. One manifestation of the ethos of inter-disciplinarity and the emphasis on mathematics was the textbook, *Mathematics for the Social and Behavioral Sciences: Probability, Calculus and Statistics*, which March co-authored with Bernard Gelbaum, Associate Dean at the School of Physical Sciences, based on the course they and others devised to meet the unique mathematics requirements that SSS imposed on its students (Gelbaum & March, 1969).
6. See Garfinkel and Sacks (1970) for their critique of professional sociology's practices, which they see as including attempts to construct a unified sociological theory (as exemplified by the work of Talcott Parsons), model-building (as exemplified by Program A in SSS), and laboratory studies of social phenomena. Sacks distinguished his own pioneering work on conversation analysis (Sacks, Schegloff, & Jefferson, 1974) from Garfinkel's related, but different, work on ethnomethodology.
7. See Olsen (1970, 1976/1979) for good descriptions and analysis of this process.

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