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The mysterious morphology of immiscible liquids: A study of scientific practice

Introduction

Until recently, the nature of scientific work has been told at second and third hand by historians, philosophers, and sociologists of science, discussed in journalistic and anecdotal essays and biographies, and recalled in the memoires of scientists. The movement away from armchair and anecdotal reconstructions of scientific work began with critiques of idealistic and heroic histories of science, positivist philosophies of science, and normative-functionalist sociologies of science. More recently, the movement from critiques to new modes of research and theory in science studies has spawned an ethnographic approach to the study of scientific practice. Sociologists of science have begun to focus on scientific work as an "on-line", "real-time" process. Our research was undertaken in this ethnographic spirit.[1]

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Objectives and methods

Our objectives are, first, to describe "what happened" in the laboratory we studied between the time a project was initiated and the culmination of the project in a published paper; second, to discuss the differences in style and rhetoric among the three major documents associated with the project — the report to the funding agency, the manuscript originally submitted for publication, and the manuscript accepted for publication; and third, to discuss the role of "contingencies" in problem formulation, dealing with difficulties and anomalies, constructing theories, and publishing results. We argue that contingencies (social and otherwise) do not simply affect the course of scientific work (i.e., they are not simply "externalities") but rather that they are an integral part of that work.[2]

We do not claim that our account of work in the laboratory is superior to or more "factual" than other accounts that might be given by other observers (including accounts by the laboratory scientists themselves). Our aim is to give *an* account of scientific practice based on a variety of standard ethnographic methods — in-depth interviews; observations; the analysis of notes and conversations, draft manuscripts, and published papers; and direct participation in laboratory work. Our account is thus one of many alternative accounts that are possible. This is a consequence of what we call the "Rashomon theorem": there are numerous, if not an infinity of, ways of describing and interpreting any given phenomenon; the status of any given account is not determined *a priori* by whether it is "true" or "real" in some absolute sense, but by how useful it is in the competitive realm of knowledge production and utilization.[3]

Our study spans a period of approximately two years (1979-1980). During the first year, we — singly or together — were in the laboratory two to four days a week, sometimes for several hours at a time. We were, for the most part, observers; sometimes we asked questions, sometimes we watched people at work, sometimes we pored over laboratory notebooks. But we also participated in efforts to explain the results of the morphology experiments. We spent less time in the laboratory during the second year, but interviews, observations, the analysis of notebooks and documents, and participation in theoretical work continued. The following description of the morphology research is based in part on our reconstruction of events that occurred prior to the time we entered the laboratory, and on our own observations during the two years we were associated with the laboratory.

Colloids and chemists

The discipline: colloid chemistry

Colloid chemistry is a remarkably complex discipline which is concerned with phenomena ranging in size from one to one-tenth of a micron. If we use the ordinary categories of liquid, solid, and gas to describe the phases of a physical system, then colloid chemistry can be described as the study of mixed or intermediate states such as mixtures of dissimilar liquids or liquid-gas systems of given substances. Bubbles, foams, emulsions, and slurries are the relevant phenomena in colloid chemistry. Such phenomena are part of everyday life in households and industries; thus, colloid chemistry (along with surface chemistry) is intimately tied to applications. It is important in creating exotic alloys as well as salad dressings that will not separate.

The chemists

An alarm sounds. Pilots race to their planes, take off, and climb rapidly to meet approaching bombers. But oil spews from their engines and smears the windshields. What causes this problem, and how can it be remedied? This problem provided Sydney Ross with a major post-doctoral research project at Stanford University during the 1940s. Ross went on to become a specialist in colloid chemistry, and now heads the laboratory we studied.

Ralph Kornbrekke majored in physics as an undergraduate. At the end of his first year in graduate school, he transferred from the laboratory he had been working in to the Ross' laboratory. His decision to transfer to Ross' laboratory was influenced by the recommendations of his peers, tacit institutional pressure to find a funded project, and the fact that his original interest in solid state chemistry had led him to surface and colloid chemistry. Ross does not have a reputation as a grantsperson; but Kornbrekke was attracted to his laboratory by what he perceived as the "wide-open" character of colloid chemistry, and its "practicality". He viewed the discipline as an "employable field"; indeed, he expressed a keen awareness of the differences between areas which are heavily funded as academic research but do not readily lead to employment upon graduation, and those areas which get modest levels of support within a university context but lead to numerous employment possibilities.

Henry Hollinger is a physical and theoretical chemist who specializes in thermodynamics and statistical mechanics. He is what we refer to as a *resident theoretician*. Colleagues (including Ross) come to him with theoretical and conceptual dilemmas, or simply to get a "theoretical framing" for an experimental piece of work which is ready to be reported. His role is such that he appears to get credit in an acknowledgement sometimes for a contribution that might give him a claim to co-authorship.

The laboratory atmosphere

Ross' laboratory consists of four rooms on the third floor of the Cogswell Laboratory Building on the Rensselaer Polytechnic Institute campus. There is also a basement in another building which is sometimes used by members of Ross' laboratory. Ross generally works very closely with one or two graduate students; there are usually three or four additional people working in the lab — other graduate students, undergraduates doing senior theses, and occasionally high school students soon to matriculate at RPI.

Ross has a strong bond of *professional* intimacy with his older and premier graduate students. He is uncomfortable with students who have "personal problems", and he will not tolerate what he refers to as "neurotic personalities" in his laboratory. Ross and his students are linked to the world outside the laboratory and the profession not by a shared social life but by anecdotes and tales about life "out there".

Ross does not see himself as a "director". His image of the educational transformation that ideally takes place in his laboratory is one in which "students" become "collaborators". He does not think that "too many ideas arise from cross-fertilization between the students"; he notes, however, that he does not "monitor their conversations" and there may in fact be more intellectually stimulating interactions among the graduate students than he is aware of.

As the morphology research developed from 1977 onwards, Kornbrekke and Ross developed the professional bond that would eventually make Kornbrekke the "top man" or "senior man" in the laboratory (Ross' designations for his premier advanced doctoral candidate). By the time we entered the laboratory toward the latter part of the Fall 1978 academic term, Kornbrekke was "top man" and nearly ready to begin writing (with Ross) the first paper on the morphology project, a report to the National Aeronautics and Space Administration (NASA), the agency which had funded the project. At this time, Kornbrekke and two other full-time PhD candidates were working in the laboratory, along with two first year MS students. There were also three undergraduate seniors working in the laboratory on theses related to the morphology project. Kornbrekke noted that he was working on "conceptual things" at this point, whereas earlier in his graduate career Ross had had him "doing *things*", that is, working on more or less mechanical problems. He was now in what Ross referred to as a "mature state"; another sign of this was the fact that he was now being given responsibility for writing reports for and meeting with officials of NASA. Still, the hands-on aspect of laboratory work remained a central part of Kornbrekke's research. We will have more to say about the laboratory atmosphere later on.

The morphology research

The following account of the morphology research focusses on the work of the graduate student Kornbrekke. It is based on in-depth interviews with Kornbrekke as well as with Ross and Hollinger; analyses of Kornbrekke's detailed log of his laboratory work; and our observations in the laboratory. Our account focusses in part on Kornbrekke's socialization as a scientist. We are concerned here with how Ross and Kornbrekke got involved in studying the morphology of a liquid-liquid dispersion; why they chose to study the two-component system benzene-water (with ethanol as a nonreactive solvent); what problems arose in the course of the research and how they were solved; and what contingencies were associated with the research.

During his first year in graduate school, Kornbrekke did some work on solar cells and surface effects. He also took a course on the chemistry of interfaces taught by Ross — and, for reasons noted earlier, Kornbrekke sought out Ross as an advisor. When Kornbrekke arrived in Ross' laboratory, two projects were in progress. One was an Environmental Protection Agency project on pesticides; the other was a NASA project on emulsions.

The morphology story begins at NASA. NASA scientists are interested in making alloys of two metals for certain space applications. But their efforts at "alchemy" prove unsuccessful. A researcher at NASA about to be promoted to project director contacts Ross. Ross is a NASA consultant and has a good reputation there. The NASA project director and Ross discuss a research proposal, the proposal is written, and the morphology project receives funding. This establishes the "NASA connection", a significant and continuing influence on the nature and directions of the morphology project.

Ross (1973, p. 54) had written a paper on "Adhesion vs. cohesion in liquid-liquid and solid-liquid dispersions" some five years prior to beginning work on the NASA project. This paper was a conceptual analysis prompted by Ross' readings in his collection of rare books in the history of science; the analysis contributed to Ross' view of alloys as emulsions. Metallurgists at NASA had been using phase diagrams as guides in their work on creating alloys. Experiments were therefore developed to study emulsions and elaborate the phase diagrams. The morphology of systems containing two immiscible liquids is not given in their phase diagrams; one of the early aims of the morphology research was to gather data that would make it possible to include morphology in the phase diagrams of two-component immiscible liquid systems. Ross put Kornbrekke to work on the morphology experiments.

Kornbrekke, more or less on his own at this point, had to decide what system — that is, what mix of liquids — he should study. One factor that had to be taken into account was NASA's prohibition on experiments in space with aniline, a substance NASA scientists considered too dangerous to work with. One of the senior graduate students in the laboratory had been studying foaming for a certain system and had a great deal of data on surface tensions for this system; Kornbrekke decided it was reasonable for him to study the same system. His efforts were frustrated. He constructed isothermal bath experiments but was unable to make up a mixture which behaved predictably near the critical temperature (inversion point). He turned to the literature and found eight different critical temperatures reported for the system he was studying, with a range of 15 degrees. This appeared to be a recalcitrant system. Ross, sensing Kornbrekke's frustration (Kornbrekke had worked an entire summer so far), told him to drop the system he was working on. He told Kornbrekke that another student was measuring interfacial and surface tensions for a benzene and water system and that perhaps this system would "behave better". Kornbrekke was particularly attracted to this idea since it would involve a new experimental procedure. He would not need to use an isothermal bath. Isothermal baths, notoriously temperamental according to Kornbrekke, are used for cooling experiments; the new experiments would involve *manually shaking* test tube mixtures. This was an innovative approach to the morphology problem and appeared to offer some hope for success.

Ross, apparently still very much aware of Kornbrekke's frustration over the first set of experiments, suggested a procedure for the new set of experiments. He recommended setting up a variety of test tube mixtures, each having different volume fractions of benzene and water (this mixture soon came to be referred to around the laboratory as "oil and water"). The test tubes were to be *manually shaken* and visually examined to determine whether the emulsion type was benzene in water or water in benzene. The morphology mystery emerged from these shaking experiments.

Kornbrekke's first problem was to determine what emulsion type he had after shaking. This sounds innocent enough, but it is fraught with difficulty. After shaking, most of the mixtures separate quite rapidly and one sees a colorless solution with a highly active boundary or interface between the two volumes of liquids. All the standard techniques for determining emulsion type or morphology apply only to *stable* emulsions. Kornbrekke had *to teach himself to see* what a short-lived emulsion "looks like". By "thinking about what you have" (in Kornbrekke's words) and hypothesizing relevant visual parameters, Kornbrekke had to make the data manifest themselves. One could hardly ask for a closer parallel between "scientific observation" and "aesthetic seeing" (Hanson, 1958; 1969) than what is involved in determining emulsion types in these shaking experiments. The foreground-background character of perception is crucial here; Kornbrekke found it extremely difficult to judge the relative motions of the liquids as they separated and moved up or down in the test tube. He could not judge initially whether droplets were moving up (or down) against a stationary liquid background or whether the liquid background was moving down (or up) while the droplets remained stationary. After numerous observations, Kornbrekke was able to distinguish "a roving interface"; droplets seemed to be moving in two directions, but "one direction was smooth, the other showed oscillation". He "saw" that the "emulsified droplets *break* into their phase". In the end, Kornbrekke learned to focus on the interfacial boundary because it forms right after the shaking stops. By attending to the way the droplets coalesced and the direction in which they ruptured, Kornbrekke was able to satisfy himself and others that he was able thus to determine the morphology of different mixtures. In spite of the fact that the shaking technique was not mechanized (something we heard several outside scientists object to), the results seemed to be *reproducible*. The question was: "Is the result reasonable?"

Kornbrekke had anticipated certain results based on conversations with Ross; his expectations were violated in a rather odd way. When he shook a test tube containing approximately equal volumes of liquids A and B, he sometimes got A dispersed in B and sometimes B dispersed in A. Thus, it was this *inconsistency* — in effect, the impossibility of determining the inversion point — that was reproducible. Kornbrekke was unwilling to go to Ross with such results. He consulted a graduate student working in the laboratory who had a background in statistics. The student was a chemical engineering major who had recently moved to a workplace right across from Kornbrekke's territory in the laboratory. This student suggested that Kornbrekke should try an "averaging procedure". Kornbrekke obtained data on the probability of getting A dispersed in B for a given relative initial volume of A and B. These data were plotted and Kornbrekke discovered that a straight line relationship could be obtained by plotting logarithms. Ross then noticed that the analytic expression for the plotted data was formally analogous to the Boltzmann expression for entropy. This was interpreted to mean that here is a case where the Boltzmann W's of statistical mechanics would be proportional to *probabilities* that are *directly measurable*. This was a remarkable possibility, according to the chemists; they spoke, apparently somewhat loosely, about the possibility of "seeing entropy".

After discussions with Hollinger about interpreting the experimental results appeared to shed some light on some of the thermodynamics arguments, Ross began preparing a manuscript on the research. In doing so, he discovered that the thermodynamics arguments which were to provide a basis for a mathematical description of the observations were invalid. Not only was the derived curve quantitatively incorrect, but it even had the wrong slope. There were thus two anomalies that emerged in the morphology research. One was the experimental result that the inversion point is stochastically related to the volume fraction; the second was theoretical: straightforward thermodynamics arguments which should have worked (according to Hollinger) failed.

At this point, the theoretical work was in limbo. Ross felt that the experimental results were intrinsically interesting and deserved publication on that basis. It seemed to him that an extremely complicated model based on high level mathematical work would be needed to provide a theoretical basis for the observations. In his judgement, further experimentation was unwarranted. He quite willingly admitted that at this point he had lost interest in the problem.

Kornbrekke continued to think about the problem of modeling the experimental results; he suggested a "thin films" model in which the driving forces toward nucleation would come primarily from the cohesion of the liquid. Ross and Kornbrekke discussed the model and the possibility of additional experiments at some length. When Hollinger was involved in these discussions, he would offer an occasional suggestion or question. He felt, as we noted earlier, that "if statistical mechanics ever works, it should work now", that is, in the case of the liquid-liquid dispersion results. He continued to reserve judgement until he could examine the thermodynamics arguments upon which Ross relied for his conclusion that the slope of the derived curve was wrong.

Ross countered every suggestion made by Kornbrekke regarding what could be done experimentally to determine the driving force behind the morphology process. According to Ross, the effects due to cohesion cannot be readily separated from those due to viscosity and surface tension. It is possible to imagine various experimental configurations but the physical situation is such — apparently — that parameters cannot be separated. Therefore, it is not possible to treat one parameter as an independent variable. Ross believed that he and Kornbrekke had been quite lucky to start with a configuration that led to a relation in which the probability of a certain morphology is a smooth continuous function of the concentration.

Kornbrekke and Hollinger remained interested in the theoretical problems raised by the morphology experiments, but were uncertain about how to proceed. Zenzen then came across a general article on critical point phenomena in *Scientific American* which he (acting as a participant) thought might stimulate some new thoughts on the morphology problem (Wilson, 1979). Ross left the country at this point, leaving Kornbrekke with the task of sending the morphology research paper to the *Journal of Colloid and Interface Science*. Kornbrekke had serious reservations about some of the claims made in the paper and about the interpretations; it should be stressed that Ross takes full responsibility for writing papers co-authored with graduate students. Kornbrekke began re-examining and re-plotting his original data (gathered about a year and a half previously), and discovered some arithmetical errors and questionable experimental practices. In any event, the paper was submitted.

The morphology project was beginning to run down. The submitted paper was a revised version of a report to NASA. It was not accepted. One of the major criticisms raised by the referees was that the paper lacked a theoretical foundation. Hollinger was able to help with a theoretical frame (in his role as theorist), and after some additional modifications the paper was resubmitted and accepted. We will examine these documents in some detail below. But first we offer some reflections on what we call "a phenomenological laboratory" prompted by Kornbrekke's experience of "teaching himself to see".

A phenomenological laboratory

Kornbrekke's experience in learning how to identify emulsion types in the shaking experiments was not, in the context of Ross' laboratory, idiosyncratic. Indeed it appears to reflect precisely the outcome Ross consistently seeks in what can be characterized as "a phenomenological laboratory". The emphasis is on "seeing", and especially on "learning to look for something different". Ross admires Faraday. He has written scholarly papers on Faraday, and a portrait of Faraday hangs in the office adjoining the room where the shaking experiments were carried out. Faraday's style of research is the model for Ross' scientific work, and the model he tries to pass on to his students. One hears Faraday's motto: "Work, Finish, Publish"; and one senses Faraday's spirit as Ross tries to educe in his students that power of observation that will allow them to look at a situation and see what no one else has seen before.

There are, Kornbrekke reports, "anomalies everywhere"; and this is generally accepted not as a temporary situation awaiting its Kuhnian resolution, but as characteristic of a creative research environment. The anomalies sometimes cause consternation; sometimes they are viewed as curious puzzles; sometimes they are ignored. But they are always there. Bits of rubber tubing, styrofoam, scotch tape, and aluminium foil are converted, adapted, modified, and transmuted into tools of modern research. Idiosyncratic test-tubes, floor to ceiling tubing, outrageous twists and turns in home-made glassware are additional signs of the alchemic instincts drawn on in this laboratory.

But there is more to this laboratory than the romantic idea of a deceased scientist's spirit, or the equally romantic notion of alchemic instincts. The phenomenological quality of this laboratory is a sign of adaptation to an environment of scarcity. The interface between the laboratory scientist and his/her technology becomes more tightly bonded under conditions of scarcity than it is likely to become in a well-funded laboratory where state-of-the-art equipment can be taken for granted. The very shaking experiments at the center of the morphology mystery were designed to provide Kornbrekke with a relatively simple, inexpensive project. As we travel through the morphology project with him, we find Kornbrekke cleaning glassware, scrambling for parts from shelves of outdated electronic equipment to put together a Wheatstone bridge for the thermo-couple that he will use instead of an expensive electronic thermometer for temperature control, blowing his own glassware. Survival — and success — in this laboratory depends on getting the "hang of putting things together out of what's available". So Kornbrekke, like the graduate students before him, has developed a sense of the importance of *generational linkages* — the tranmission of survival skills takes place through the interactions between the older and the newer students. This is not unusual except that in this case scarcity makes it a more salient and crucial feature of social interaction. People must be good at electronics, and handy with tools and various materials so that the inevitable problems of building, maintaining, and modifying apparatus can be handled. It seems obvious to us that selection and socialization in such an environment of scarcity will produce a different scientific "type" than selection and socialization in a laboratory environment of abundance. The environment of scarcity seems more likely than the environment of abundance to put a premium on a keen awareness of, and intimacy with, a wide range of "things" which — in one form or another — can be drawn on to nourish the research process — and among these "things" one must count the five unaided senses.

The rhetoric of persuasion in science

Three principal documents are associated with the morphology research we have described. The first, published as a quarterly report to NASA, is titled "Emulsion-type inversion for the system benzene, ethanol, and water"; it is co-authored by Kornbrekke and Ross. The name order is reversed in the paper submitted for publication; it is titled "Change of morphology of a liquid-liquid dispersion as a stochastic process". This paper was submitted to the *Journal of Colloid and Interface Science* in the Winter of 1980. In March, the journal editor informed Ross that he and Kornbrekke would have to comment on the criticisms raised by the two referees and/or make appropriate revisions before a "final decision on acceptance" could be made. The paper was revised and resubmitted in September 1980. Within less than a week, Ross and Kornbrekke were notified that the paper had been accepted for publication.

The relationship between documents and scientific practice has been discussed in earlier studies of laboratory life. Latour and Woolgar for example (1979, pp. 51-52, 245), argue that there is "an essential similarity between the inscription capabilities of apparatus, the manic passion for marking, coding, and filing, and the literary skills of writing, persuasion, and discussion"; the laboratory is "a system of literary inscription". Writing is "not so much a method of transferring information as a material operation of creating order". Knorr and Knorr (1978, p. 39) have studied the relationship between scientific practice and published paper in a laboratory in some detail. They analyze texts as media for *constructing* reality rather than as "data which represent reality". We do not have the space to discuss all the documents associated with the morphology project in detail. The works of Latour and Woolgar, and Knorr and Knorr, offer a rationale for focussing on the *persuasive* aspects of article production. We will examine the three principal documents produced by Ross and Kornbrekke, then, as persuasive efforts. We do not claim they are designed to persuade people that something that doesn't exist does exist; nor that persuasion is the only aim scientists pursue in writing papers. We do claim that scientists need to use a *rhetoric of persuasion* in order to draw attention to and legitimate their findings. Getting a paper which sets forth claims accepted for publication requires skillful use of a persuasive rhetoric. This is not simply a matter of choosing the "right" words, but of deciding when and how to use analogies, mathematics, systematic theory, and so on.

We begin by describing some of the differences among the three documents and in particular between the paper originally submitted for publication (M1) and the revised version (M2) accepted for publication. We will then examine these differences in terms of their persuasive relevance.

The manuscript submitted for publication (M1) was a revised version of the first formal document on the morphology research, the NASA report. The text of the eighteen-page NASA report is all under one subheading: "Phase diagrams and morphology". There are five references, one table, and six diagrams in the NASA report. M1 is organized as follows: (1) abstract, (2) subheadings: Factors affecting morphology; Experimental section; Results. Only two of the five references in the NASA report are included among the seven references in M1.

Most of the text changes in M1 make the argument more general, more conclusive and less controversial (this is accomplished by selecting words that indicate greater confidence in what is being reported, or by adding qualifications), and more technical. Here are some illustrations: (1) a "certain range" (NASA report) becomes an "intermediate range" in M1 (specification); (2) "is shaken a large number of times" (NASA report) becomes "is given a large number of trials" in M1 (technicization); (3) "Phase diagrams and morphology", the single subheading in the NASA report, becomes "Factors affecting morphology" in M1 (generalization); (4) "defined" (NASA report) becomes "defined operationally" in M1 (specification); (5) "cohesive forces overcome to some degree by the physical action of shaking" (NASA report) becomes in M1 "cohesive forces must be overcome by an input of energy from mechanical agitation" (technicization); and (6) "This mechanism holds..." (NASA report) becomes "The rule that..." in M1 (generalization). In addition, there is a pragmatic aspect to article production illustrated by the changes in the explanatory framework adopted in each document. The NASA report contains a speculative section on entropy; this is replaced by a magnetic coin analogy in M1; in response to referees' criticisms, M2 eliminates the magnetic coin analogy and introduces a statistical mechanics argument. Let us examine a few of the details of these transformations.

In the NASA report, the following mathematical discussion occurs:

> The cohesive energies of the two liquids leading to one type of emulsion may be opposed or assisted by the tendency of the system to produce the least interfacial area. When the more cohesive liquid is present at a minority volume fraction, the tendency for it to appear as the dispersed phase is assisted; but when it is present at a majority volume fraction, the tendency is opposed. The inversion point of the emulsion occurs at a volume fraction where the two tendencies are equal. These two factors, energy or force of coalescence, and degree of subdivision of liquids (and area of films) depends upon the amount of shearing of the two liquids as well as their volume. Hence the viscosity (η), density (ϱ), surface tension (γ), interfacial tension (γ_i) and volume fraction ϕ (where ϕ represents the volume fraction of (A) the less dense phase) of each liquid must be considered to determine the Helmholtz Free Energy (ΔA) of coalescence:
>
> $$A = \gamma \Delta \Sigma, \ \Sigma = \frac{k \phi \gamma_i}{\varrho \eta}$$
>
> where k is the constant which relates ϕ, ϱ, η and γ_i to the area.

In another section of the NASA report, there is a discussion of probability and entropy:

> Probability of the existence of one state versus another is a function of the entropy change between states. The inversion region describes conditions where two different states can exist. The probability is related to the entropy difference as follows:
>
> $$\Delta S = S_{final} - S_{initial} = k \ \ln(1-P) - \ln P$$
>
> $$\Delta S = -k \ \ln\left(\frac{P}{1-P}\right)$$

The Helmholtz Energy, if differentiated with respect to temperature (T) is also ΔS:

$$\left(\frac{\delta \Delta A}{\delta T}\right)_{p,V} = \Delta S = \frac{\delta}{\delta T}\left(\frac{k_B \gamma_B (1-\phi)\gamma_i}{\varrho_B\, \eta_B} - \frac{k_A \gamma_A \phi \gamma_i}{\varrho_A \eta_A}\right)_{p,V}$$

Equating the two expressions gives:

$$-k\ \ln\left(\frac{P}{1-P}\right) = k_B(1-\phi)\frac{\delta}{\delta T}\left(\frac{\gamma_B\gamma_i}{\varrho_B\eta_B}\right)_{p,V} - k_A\phi\frac{\delta}{\delta T}\left(\frac{\gamma_A\gamma_i}{\varrho_A\eta_A}\right)_{p,V}$$

This equation is similar in form to the one obtained empirically, but with additional modifying terms.

There is also a reference in the NASA report to Maxwell's equations, although the equations are not actually written out:

> The dispersement of tiny droplets of one phase will alter the conductivity in the direction of the conductivity of that phase and proportionally to the quantity present, as in Maxwell's equations.

All mathematical equations, with the exception of one which has no theoretical import (the standard deviation formula) are deleted in M1.

M1 is organized into three sections: factors affecting morphology, experimental section, and results. There are no references in M1 to the Helmholtz Free Energy equations, nor to entropy. The emphasis is on the stochastic nature of the "ability of the dispersion to invert":

> The morphology of systems containing two immiscible liquids is not a phase-diagram variable, nor can it be predicted from any current theory but must be observed in every case. The ability of the dispersion to invert is spread over a range of compositions, and within that range only a probability of one type rather than another can be determined.

The entropy discussion and the accompanying equations are replaced in M1 with a magnetic coin analogy. This change reflects the uncertainties associated with the otherwise interesting exercise in linking probability and entropy as an explanatory strategy. In M1, Ross and Kornbrekke eschew esoteric argument in favour of a modest explanatory effort based on an analogy to an everyday phenomenon — coin tossing. The idea of a *magnetic* coin subtly transforms the everyday phenomenon into a complex physical analogy but one that remains intuitively accessible (at least in the view of Ross and Kornbrekke). It should be noted that the magnetic coin analogy was discussed at great length in trying to account for the results of the shaking experiments.

Ross and Kornbrekke were not persuasive enough in M1 to satisfy the two chemists who refereed their paper. They were, however, persuasive enough to earn a recommendation for publication with revisions from one referee. The major criticism in the other review turned on theory. In order to satisfy the second referee, Ross and Kornbrekke had to delete the magnetic coin analogy (which the referee saw as an indication that their treatment was not "fundamental" and therefore more suited to a chemical engineering journal), and construct a theoretical frame for their experimental results. They were able to supply the theory with some assistance from Hollinger. The major change in M2 is the inclusion of a statistical mechanical interpretation in place of the magnetic coin analogy.

The abstract for M2 begins with the statement: "We report a new phenomenon"; this is followed by a more general statement than occurs in the earlier versions — the new phenomenon is "that the morphology of an unstabilized liquid-liquid dispersion is predicted by a statistical law rather than a causal law". It should be noted that this rhetoric is designed to establish the "fundamental" nature of the way in which Ross and Kornbrekke treat the morphology results. This is the significance of the reference in M2 to "lawfulness" — even though the treatment remains statistical in keeping with the experimental data.[4]

The "alchemic" rhetoric of "created" and "disappeared" in M1 is transformed into a more mechanistic mode in M2: "retraction" replaces "disappears", and "extended as films" is substituted for "created"; and "*tend* toward coalescence of like liquids" becomes "*cause* the extended liquids to retract" (our emphases). In general, the rhetoric in M2 is more mechanistic and causal or deterministic. The same rhetorical tendency characterizes the transition from scientific practice to scientific exposition. There is no explicit reference, for example, to the fact that the morphology research involved shaking test tubes *by hand* in any of the three documents.

In M2, the words "shaking" and "agitation" are used synonymously. We find such phrases as "vigorously mixed", "thorough mixing", "vigorously shaken up and down", "mechanically conferred motion", and "externally applied agitation", but nothing about "manual-" or "hand-shaking". There is a clear effort in M2 to portray the shaking or agitation procedure as a mechanistic, controlled procedure. For example: (1) ''In the foregoing argument the mode and degree of mechanical agitation in creating the dispersion is taken to be invariable'', elsewhere in the sentence this phrase is taken from, the word ''shaking'' is modified by ''manual'' and the word ''agitation'' is modified by ''mechanical'', and this *might* lead readers to assume that the test tubes were shaken in appliances when they see the words ''mechanical agitation'' used to describe the shaking procedure; (2) ''To ensure that shaking produced each time a consistent and thorough degree of mixing, several precautions were maintained. The length of time and manner of shaking were kept uniform for all trials''; (3) the authors cite a 1939 paper in which Sasaki points out that dispersion type is affected by the mode of agitation;[5] they go on to list other factors that are known to affect dispersion type, and note that in the present case all the relevant variables are ''frozen'' and only volume fraction is allowed to vary; (4) ''Some variation in shaking technique and method of mixing was tried and gave the same result for the time of separation''.

The failure to specify that the test tubes were shaken by hand and the stress on the controls established to ensure uniform agitation (and replicability) are especially noteworthy because in the two instances in which we were present when the morphology research was presented to a scientific audience, physicists, chemists, and engineers raised strenuous objections to, and queries about, the shaking procedure. Thus, it appears that Ross and Kornbrekke enhanced — or believed they enhanced — the persuasiveness of their argument by not specifically referring to ''manual-'' or ''hand-shaking''. On the other hand, they might claim that ''insiders'' who read the paper will know that the test tubes were shaken by hand. In any case, the discussion of the shaking experiments is consistent with the general tendency to mechanize rhetoric in the movement from the initial to the later stages of scientific practice, and from scientific practice to formal expositions observed in this and other laboratory studies.

Understanding laboratory life

1. Reflections on life among the colloids and the chemists

We begin our analysis of our observations in the laboratory by reflecting on the problems of studying scientific practice, and on the way in which we were led to the conclusion (supported by the results of other studies of scientific practice) that contingencies are constitutive of scientific practice and products.

Orienting ourselves to Ross' laboratory and the morphology research was a difficult task. Part of the difficulty involved finding a point of entry, something we could identify roughly as the "beginning" of a particular phase of the laboratory work. As we interviewed Ross and Kornbrekke, we found that, in their view, research is a rather continuous process punctuated by the arrival of a new "man" in the lab, or of a new grant, or of other factors moving from outside to inside the laboratory. Thus there seemed to be two overlapping histories: one, a continuous story at the level of ideas and experiments; the other, a story characterized by the rhythms of people starting and finishing projects in the lab, and by the pushes and pulls of the funding cycle.

Initially, we attempted to establish a point of reference by focussing on experimentally and theoretically anomalous aspects of the research in progress. This gave us something around which we could try to organize our own research, something on which to hang a narrative. However, things were not to work out so simply.

As we grew more accustomed to laboratory life, we found that the importance attached to anomalous features of the research process varied greatly. Sometimes a theoretically perplexing situation was stressed because publication would be impossible unless the difficulty could be resolved. But if the experimental work was going well, there was a tendency to argue that the data were intriniscally interesting and should be published without theoretical embroidery. The ebb and flow of judgements about the significance of anomalies made it extremely difficult for us to construct a sort of one-dimensional narrative. The day-to-day, ongoing work in the laboratory seemed to continually overflow simple categorizations. This work appeared as a large series of responses to imposed demands, perceived needs, given conditions, etc. — including:

(1) The background knowledge and experience of the researcher; what courses he/she has taken; what journal articles he/she

reads and remembers; what equipment he/she is familiar with; what experimental techniques he/she knows; how cautious he/she is about materials; how methodical he/she is; whether he/she is theoretically or experimentally oriented; whether he/she does exhaustive literature searches before beginning an experiment; how creative he/she is at experimental design; how patient he/she is with delicate apparatus; how open he/she is to new phenomena; how secure he/she is in terms of ability and position; how willing he/she is to make his/her mistakes known to others; how open he/she is to advice from others...

(2) what equipment is available, and how much it costs; the possibilities for modifying given instruments, and the costs involved...

(3) the style of the laboratory and the way it is organized for research; available funds and attitudes about how they should be spent; contractual obligations with funding agencies ("ropes attached to research funding", in Kornbrekke's words); the pressure on PhD candidates to finish quickly; the pressure to choose research which is fundable and likely to lead to many job prospects; the pressure to be productive...

(4) the respect people have for each other's judgements; the roles various people play in the research process and in the university community; the form that communication takes depending on who is involved; the biases researchers have about ideals in science and education...

A multitude of contingencies have to be continually managed. As our work proceeded and our involvement intensified, we saw these contingencies increase and form an interlocking structure, a structure which was inseparable from "the research itself".

By focussing initially on anomalies, our research became an anomaly! We had been proceeding as if the "real research process" (which we at first identified with the discovery and analysis of certain anomalies) could be separated from the contingencies. As our work progressed, we experienced a shift in conceptual gestalt — the contingencies were *constitutive* of the research process. They continually *problematized* research, in such an intimate way that we had to abandon any vestigial ideas we had about "externalities".

2. *Contingencies and science*

The concept of "contingencies", the more complex concept of "contingencies as constitutive of scientific practice and products", and the general notion of "the social construction of scientific facts" are all being used in laboratory life research and in science studies generally. We believe these are useful ideas; however, a great deal of work remains to be done in order to clarify these ideas and determine whether, to what degree, and in what circles in science studies consensus about the meaning and usage of these ideas can be achieved. It would be premature for us to try to resolve these difficulties, and presumptuous of us to try to construct a theory or model of scientific practice based on the ideas of contingency, constitutiveness, and constructivism. More modestly, our objective in this section is to offer some remarks as a contribution to the discussion and debate about contingencies and science. Our remarks are general, but we offer them as conjectures not as generalizations applicable to all of science or as prescriptions for "good" science.

One of the difficulties we face in trying to convey the way in which laboratory workers create, elicit, and incorporate contingencies, and the way scientific products or results embody contingencies, is that when they are simply enumerated contingencies seem quite unremarkable. It is not surprising that certain instruments are unavailable, break down, or need to be drastically modified. Nor is it surprising that literature searches, professional meetings, and luncheon discussions sometimes lead to ideas which structure ongoing research in important ways. Taken singly, or in a list, such things seem to be discrete events, the inevitable "asides" of any directed action in the "real" world. The point is, however, that the research process is *not* primarily determined by "nature" or "physical reality". Scientific knowledge is created out of available resources — including formal and informal modes of communication, and instrumentation (Mulkay, 1979, pp. 60-61). In the deepest sense, the available resources in a given laboratory refer to the researchers' capacities for creative and critical thought, persuasion, communication, conflict, and cooperation. The indeterminacy of scientific criteria, the "looseness" of laboratory research, provide room for the exercise of those capacities. It is not as if a determinate path to some piece of information pre-existed and the researchers are deflected toward or away from that path by various sorts of perturbations. Rather, there is always a context, an inherited, assigned, or constructed problem situation which must be continually problematized and kept in motion. This flux (change without directionality) can be maintained and eventually given direction by the creative use of relatively forced choices and judicious selections from among relatively free choices (i.e., where alternatives are available). We have then a kind of evolution from randomness, the building of an organism — the research process — from an environment which the organism itself helps to create while it itself is growing and transforming. In this evolution, the discrete events called contingencies lose their contingent aspect because of a complex implication, literally an enfolding, which makes them *constitutive of*, and not *external influences on*, research.

The research environment the research process helps to create is the environment of the immediate problem-context and it is structured primarily by the instrumentation and first order mediation of informal dialogue among the researchers. There is also a more remote and subtle background environment which is formed by such things as the social structure of the laboratory, the style of the researchers, the respect people in the laboratory have for each other's judgements, the reputation of the laboratory outside the university, and the relationship of the laboratory to the larger university community.

For the most part, this more general environment functions as a background. The research process does not *usually* make use of this domain to problematize things, nor is it *likely* to lead to relatively forced choices. We have then a sort of figure/ground structure but a complex one in that the figuration (the research process) operating with a general ground creates and maintains itself from a more immediate ground which it helps to create.

Having said this we must immediately stress the dynamical features of this characterization. Figure/ground have, unfortunately, connotations of fixity and definite boundaries. We do not intend those connotations here. The figure/ground structure itself may be radically altered during the flux of research.

There is, then, a constantly shifting set of relevancy systems which alter the figure/ground structure of the research process (cf. Gurwitsch, 1974, p. 121). The remote comes near and the near recedes. Thus while one might be tempted to identify levels of, or a hierarchy of, contingencies (instrumental, conceptual, psychological, and sociological), the dynamics of the situation we experienced in the laboratory suggest that this would be a fruitless approach. Which choices, relatively free or forced, will be most significant for the research process can not be determined *a priori*. In studies such as this, the social investigators *are themselves* contingencies; with or without the help of the laboratory scientists, they *introduce* contingencies. These contingencies affect how the social investigators view the role of contingencies in science. The social construction of stories about laboratory life involves negotiations between social investigators and laboratory scientists; stories are not constructed in an "objectified", "detached" setting and manner. Recognizing the complexities involved in the interactions among contingencies and in the negotiations out of which sociological or ethnographic accounts of laboratory life emerge can help to problematize the *social* research process. Ruminating on these issues led us to consider the probabilistic figure/ground model we sketched in this section. It seems clear, furthermore, that many of the things we have said about laboratory work apply reflexively to our study.

Denouement: Indra's net

The Hindu god Indra inhabits a celestial palace that is covered by a network of jewels arranged so that by looking at any one you can see all the others reflected in it. We were reminded of Indra's net when we reflected on various ways in which our study reflected the morphology study — and even the morphology phenomenon. The *Scientific American* article that Zenzen brought to the chemists' attention, for example, appears to have implications for our research too. An often neglected aspect of natural systems is that in order to study them the theorist must be able to isolate some limited range of length scales (Wilson, 1979, p. 158):

> ...events distinguished by a great disparity in size have little influence on one another; they do not communicate, and so the phenomena associated with each scale can be treated independently. The interaction of two adjacent water molecules is much the same whether the molecules are in the Pacific Ocean or in a teapot. What is equally important, an ocean wave can be described quite accurately as a disturbance of a continuous fluid, ignoring entirely the molecular structure.

Thus, the causal analysis of multi-scaled complex phenomena presupposes a decomposition into levels which can be treated as effectively non-interactive. When we use categories such as "new graduate student'', ''experienced'' or ''older graduate student'', ''researcher'', ''NASA'', ''resident theoretician'', and so on, we already are performing a decomposition of the multi-scaled process called scientific research. Now it may be that such a decomposition is significant, but what should be noted is that the degree of success one has in developing a causal analysis of the phenomena associated with each scale will depend on the degree to which these scales can be treated as if they were non-interactive.

We see no *a priori* justification for interpreting the social production and construction of knowledge in the restricted manner necessitated by a commitment to causal analysis. This is not the way to make the sociology or ethnography of science ''scientific''; the success of causal analysis in *natural* science depends on assumptions about non-interaction among levels of phenomena.[6] The appropriate ''scientific'' approach to science studies may be to *research* the question of interactions and leave open the question of scales. Various decompositions are possible in studying science, and it is probably wise to avoid the naive assumption that the obvious and the easiest decompositions are necessarily the most important ones or the crucial operative factors in the scientific research process. We ourselves may have fallen into some methodological and interpretative traps in this study for failing to take account of problems of decomposing phenomena.

We seem to have been in a position relative to the morphology project somewhat like that of the chemists relative to the morphology phenomena. Like Kornbrekke, we had to learn to ''*see* changes of morphology''. The clearest instance of this isomorphism occurred when we analyzed the three documents. Learning to see the changes in the papers was like learning to see the changes of morphology in the test tubes. There appears to be a reciprocity between some of the major conceptual features of the physical system at the core of the morphology project, the structure of the laboratory we studied, and the way in which we have come to think about scientific research in the course of this study. Perhaps we can say that the social production and construction of natural scientific knowledge is mirrored by the scientific production and construction of social scientific knowledge. Such a reciprocal animation of ''natural'' and ''social'' is quite harmonious with the view that foreground and background can reverse their roles.

Conclusion

In general, the results of our research are consistent with the "constructivist interpretation" of science associated with the recent research on laboratory life, and some other areas of science studies. The laboratory studies suggest that "scientific objects are *produced* and *reproduced* at the sites of scientific action". They converge on a view of scientific objects as socially situated, contingent, discursive accomplishments (Knorr, 1981, pp. 4-5). This conclusion does not necessarily support radically relativistic sociologies of knowledge. The social construction of scientific facts can be conceived, following Fleck (1979, p. 100), as events in the history of ideas, stylized by contemporary, local, social, cultural, and environmental factors (cf. Spengler, 1926, p. 59). Neither Latour and Woolgar nor Knorr, for example, adopt relativism. They do not deny that in some sense facts exist; there *is* a recalcitrant reality. Latour and Woolgar (1979, p. 180) argue, however, that the notion of a reality-out-there "is a *consequence* of scientific work rather than its cause". And Knorr (1979, p. 369) writes:

> A constructivist interpretation of knowledge is not to be confused with an idealist ontology: I do not maintain that reality is produced (constructed) in the sense that its appearance has no independent existence. Rather, this approach claims that once we see scientific products as selectively carved out, transformed and constructed from whatever is, we will also see that there cannot be any warrant in the claim that we have somehow captured (subject to progressive improvement) what is.

Perhaps the most important and controversial idea which has emerged in contemporary science studies, especially among students of laboratory life, is that social constructions are *constitutive* of truths, facts, and scientific knowledge — and of knowledge in general. The idea remains primitive and elusive. Clearly, there is an intention here of moving beyond traditional notions of social construction in the sociology of knowledge, and beyond the idea that science is *mediated* by society, or "externalities". It remains for future studies of scientific practice to establish whether sociologists of science are indeed, as we believe, engaged in a process which will radically alter our views on the nature of science by transcending traditional distinctions between "external" and "internal", "objective" and "subjective", and "reality-out-there" and "social constructions of reality".[7]

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Notes

1. See, for example, Latour and Woolgar (1979); Knorr (1980); and Lynch (1981). For a critical introduction to this literature see Knorr (1981).
2. On contingencies and scientific research see Dean (1979, p. 212); Barnes (1977, p. 54); and R. Collins (1975, p. 496).
3. The Japanese film 'Rashomon' deals with various versions of "the truth" regarding an attack, a rape, and a robbery: see Richie (1972); cf. Latour and Woolgar (1979, p. 257); and H. Collins (1975, p. 205).
4. We do not mean to imply that statistical results are not "lawful"; see, for example, Bohm (1971, pp. 28-32). In this case, Ross seemed satisfied with *reporting* a statistical result. In the end, he and Kornbrekke were led to present this result in a theoretically-grounded manner that transformed "result" into "law".
5. Sasaki (1939), incidentally, discusses experiments in which test tubes are shaken by hand and describes two modes of manual shaking. This paper was not discovered until after the morphology experiments had been carried out.
6. Bloor (1976) advocates a causal sociology of science that ignores this point.
7. The citation for M2 is as follows: Ross, S. and Kornbrekke, R., "Change of morphology of a liquid-liquid dispersion as a stochastic process", *Journal of Colloid and Interface Science* 81 (1), pp. 58-68.

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