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The Lost Experiment in Exploration and Exploitation.

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Abstract

This paper focuses on James March's 1991 article on 'Exploration and Exploitation in Organizational Learning', which is now the seventh most highly cited paper in management and organisation studies. March's paper is based on a computer program that simulates the collective and individual learning of a group of fifty individuals. The largely forgotten story that this paper re-calls is the real-life experiment that March, 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. Taken together, both stories illuminate important moments in the history of organisation studies. The comparison suggests that March's model, which was probably the first simulation of an organisation learning, also worked to constitute rather than model the phenomenon.

Introduction

This paper focuses on a seminal contribution to management studies, namely James March's article on 'Exploration and Exploitation in Organizational Learning' which was published in Organization Science twenty years ago (March, 1991, hereinafter E&E). March's paper is based on a computer simulation of how fifty individuals, with

randomized views of the world, learn as a collective and as individuals, and, how, over time, they come to share a collective understanding of reality. It has had a profound effect on our subject.

What is interesting, and largely forgotten, is that March's computer simulation of fifty random individuals coming to a collective and shared understanding of reality is mirrored by a real-life experiment that he, in large part, designed and conducted between 1964 and 1969. This paper recollects this second, 'lost' experiment. It begins by summarising the E&E paper and its influence. It then presents a description of the real-life 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.

Recollecting the story is interesting for a number of reasons. First, since March is such an influential figure in management and organization studies, it is worth inquiring into the immediate context out of which the ideas associated with him emerged. If we accept that knowledge is situated (Lave and Wenger, 1991), then context is implicated in theory and theory development, which means that important theoretical contributions to the field, such as March's 'garbage can' model of decision-making, his concept of 'organized anarchy', and his distinction between exploration and exploitation, need to be understood contextually. Second, the 'lost experiment' is an unusual case of a keen and skilled student of organization setting up, running, studying, playing with and leading an organization. This was an interesting and early case where the 'manager'/'leader' is a knowledgeable manager leader, as indeed, to a lesser extent, were those being led/managed. Third, it describes an unusual attempt to escape from, subvert and yet work with institutional powers. It is a case study of creativity, work, power and play that might illuminate similar experiments. Fourth, the experiment figured in the development of significant 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.

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 (Figure 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.

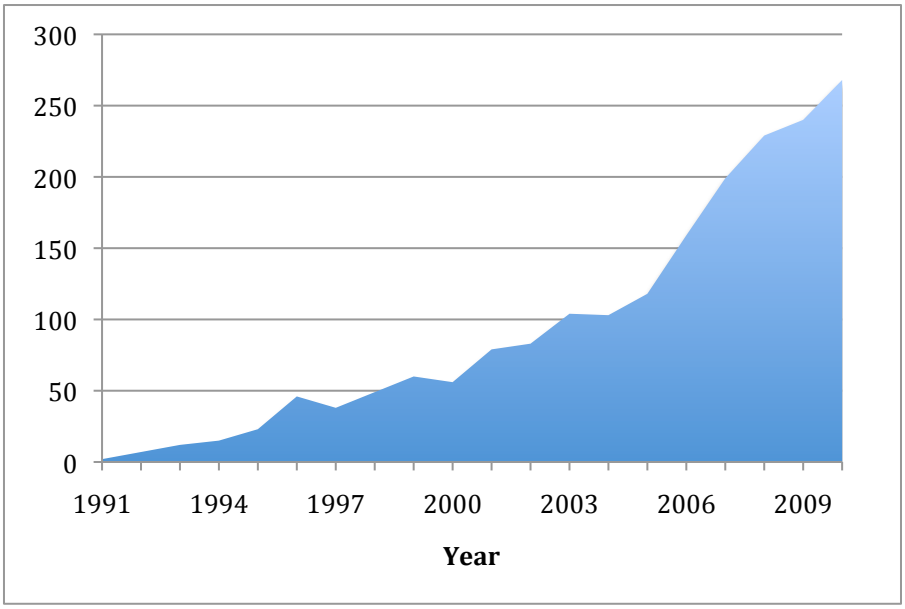

Figure 1. Annual count of citations for March (1991) in the Web of Science.

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

[1] March's 'garbage can' article (Cohen et al., 1972) – his next most highly cited paper – is also based on a computer simulation model.

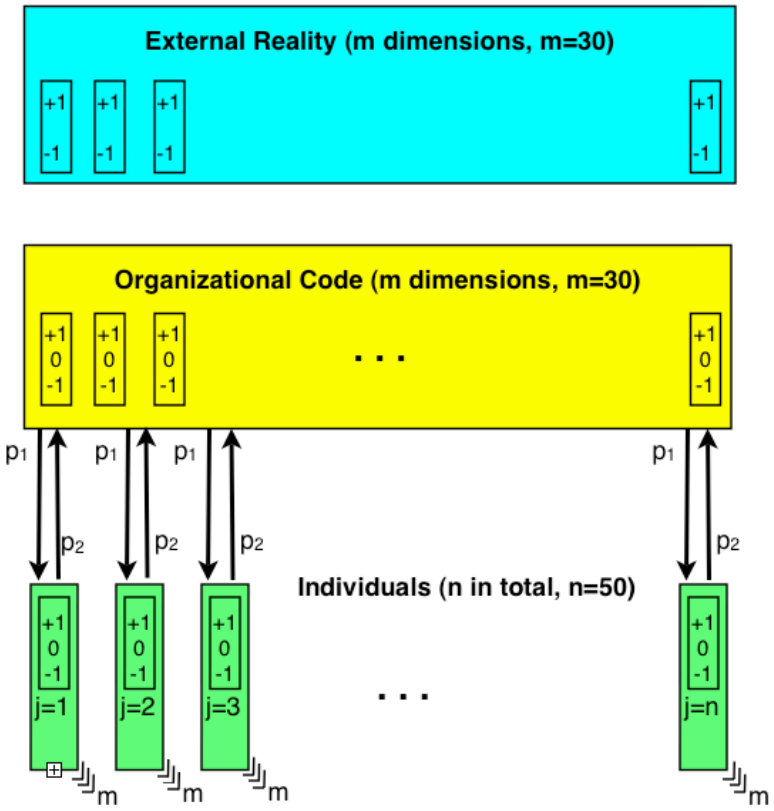

Figure 2. March's model in schematic (after Bray and Prietula {, 2007 #7080}).

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 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 'organizational 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 organizational 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 organizational code (the collective belief) then the individual will change his/her belief

[2] One might also identify March's 'organizational code' with Lacan's 'Big Other' (Evans, 1996) or Baudrillard's 'code' (Ritzer, 1997: 80–84).

with probability p_1 (the effectiveness of socialization). Crudely, p_1 represents *exploitation* as a probability that an organization will influence an individual's knowledge (high p_1 means that collective norms, routines, etc. are strong). The organizational 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 organizational code adapts to the knowledge of the experts and as individuals conform to the knowledge of the organizational code. The organizational 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.

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 organization 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 (2007) modelled a hierarchical structure within the group, Rodan (2008), Kane and Alavi (2007) and Miller

et al (2006) incorporated interpersonal learning rather than learning through a collective code, while Rodan (2005) incorporated a number of different real-world organizational features. These, and others, have extended March's insights, but his original paper remains probably *the* critical contribution to the literature on organizational learning.

The SSS Experiment

The lost part of the E&E story is centred on a real-life organizational 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 (hereafter SSS) 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.

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 (see Kavanagh (2010) for a bibliography and, in particular, the special issue of *Organization Science* in 2007, Vol 18(3)). 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 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. March set out his vision for the Division (the unit was initially called a Division, before it became a School some years later) in a letter he wrote to Ivan Hinderaker, Vice Chancellor of Academic Affairs, in November 1963. 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 organization, 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 specialization 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 (1969-70) 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 (1946-66). Moreover, 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 gets 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 organization, 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 organizations, 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 programs 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 fifteen faculty 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 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

[3] In fact, March was Professor of Psychology and Sociology.

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]

Kim Romney, who joined in 1968, opined that 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 organizations.

> 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.

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[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. Rather, as Lave put it, "We were all pre-postmodern structural functional run of the mill anthropologists."

In implementing an alternative, de-differentiated mode of organizing, “March had set it up so that there was far more equality among faculty, students and staff than in most universities” [Kim Romney]. He and others clearly emphasised socialization 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” [Romney] it was to have staff, students and faculty socializing 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 emphasized the notion of disorganization:

> ‘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 organizational 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. Initially, 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. Oftentimes there were more than three programs.

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 organizational 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.

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[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 in 1969, based on the course devised to meet the mathematics requirement (Gelbaum and March, 1969). This textbook was the outcome of the work that March, Gelbaum and others did in teaching mathematics to Irvine's social science students.

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, Nov. 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, 1991 #2604}, and Mike Cole whose research focused on cognitive development and cultural psychology.

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). 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: 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 et al., 1984; Lave, 1988). 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,

[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 et al., 1974) from Garfinkel's related, but different, work on ethnomethodology.

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…" [Charles Lave]. "His leading left a big vacuum" [Kim 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" [Mike 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 50s it worked because there was a surplus of jobs. But the moment the jobs get scarce then a student is penalized for not having a named specialty". Whether students were applying to graduate programs in other universities or for positions in industry, their ambiguous study program 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. [Kim 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 [Deane 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' also went a bit sour around that time. In particular, 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 (Weiner, 1950; Boyer, 1996). 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.

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[7] See Olsen (1970; 1976/1979) for good descriptions and analysis of this process.

Reviewing 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.

Structural issues

The E&E model assumes that the group of individuals is random and disorganised at the outset, with no *a priori* structure. 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. More generally, there are probably relatively few occasions where a group has no *a priori* structure and so E&E simulates an organisational oddity at best. More recent models seek to address this structural issue: for instance Kane and Alavi (2007) have ten teams of ten individuals as an initial condition in their extension of March's model.

In the E&E simulation, the original disorganisation inexorably evolves into one homogenous group with a common (but not necessarily 'true') understanding of reality. Central to the evolutionary process 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' may, stochastically, change the collective belief system. A quite different process operated in SSS. If anything, the process was reversed. Notwithstanding the 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. And instead of a single 'superior group', 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 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 symbolized 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. The members were not connected to the university, and were, as March remembers, "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 heterogenous, undifferentiated collective and ends up with a homogenous, undifferentiated collective.

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 is 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.

Order and chaos

E&E starts with a disorder – a heterogenous group with random 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. In contrast, SSS presents quite a different picture of the interplay between order and disorder and exposes some of the E&E model's limitations and assumptions. First, the SSS experiment was in many ways an 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.

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. Furthermore, paradigm change is impossible in the E&E model, while it was an intrinsic feature of SSS, most notably 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.

Ontology and epistemology

Another important point is that the E&E model 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, 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 the model 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.

Miller and Lin (2010) have recently developed an agent-based model that seeks to address some of these perceived limitations in March's model. The distinctive features of their more complex model are as follows. First, the environment is not objectively given and exogenous, there to be discovered by organizations, but is instead socially constructed (Berger and Luckman, 1966), enacted (Weick, 1969/1979), and hence amenable to organizational control (Cyert and 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 organization about the change to be enacted. Second, they model organizations 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' 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 'organization' 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 seek 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: 198), and why another actor-network theorist, John Law, dismisses functionalism and systems theory as "the immodest sociology of order that came close to sterilizing American social thought in the 1950s and 1960s" (Law, 1994: 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 and Snehota, 1989:191).

Reviewing SSS

March's paper on exploration and exploitation is now a classic in the organisation studies canon, with an enduring and wide influence. In contrast, the S&S story is largely unknown and forgotten, part of organisation studies' forgotten history.

But the SSS story is worth recovering for a number of reasons. First, if we adopt Lave's later position of seeing theory and knowledge as contextual, situated and historical – then to understand, *inter alia*, E&E, we need to look at the context, practices, and historical narrative within which this knowledge is situated. In this respect, the SSS story is an integral part of E&E, and indeed E&E is maybe best understood as one emergent outcome of the SSS story.

Second, the SSS story may itself be understood as a continuation of the GSIA story, which is already well documented (Gleeson and Schlossman, 1995; Augier and March,

2001; 2002; Hosseini, 2003; Augier and March, 2004; Crowther-Heyck, 2005; 2006; Augier and Prietula, 2007; Gavetti et al., 2007; March, 2007; Tadajewski, 2009)}. The phenomenon that was GSIA, and the 'Carnegie School' that emerged out of it, profoundly influenced not only the fields of organisation and management studies, but also contemporary understandings of the nature of the business school and management education. For instance, Daniel Levinthal, on assuming the editorship of *Organization Science* in 2010, made the following comment:

> I can't help but observe over the 20-year history of this journal the strong imprint of the "Carnegie School" on the journals editors, starting, of course, with Arie Lewin as the journal's founding editor. Arguably, the basic research agenda of the Carnegie School was to understand the nature of organizational intelligence in light of the bounded rationality of its individual members. This strikes me as a powerful conceptual basis for the emerging field of organization studies in the late 1950s, as well as a useful defining agenda for a journal of *Organization Science* in 2010. (Levinthal, 2010: 803)

The management historian, Roy Jacques, also calls attention to the influence of the Carnegie School, but is less optimistic about the future: "The last period of great vitality in organisation studies was the 1950s, stretching into the late 1960s. What has followed has increasingly been, to borrow an image from Yeats, the rattle of pebbles under a receding wave" (Jacques, 2004: 62). Similarly, Davis and Marguis identify the 1950s as a watershed in organisation theory's development, but make the important observation that

> most organizations [are] mere legal fictions with no 'inside' or 'outside' analogous to borders – they are simply dense spots in networks of contracts among sovereign individuals (who may themselves be mere fictions – Jensen and Meckling 1976). With corporations, there is no there there – they are simply legal devices with useful properties for raising finance. (Davis and Marquis, 2005: 332)

One thing that happened in the 1960s was that organisational researchers 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 organizational 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 and Ventresca, 2002: 6). Interestingly, March was on the cusp of this transition, with three seminal works providing the foundation for this emerging science of organisation (March and Simon, 1958; Cyert and March, 1963; March, 1965).

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 and 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 a substantial 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 organizational learning brings into being the reality of, not only the existence of organisations, but also the phenomenon of organisational learning.

March's E&E paper was certainly interesting and his model was ingenious, but in many ways the insights are fairly ordinary – for instance the notion that slow learners (stubborn or stupid people) will inhibit groupthink. However, when we set it within the larger story, of which the SSS story is a microcosm and a hinge point, we can see, with much greater clarity, why *Exploration and Exploitation in Organizational Learning* has become a classic in organisation studies' relatively short history.

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