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Decisions, Consequences and Public Explanations: The Relationship between Research Activity and Formal Knowledge in an Ecological Laboratory.

TITLE OF THESIS/TITRE DE LA THÈSE

Simon Fraser University

UNIVERSITY/UNIVERSITÉ

Master of Arts (Communication)

DEGREE FOR WHICH THESIS WAS PRESENTED/GRADY POUR LEQUEL CETTE THÈSE FUT PRÉSENTÉE

1983

THIS DEGREE Conferred/ANNÉE D'OBTENTION DE CE GRADe

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DECISIONS, CONSEQUENCES AND PUBLIC EXPLANATIONS: 
THE RELATIONSHIP BETWEEN RESEARCH ACTIVITY AND 
FORMAL KNOWLEDGE IN AN ECOLOGICAL LABORATORY.

by

DOUGLAS ALBERT HUGH MCKEGNEY

B.Sc., Simon Fraser University, 1976.

A THESIS SUBMITTED IN PARTIAL FULFILLMENT OF 
THE REQUIREMENTS FOR THE DEGREE OF 
MASTER OF ARTS (COMMUNICATION)
in the Department

of

Communication

DOUGLAS ALBERT HUGH MCKEGNEY

SIMON FRASER UNIVERSITY

JULY 1982

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Decisions, Consequences and Public Explanations:

the Relationship between Research Activity and

Formal Knowledge in an Ecological Laboratory

Author: Douglas McKeagney

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January 31, 1983

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ABSTRACT

This thesis reports a two year ethnographic investigation of university based ecologists who studied the nutrition and reproduction of captive deer. Trained in biology myself, I worked for them as a research technician to become familiar with how they organized, conducted and explained their experiments. I contend that the origins, substantive claims and validity of scientific knowledge can be explained by examining the research process first hand and then re-enacting and analysing it in the appropriate narrative form.

"The research process" includes: the construction of equipment, facilities and experimental routines; personal relationships among group members; discussions in which members organized and explained their work; dealings with universities, corporations, governmental agencies and professional journals; deer; the use and production of research reports. I recreate the special laboratory world these ecologists built, and portray them working there. I focus on them assessing their situation, deciding what to do, doing it and dealing with the consequences of their, and the deer's, actions. They developed formal characterizations of deer in the course of their attempts, lasting many months, to establish and maintain the order and continuity of their work.

My approach contradicts the conception of science that natural and social scientists reproduce when representing their work in research reports. When writing, they must re-enact their
multifaceted, evolving activities in a positivistic narrative form, thus transforming research into Science, the perception and analysis, by the mind, of a static, external reality. This also happens when scientists converse. These narratives are their knowledge, and are histories of their research. These narratives eliminate the scientists' active participation in their research from their own histories of it, but are integral to that research and necessary to the scientists' professional survival.

This intrinsic contradiction has two fundamental consequences for my thesis. First, I portray the ecologists' way of understanding deer without accepting it as it stands, by alternating between novelistic recreation of their world, and critical analysis of their discourse. Secondly, my methodology matches my active/critical portrayal of the ecologists' inquiries, stressing my involvement with these scientists and the evolution of my questions about their work.
ACKNOWLEDGEMENTS

It is only fitting that the acknowledgements come at the beginning of a book. This work could not have been done without the cooperation and support of many people. I am extremely grateful to the people who generously allowed me to study their research. I can name them only by their pseudonyms, for their privacy should be respected: Roger Williamson, David Simpson, Elaine Garfield, Karl Voelker, Kirstin Salsky, Michael MacDonald, Victor Rosen and Don Dempster. I also owe a large debt to my first thesis supervisor, Frederick John Brown, who let me begin what at the time must have seemed a rather unlikely undertaking. Equally, I am grateful to Robert S. Anderson, who took over supervisory duties when Fred Brown retired. It must be said that Bob was strikingly patient. He gave good advice when I needed it, carrying out his role with subtlety and good effect, and I owe to him the opportunity to write this thesis in the manner which the subject called for. Paul Heyer and Steve Straker gave useful comments on the manuscript, as did Philip Moore, Simon Foulds, and Patricia McKegney. I am grateful to Lynne Hissey and Patricia McKegney for their careful proofreading. Margaret McKegney gave me invaluable assistance with all of the figures and illustrations. The faculty and students of the Department of Communication of Simon Fraser University, through the combination of their interests and skills, provided an ideal intellectual environment for my work, and Karen Gardner, the departmental secretary, was extremely
helpful to me. Harriet Palladown encouraged me in numberless ways, commented on the manuscript and, with the able assistance of her children, kept me sane. Madeleine Chisholm provided invaluable support and like my other friends showed a remarkable tolerance for my eccentricities. I typed this thing myself.
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I. The portrayal and analysis of scientific research

Research

Suppose we take commercial cattle feed, blood sampling vacutainers, scintillation counters and some established facts about nutrition and reproduction, and mix them together with hunting regulations, the funding policies of the National Science and Engineering Research Council of Canada, captive deer, the requirements of professional career development, the narrative form of public justification, the current technical literature, and a small handful of people, their conversations and relationships with one another, their experimental routines and other physical activities and the politics of their profession. What we get is a random assortment of the bits and pieces of the everyday working world of a small group of animal ecologists. They did research on the nutrition and reproduction of captive deer at a university research forest in British Columbia, Canada.

Their world was very diverse, an uneasy combination of disparate things, and you may rest assured that these various fragments did not spontaneously form themselves into workable research projects. My ecologist friends were obliged to weave them together into a more or less coherent fabric of research,
and to rework and maintain that over a period of years. This was their task, repeated, with variations, every day. Their activities and thoughts, their collaboration with one another, and the place they established for themselves within the university, formed a focal point for all these things. This focus moved through time, constantly changing and being changed.

They built a cultural outpost of animal ecology within the university, and in the course of trying to keep it together and on track through all of the flux of everyday work, they produced formal, rationalized, and apparently immutable knowledge about deer. This eventually appeared as published research reports, which they wrote for professional scientific journals in the hope that their colleagues would read these reports and take them seriously.

The purpose of my thesis is to show how these scientists accomplished all of this, and to show what their resulting knowledge is about. In the text which follows, I re-create the special milieu these people built for themselves, portray them at work in it for a period of eight months, and through a combination of portrayal and critical analysis, explain how they produced formal scientific knowledge in the course of doing their work.

This thesis is based on a participant-observation study. The group I studied consisted of a full professor whom I shall call Dr. Roger Williamson and his graduate students and technicians. They worked in the biology department of Western
University. In 1977 and 1978 I worked with these people as a research technician, studying them as they studied their deer, and studying the deer myself. My undergraduate training had been in biology and physics and I used this background and my technician's role to become as involved as possible with their research activities so that I could document the ways they developed their experiments and analyses during the two years I worked with them.

My task in writing this thesis, if I may rephrase my intentions, was to compose a narrative in which I could resolve the diverse parts of the scientists' working world, which I had learned to see while I was doing my field work, into a coherent representation of how they produced formal knowledge through their transactions with the deer, their equipment, one another, and their profession. I therefore wish to show what determined the so-called "content of their knowledge".

I have chosen to show how all the things I mentioned at the beginning fit together by re-enacting, in story form, the work that Williamson and the others did to put them together, and keep them together. I emphasize how these researchers assessed their situation, decided what to do, did it, and then dealt with the consequences of what they—and their deer—had done. This was normal daily activity, and I am concerned to portray it in some detail. I also stress very strongly that a major research

---

1 The names of the university and all the people I worked with have been changed.
project takes years to complete. Therefore, in somewhat the same manner as a novelist, I recreate a series of incidents and situations, in order to portray the developing action of the research. In this way, I hope to avoid the reductionist trap of focussing very narrowly on one very limited event, and the opposite problem of standing back so far that no particulars stand out.

If one is going to understand the research process, one must see how it is conducted, directed and developed over a long period of time by the people who come to work every day to do that research. It is my hope that in the course of portraying this, I shall also be able to reconstruct the production of formal accounts of the nature of deer—for trying to understand these animals scientifically was an integral part of the way these people organized their research, made sense of its daily chaos, and planned its course.

A major theme of this thesis is therefore how Williamson and his co-workers maintained the continuity and rationality of their research in the face of disruption and change. This is not an ethnomethodological treatise, however. When I refer to the creation of rationality, I do not mean that I shall minutely analyse the conversations of my scientist friends. It is rather the full range of their attempts to direct and regulate their laboratory experience, to keep it under control for months at a time, that interest me. I shall dwell upon the rationally reconstructed, routinized behavior they enacted, the patterns of
authority they developed within the group, and the physical
controls they imposed upon the deer and themselves, to name only
three areas of concern. The intellectual order of their graphs,
data tables and conversations are only one part of the
artificial system I am considering here.

I shall try to re-create the complex relationships among
these kinds of order and the endless barrage of unexpected
events that repeatedly forced Williamson and his students to
rework their circumstances and their behavior, and re-negotiate
their understanding of what was going on around them. Most
importantly, I wish to show how the behavior of their deer and
the requirements of their profession shaped the orderly patterns
these people developed, which gave structure and stability to
their little world. Because the most stable things these people
produced, in relation to both the deer and their profession,
were the public accounts of their research which they published,
the papers they wrote are an important consideration here.

A crucial part of my argument is that formal written
accounts were not only a public affair. There is an intimate
relationship between local action and public representation in
scientific research. The scientists I worked with often
explained the daily events of their experiments to one another
in much the same way they would explain it to a public audience.
Writing informed and stabilized much of the supposedly local and
informal practice of the lab.
Therefore, my representation and analysis of research includes a heavy emphasis on the relationship between the affairs of the group, and the world outside the lab, examining how these people incorporated external influences into their local scene and organized their work with outside audiences in mind. The kinds of external influences I have considered are exemplified by the officials of the Fish and Wildlife Branch of the Ministry of the Environment, who allowed them to catch deer and funded their research, and the colleagues who provided useful information. Although my work has a marked political cast, I specifically chose to study a scientist whose work had not been caught up in public policy hearings or a polemic dispute within the profession. I did not want the obvious pressures such situations would include to obscure my argument: that the origins, validity, and substantive claims of formal scientific knowledge can be understood through an extended examination of the research process. What scientists know is a consequence of what they do. It follows from their involvement with their subject-matter, and the way they organize and conduct their activities as members of modern industrial societies or their imitators. I contend that these activities can be studied, and the production of knowledge explained by re-enacting and analysing research in the appropriate narrative form.
II. Logical empiricism and the politics of public representation

In the preceding pages I have summarized a way of telling the story of research. Much of the significance of my proposal lies in its contrast with another view of the technical and intellectual accomplishments of scientists, which my view must both answer to and subsume. I am referring to the image of Science-as-Knowledge which lies at the heart of the positivistic philosophical tradition. This tradition has been reworked with numberless variations by philosophers and the methodologists of social science, but that is not the form in which I shall consider it here. I shall instead discuss how this philosophical tradition is enacted and reproduced by natural and social scientists. Specifically, I shall focus on the image, Science, which they create when they write and discuss the papers about their research which they are obliged to publish in professional journals or circulate as in-house reports if they are to be recognized as members of their profession.

By starting this way, I shall achieve two ends. Firstly, this will begin my introduction to the working world of Dr. Williamson and his students, for public representation of one's research is a vitally important part of any scientist's professional life. The effects of this kind of public justification reach down very deeply into the daily conduct of research, as well as dominating the scientist's activities.
outside of the lab. Secondly, this entrenchment of the
positivistic view of Science (or any of its cousins) in the
formal communication system of the sciences was the starting
point of my inquiries. My experiences with it as an
undergraduate student of biology and physics provided the
tension, the sense of there being questions that should be
asked, that impelled me to study research. Therefore, a
discussion of this entrenchment will best enable me to explain
why I did my field work and wrote my thesis as I have done.

The politics of public representation are the backdrop
against which I shall now flesh out and establish the
significance of the question I posed in chapter One: what did
these ecologists actually do, and how did their activities
eventuate in formal public knowledge about deer? The problem
will be established in terms of the way that scientists and
social scientists are constrained to communicate with one
another as a precondition of their membership in their
profession. I shall not try to establish the question in terms
of current theoretical debates about, or empirical sociological
investigations of, science, although I obviously share some
concerns with these. The following pages should make clear why I
have chosen this course.

I shall first provide a brief summary of the philosophy of
logical empiricism. This is not meant to be an assessment of the
work of professional philosophers. Rather it is intended to lay
out the basic features of the positivistic image of Science that
is conveyed by textbooks and professional publications. This is the everyday positivistic world view of the professional who must provide a public justification of his work. In this case, I am referring to the rather limited public made up of a scientist's peers.

Throughout this chapter I distinguish between 'Science' and 'science', and between 'Scientist' and 'scientist'. The terms 'Science' and 'Scientist' refer to the positivistic image of the scientific profession that scientists create when they write professional research reports, textbooks and popular articles. When I use 'science' and 'scientist', it will be to refer to all and any of the events, activities, circumstances and the like which constitute the research process. I gave a short list of these at the beginning of Chapter One. Because one of the things that scientists do quite frequently is reconceptualize their research as Science and themselves as Scientists, the distinction between the two sets of terms can be quite subtle.

As I switch from one set to the other, there may be some confusion. This confusion arises because we normally think about scientific knowledge only in terms of the positivistic image. There is no established tradition of investigating the research process as such. It has been a non-topic, and methodological discussions have in fact been discussions of Science, not of research. This is a basic source of confusion in our traditional way of understanding the scientific profession, and resolving this confusion is a central purpose of this thesis. I switch
from one set of terms to the other in order to avoid falling into that confusion, and this tactic also has the effect of pointing out the way we customarily treat science and research as equivalent terms, which they are not. Making an effective distinction between the two is one of the main purposes of this work, and this is the reason that I take the time to define Science.

Once I have outlined this rather simple variant of empiricism, I shall show how professional publications in the natural sciences, and much of the social sciences, embody this philosophy in their style and narrative form. Then I shall consider the pressures that public debate places on scientists, and why it is to their advantage to formulate their explanations of their work in this positivistic form. This will, I hope, explain the tenacity of the average scientist's commitment to the empiricist world view, even though he knows what it leaves out. Then I shall summarize the consequences of this form of public explanation for a person who wishes to study the scientific research process.

The orthodox view of Science

According to this view, Science is most importantly an

'This section is largely based on Michael Mulkay's summary of what he calls 'the standard view of science', in Science and the Sociology of Knowledge. (1979) London: George Allen and Unwin. pp. 19-21. I have chosen the term 'orthodox', instead of 'standard' because this better conveys the political character of its entrenchment.
abstract structure, a formal system of ideas. This system rests upon the facts, which occur in the world, independently of anything people do. Just as the facts are part of the real world, so they are also part of Science. The Scientist observes them directly and expresses them, unchanged, as mathematical laws of nature which summarize the data of his senses. These lawful expressions constitute the fundamental ideas of Science. There is, therefore, a straightforward, unproblematic correspondence between the empirical laws of Science and the real world. The true objective world is determinant and invariant, that is, the various parts of the world are eternal and interact with one another in mathematically predictable ways forming a unified, symmetrical whole which underlies the changes and flux of the phenomenal world we live in. Just as the real world forms a unity, so does Science. The correspondence of its empirical laws to reality is guaranteed by formal experimental criteria and rules of evidence that are used by all Scientists.

Upon the foundation provided by empirical laws are erected theoretical laws in a hierarchy of increasing abstraction. They provide order among the empirical laws and help to explain them. An example of a theoretical law is the wave theory of light or its traditional competitor the corpuscular theory, which Isaac Newton favoured. For Newton, the laws of reflection and refraction, according to which mirrors and lenses work, could best be explained by assuming that light consists of a stream of tiny particles. During the nineteenth century this was replaced
by the wave theory, and with the triumph of quantum mechanics in
the 1920's the wave/particle dualism of both light and matter
was firmly entrenched. Such theories came and went but the
established facts, such as the laws of reflection, were
unaltered, although new ones accumulated. If theories were
contradicted by new facts, they were discarded and replaced.
Scientists could choose among competing theories by comparing
their predictions with the facts. For empirical laws, that is,
facts, to stand as an independent test of theoretical laws, they
must be nothing more than summaries of the data of observation
of the real world, unbiased by the scientist's theoretical, or
other, commitments. Nevertheless, there is a logical
relationship of explanation between them, in which a fact or an
observation is considered to be explained when it is established
to be an instance of a more general law.²

This form of positivism is concerned with formal written
discourse, with the finished products of inquiry as they appear
in professional journals and other publications. Questions about
the validity of knowledge, as they are dealt with in this
philosophical tradition, typically result in a dissection of the
alleged formal logical relationships between facts and theories.
The logical framework of this process of explanation and

---
¹Giddens, Anthony (1978) "Positivism and its critics". In A
History of Sociological Analysis. T. B. Bottomore and R. Nisbet,
justification is called the Scientific Method. That is to say, the Scientific Method is a form of logical analysis of existing data. It is more a form of justification than a prescription for how to do research.

A person who has questions about the origins of knowledge, or who is skeptical of scientists' ability to know what the world is really like, is directed to the mind, "as a special subject of study, located in inner space, containing elements or processes which make knowledge possible." He or she most certainly is not directed to examine the ordinary daily affairs of scientists as they do research, in order to account for what they know. Any discussion of the Scientist as an active, credible human being is considered to be irrelevant to the truth of the knowledge he professes to have. Therefore, social and moral values, and political ends, are not a proper part of Science itself. It would be wrong to suggest that scientists are not concerned with moral or political issues. Frequently they are, but their discussion of them does not normally question the autonomy of scientific knowledge, but rather presupposes it, confirms it. Typically, when scientists discuss


morality or political issues in relation to science, the
empiricist notion of science, of free inquiry, provides the
foundation of their comments or an ideal to which they aspire.

The most important fundamental characteristic of the world
view I have been describing is mentalism, the very widely held
belief that knowing is to be understood in terms of the internal
processes of the mind. It is basic to many schools of thought
besides logical empiricism. The prevalence and consequences of
this belief, which are crucial to my argument, were succinctly
stated by the American philosopher John Dewey:

Special theories of knowledge differ enormously
from one another... Some theories ascribe the ultimate
test of knowledge to impressions passively received,
forced upon us whether we will or no. Others ascribe the
guarantee of knowledge to synthetic activity of the
intellect. Idealistic theories hold that the mind and
the object known are ultimately one; realistic doctrines
reduce knowledge to awareness of what exists
independently, and so on. But they all make one common
assumption. They all hold that the operation of inquiry
excludes any element of practical activity that enters
into the construction of the object known. Strangely
enough this is as true of idealism as of realism, of
theories of synthetic activity as those of passive
receptivity. For according to them, "mind" constructs
the known object not in any observable way, or by means
of practical overt acts having a temporal quality, but
by some occult internal operation.

The common essence of all these theories, in short,
is that what is known is antecedent to the mental act of
observation and inquiry, and is totally unaffected by
these acts: otherwise it would not be fixed and
unchangeable.6

This is the crux of the matter. Logical empiricism is one
of a host of philosophies that tries to reconcile two

irreconcilable things. One, the knowledge the scientist acquires must be a true representation of the real world. This is to say, it must not be tainted by the scientist, it must represent the world as it is when the scientist is not investigating it. Two, this knowledge is the scientist's property, and for that claim to hold true, he must have done something to acquire it. It could not have come to him purely randomly or spontaneously, or any person could acquire such knowledge, without special training or special effort. If that is true, then why is it in fact so very difficult to understand the world? Moreover, if it is true, why should we respect scientists or any other knowledgeable persons, and heed their words?

This dilemma is traditionally resolved in the following way. Inquiry is collapsed into a mental act. Thereby it loses its overt, active character. At the same time "reality" is set apart from the knower. It must be immutable, unaffected by our attempts to know it, and perceiveable only in the mind's eye. The history of inquiry is replaced by a rational reconstruction of the logical development of ideas, one which leads inevitably to the view we presently hold.

Let me provide an example of what is lost when people collapse the process of inquiry into a mental act, into "having an idea". I take this example from Bruno Latour's and Steve Woolgar's book, Laboratory Life.\(^7\) a participant-observation

study of neuroendocrinological research. A scientist named Slovik developed an assay (a way of testing for the presence of a chemical) that worked in some laboratories, and not in others. Latour was told that one day Slovik got the idea (Latour's and Woolgar's emphasis) that the results varied from place to place because in some towns the water contained relatively large concentrations of selenium. Selenium is a metal, a trace element that is important in nutrition.

Latour and Woolgar did not accept that "Slovik had an idea". From Latour's field notes, they constructed a very different history of this "discovery". It turns out that a graduate student named Sara had studied selenium in order to fill a compulsory university requirement that she take some courses in a field outside of her own specialty. She subsequently spoke about selenium research at one of a series of informal seminars on outside interests that were a regular feature of her research group activities. At this seminar she mentioned that someone on campus had suggested there might be a connection between the high selenium concentration in the ground water in some areas and the relatively high incidence of certain kinds of cancer in those areas. Slovik attended this seminar, and he took up the suggestion, applying it to his strangely irregular assay. Later he telephoned one of the labs where the assay did not work and asked the people there to test the selenium content of the water. Such checks indicated that the assay did not work in places where the water contained
relatively large amounts of selenium.

Latour and Woolgar point out that these two ways of accounting for the group's members' understanding of selenium's role in the assay are extremely different. The lab member's account focuses on an individual having an idea. Latour and Woolgar direct attention to group traditions, discussions, university requirements and status within the group, in addition to individual acts of reasoning and imagination, and the consequences of experiments. The version told in the lab gave all of the credit to Slovik, who has higher status than Sara, the other diffuse it. The version told in the lab essentially eliminates the inquiry process, and the conditions under which the work was done, collapsing inquiry into a mental act, and leaving behind the reality which was reflected in Slovik's mind. It should be apparent from this example that learning to see research will profoundly change one's view of the relationship between what scientists do and what they profess to know.

The form and style of public scientific discourse

It is currently fashionable to believe that we have moved into a post-positivistic era in philosophy and methodology, and so some people might take exception to my beginning with an account of logical empiricism. Why not, they might suggest, root your questions in a consideration of the debates currently
carried on by sociologists, historians, and philosophers, who are trying to come to grips with the nature of science? My answer is that the people who ask this question are professional scholars; and as a routine part of the politics of their profession, they not uncommonly try to claim that they are Scientists themselves. This creates a contradiction in the position of the social scientist who wishes to understand research, and to sidestep this contradiction as best I can, I take the communication system of the academic profession as my starting point.

A quick glance through the professional journals in which such debates are conducted—for instance The American Sociological Review, Social Studies of Science and Social Science Information—reveals that whatever kind of methodological schemes social scientists may be trying to devise, it is simply the case that they, like natural scientists, usually must publish in a narrative form and a literary style that reproduces the world view of logical empiricism or one of its mentalistic relatives. This is where the real philosophical commitments of natural scientists and most social scientists are to be found, and it is these commitments which must therefore provide the starting point for our inquiries into the character of scientists' activities and the kind of account that can re-enact and analyse research adequately. Accordingly, I shall now consider the way in which professional journal articles must be written.
In order to be credible, to get a hearing from his fellows, the scientist must publish in a recognized journal, and to get his work published he must write in a rigidly defined standard format. The format is quite simple. First there is an introductory section in which the author sets the stage for his question by referring to studies previously done on topics related to his. These references establish that there are some questions that need to be answered, a gap in existing knowledge that should be filled. The question is then posed, and the author proceeds to give an extremely compressed account of the experimental procedures and equipment that he used in pursuit of it. There is a legend abroad in the land that this materials and methods section is put in the scientific paper to enable a knowledgeable reader to repeat the experiment and verify or refute its results. For reasons that will become apparent soon enough, I do not accept this view. After the materials and methods come the results section, in which the experimental data are displayed, and the discussion, in which they are analysed.

All of the scientists I know are aware that this way of representing science leaves out a great deal. Yet it has a very powerful hold on their thinking, it is one of the sacred cows of their profession. Like all sacred cows it has been satirized by

"The discussion which establishes the direction the paper will take covers up a very complex extended process in which the scientist comes to recognize a problem, the solution of which will advance his career. For a discussion of this, see K. D. Knorr (1977) "Producing and Reproducing Knowledge: Descriptive or Constructive?" Social Science Information, v. 16, #6, pp. 669-696."
some of its adherents. It is safest for a scientist to be very eminent before he takes such potshots, but fortunately we have these remarks from the Nobel Prize winning biologist Peter Medawar, who has a talent for invective which I find difficult to resist:

The section called 'results' consists of a stream of factual information in which it is considered extremely bad form to discuss the significance of the results you're getting. You have to pretend that your mind is, so to speak, a virgin receptacle; an empty vessel, for information which floods into it from the external world for no reason which you yourself have revealed. You reserve all appraisal of the scientific evidence until the "discussion" section, and in the discussion you adopt the ludicrous pretense of asking yourself if the information you've collected actually means anything; of asking yourself if any general truths are going to emerge from the contemplation of all the evidence you brandished in the section called 'results'.

Medawar claims that this format follows the precepts of John Stuart Mill's empiricist philosophy, which is an historical predecessor of logical empiricism. The scientist accumulates data, reflects upon them and inductively discovers what is out in the world. Just as empiricism separates facts from theories, the orthodox literary form of scientific publication separates "results" from "discussion".

I wish here to draw attention to a very important and subtle thing. The research report is not simply a statement of what is the case. It is written in a kind of narrative form, it is a rationally reconstructed history of the events of the research. Moreover, in composing this "history", the scientist

*Medawar, Peter (1963) "Is the scientific paper a fraud?" The Listener, December 13, p. 377.
portrays himself and his relationship to what he is studying, in accordance with the orthodox empiricist view of science. In order to get published, the scientist must take that view of his own work. This philosophy is thus well established as a hidden agenda in professional scientific writing.

But as Medawar points out, this systematically misrepresents research. As a result of taking this format too literally, many people have been misled about the nature of scientific inquiry, and science is widely misunderstood. Medawar claims that this erroneous impression could be corrected if scientists wrote up their work in accordance with the dictates of Karl Popper's philosophy of science. For this is the correct view of how science is done, and would set the record straight. Medawar is mistaken in this belief, but I shall leave that issue until Chapter Six. For the moment, let us examine this narrative form more closely.

One of the features of the idiom of discovery is that although there is an unfolding of a sort of action in the narrative, the author is strongly de-emphasized and his participation in the research becomes passive. This is made very

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10 Recently, sociological investigators have come to similar conclusions. Three works already cited, Knorr 1977, Latour and Woolgar 1979, and Mulkay 1979, bear on this question. I have used Medawar to introduce the point to underscore the fact that the orthodox account of science is not completely unquestioned by scientists themselves.

11 Note that I do not use the science/science distinction when I am summarizing the position of another author. I use it only when articulating my own view.
clear by Joseph Gusfield in his rhetorical analysis of scientific writing. He states that "(...) centrality of method and externality of data is the major key to the story (...) the paper tells...".12 The story involves a move away from the problem situation defined at the beginning of the paper to the resolution in the discussion, but it is the methods and the data that drive the narrative, not the scientist. In order to be trustworthy, the scientist must not have participated actively. He must not be seen to have determined the results, but only to have found them. He must be an observer, someone who has a hidden and unassuming posture. The author is given an identity by the line at the beginning of the paper where his institutional affiliation is noted, and by the acknowledgements section.

Elsewhere in the paper, the author chooses expressions that emphasize the externality of the source of action (the real world) and the passive character of the scientist. Gusfield quotes such phrases as these: "It was decided to use the latter method", "The test indicates that there is a significant difference", and "Data confirm a self-evident fact".13 The form and style of the paper are such that "the conclusion or result


13Gusfield, p. 20.
is portrayed as emerging from an external world of data." Woolgar has pointed out that the literary devices the scientist uses provide a path for the reader to follow, and this path leads inevitably to the discovery of something "out in the world". The role of the scientist in constructing the object "discovered" is eliminated.

I would argue that one of the most important features of the way reports are written is that there must not be any real change taking place in the narrative of the research report. It must be logically consistent. The question or problem situation which introduces the story must be an inverted form of its conclusions, or at least it must anticipate them. This produces the impression that the biologist William Wellington has noted, "Most research reports show investigators moving in such straight lines that one feels they must have known their conclusions before they obtained their results!" In effect, there must be no real action and as a result the Scientist who is working behind the scenes of this account is an incredible being. Instead of being an active participant who organizes the

14Gusfield, p. 20.

15Woolgar, Steve (1980) "Discovery: logic and sequence in a scientific text" (1), in The Social Process of Scientific Investigation, Knorr, K.D., R. Krohn, and R.D. Whitley, eds. Dordrecht: Reidel. In the case of Woolgar's paper it is important to note that he confines his analysis of the scientist's active role to the work of writing the paper. He does not deal with the research process itself, but only with the literary form of public representation.

research, directs its course, keeps it working and then writes it up, his relationship to the world is one of passive detachment, overcome by the power of his mind to apprehend reality. The ultimate impossibility of such a person is both reinforced and obscured by the impersonal style in which the report is written.

Gusfield argues that there is a rhetoric of science, that scientific papers persuade, and that the form and style of the paper are part of the process of persuasion:

...to be scientific is to exercise a definite form over the language in use, to write in a particular way which shows that the writer is "doing science". The writer must persuade the audience that the results of the research are not literature, are not a product of the style of presentation. The style of non-style is the style of science.  

My response to this argument is mixed. Gusfield's portrayal of the rhetoric of the scientific profession is accurate enough, but I think it is important to remember that scientists write this way because they have no choice. As Medawar has said, "If scientific papers are to be accepted for publication they must be written in the inductive style. The spirit of John Stuart Mill glares out of the eyes of every editor of a learned journal." How scientists would write if they had a free hand in the matter is an interesting question. Medawar's remarks indicate that at least some scientists are less than reverential.

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17 Gusfield, p. 17.

in their attitude toward the format of scientific publications, and I have heard many scientists complain that it is unrevealing. At one point, while trying to interpret a paper, Dr. Williamson turned to me and said, "They never tell you how they did it!"

All scientists know that while a very good paper will be written in this way, so will a great many very bad or irrelevant ones. The standard format means that the author has gotten his work past the editors of the journal, but really, who has not done that sooner or later? I think it is more likely that proper format is a negative criterion. Its absence means that the article was not published in a reputable journal, and work that is not routed through conventional channels usually does not get a hearing.

Furthermore, the ecologists I worked with, as well as scientists I know from other fields such as molecular biology, do not follow the format in reading a paper. They have told me that they look for interesting titles or authors, and when they have found a prospect, proceed directly to the results section to find out what the paper is actually about. If this material is relevant to their work, they move on to the discussion or materials and methods section, and then may start at the beginning and read the paper through. In other words, by the time they decide to follow the conceptual path laid out by the paper, they have already agreed to take the paper seriously. I shall consider this matter in much more detail in Chapter Ten.
where I portray Dr. Williamson's research as it might be seen by a reader of one of his papers.

Another point that must be made here is that scientists are not entirely unjustified in considering their work to be driven by an external reality. There is a great deal more to being a scientist than exercising a particular form over the use of language. A scientist must make his experiments work, and the results he gets from the experiments he does are often extremely inconvenient indeed. Often they are quite contrary to what he expected, and frequently he must accommodate himself to very surprising data. For sociologists to focus on literary form and style without reference to the temporal development of the activities of the research process in which the papers are rooted and in which they are read and used looks suspiciously like a continuation of the positivistic custom of focussing on formal discourse instead of considering real action. Such an approach also sidesteps the relationship between scientists and the world they study, and tends to create the impression that scientists exist wholly within a world of discourse. One wonders how they manage to create facts that are actually about something.

So far as this thesis is concerned, the importance of the rhetorical style of Science lies in the fact that it embeds a positivistic view of the relationship between knowledge and action deeply in the everyday affairs and consciousness of any person who regularly writes and reads such material, that is,
any natural scientist and most social scientists. Such an
attitude makes it seem rather odd to ask what scientists do,
because in relation to knowledge itself the question is
ultimately irrelevant, and in relation to other aspects of the
scientific profession, it has been answered. Learning to see
research, instead of Science, is a complex undertaking.

Enforcing the orthodox view

If we look outside the lab for indications of positivistic
rhetoric we find that there are a number of institutions whose
employees and other associates put considerable effort into
purveying the spirit of John Stuart Mill. The standard format is
enshrined in such works as The Council of Biology Editors Style
Manual, and the Guidelines for Format and Production of
Scientific and Technical Reports, put out by the American
National Standards Institute. The standard format is advocated
by such organizations as The British Standards Institute, the
Federal Council for Science and Technology (of Great Britain),
and in the USA, the National Technical Information Service, and
the Defense Documentation Center. This list is merely
indicative of the degree to which this view of Science has been

19 C.B.E. (1978) Distributed by the American Institute of
Biological Scientists. Arlington.


Individual scientists have little chance to dispute this kind of entrenched orthodoxy. Such autonomy as they possess comes at the expense of committing themselves to their profession, which requires that they circulate research reports written in the accepted manner. Without this, they have no standing, and no support. Scientific research does not usually produce revenue, the way that running a store or operating a mine does, but even if it did, it would not provide for all of a scientist's needs. In order to do research, scientists must have money, facilities, equipment, students, and an audience of peers who are professionally committed to providing both critical perspective and useful information. The key to obtaining these lies in developing a good reputation and establishing an institutional niche. To do that an academic scientist must publish his work under his own name in a reputable refereed journal. If he works for government or a corporation he must circulate in-house reports. These formal papers are the most authoritative statements that scientists make about their work and since they are public declarations they must be defended, and if proven wrong, retracted.

Formal research reports—however circulated—are very important to the professional scientist. In *Use of Reports Literature*, Charles Auger, a librarian writing practical advice for engineers and scientists, has this to say about the purpose of research reports:
the aim of any report is to influence the reader, to the extent if necessary of his taking direct action, but more often merely to enable him to be better informed in making a decision which is affected by many factors and to which the report is just one contributing element. Consequently, before starting to write his report, the author will find it good practice to ask himself five fundamental questions: (1) Who are his readers, and can they be dealt with in one report? (2) What do they already know about the subject in question, and how much background information will they need? (3) Why are they going to read this report—to find facts, ideas, recommendations, courses of action? (4) How can they be induced to read this report in spite of their possible apathy, indifference, ignorance, prejudice? (5) When will they read the report and how much time will they be able to devote to it? 22

In other words, a research report which is presumably a statement of objective fact is simultaneously a persuasive tool of professional politics. 23 A scientist who is writing up his research for publication tries to make his report relevant to the kinds of decisions and discriminations that members of his chosen audience must make. It is an attempt to get the reader to follow some recommended procedure, say a new technique for counting the numbers of animals in a wild population, or to recognize the existence of something that was not known before, such as the double-helical structure of DNA.

A respected scientist's work is taken very seriously by his peers as they struggle with their own research questions and try to come to grips with the events in their own laboratories. Scientists are repeatedly in the position of having to decide which observations to do, which experiments to conduct today.

22Auger, 1975, pp. 46-47.

23Mulkay, 1979, summarizes the recent sociological investigations of this.
later this week, next month and so on. Substantial portions of their funds, scarce and expensive equipment, the time of technicians and students, are committed when these decisions are made. The decisions in turn depend on the scientists' best guess of what they have in their test tubes, and how its character can be teased out. Their careers depend on the quality of these and other decisions, and the decisions often depend on the reliability of colleagues' work. Consequently most scientists work hard to stay in touch with their colleagues' research.

In a situation like this, people are very sensitive to any indication that the work of others is untrustworthy or wide of the mark, and very keen to be seen as doing relevant, reliable research. Here are the words of a highly respected cancer immunologist, who was waiting for experimental confirmation of a contentious theory which she hoped would back up her previous work:

This ... is the most miserable time of all. The hunch is stronger than the observation. The links between the hunch and the observation are clearly there, but at this stage people still think you are quite mad. They are as fearful as if you had cancer. The danger in cancer is to die. The failure in this madness is failure to communicate. And the danger in this process, at least in this society—is the danger of having an idea that is wrong. It is equivalent to death.2

Reputations are fragile. Scientists who are serious about building their careers think carefully about what they publish, and then are deeply committed to the story about their research.

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that they publish. The man I worked for knew this very well:

You should be very aware of what you write down, not because of the crucial importance of your special words, but because you are entering something into the record, and once its in, its in. (Interview (S-2-28), April 1981)

We should think about what is at stake here. A scientist does research, and then publishes a paper in which he claims to have discovered something real. That paper will advance his career very little unless his colleagues read it and take it seriously. A paper that is taken seriously is ripped apart by its readers, so to speak. They compare it with their own research, look for flaws in his work and try to project what the consequences of this paper would be for their own experiments should the author be correct in his claim. There may be a sizeable group of professors, students and technicians who discuss the paper among themselves at some length. This bodes well for the author. Even if the readers ultimately decide that they do not agree with the author, or that his work is not quite relevant to theirs, the important thing is to be part of the debate. Apart from having made a serious mistake, the worst fate actually is to be ignored.

Suppose we now consider an author who is being taken seriously. According to some ethnomethodologically oriented social scientists who study natural scientists,25 there is at

least one very striking feature of the discussions that will ensue. When scientists dispute a colleague's claim, they do so by drawing attention to what he did and to the conditions under which the "discovery" was made. They take the object he claims to have discovered and bring it down to earth, so to speak. They may question his technique, criticize the form of analysis he used, suggest that he is committed to an outmoded theory, or perhaps is just trying to cover for one of his graduate students. **Doubting the reality of his findings takes the form of paying close attention to the conditions of their supposed discovery:** the false object is a human creation, an artifact, while the true object is discovered, not made. This can happen when scientists are reading a paper or when they are trying to figure out what is happening in their own work. To go beyond the ethnomethodologists' arguments, it seems that scientists working in the lab will put their own hypothesis through the same process of construction and deconstruction that they know will be subjected to in other labs once it is published—if anyone takes notice of it.

However, when a claim is accepted as fact, when the reality of the object is granted, references to the activities and circumstances of the authors cease. If the author appears after that, it will be in a highly stylized account of the experiment he did, or like Slovik, he will be held to have had an idea.

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one day. At the same time as he becomes credible as the discoverer of a thing, he disappears from the account of it, to all intents and purposes. The true object is not a product of human endeavor, but rather a representation of what the world is really like.

The point I am making here is not relevant only to the scientific profession but is a more general one. It has to do with what happens when local or private events are disclosed publicly. When a person frequently is in the position of having to justify what he has done, to an audience of people the great majority of whom he does not work with closely, some of whom he seldom meets, and the rest of whom he will likely never know personally, then the issue of what is known or inferred about him in the public arena is of crucial importance. A politician's career can be destroyed by one personal indiscretion if enough members of the public choose to take umbrage at it. No matter how highly he may have been regarded previously, if enough people believe they have good reason to point to this specific thing he has done, or is alleged to have done, rather than to his earlier reputation, then he is finished. Not surprisingly, politicians are extremely careful of how they are seen. The administration of justice is subject to a similar constraint. It is not sufficient that justice is done. Justice must be seen to be done.

My point here is that what people are seen to have done is crucial to the credibility of what they claim is true, and that
science is no exception to this. Maintaining the public image of one's own work is critically important. Clearly, no scientist in his right mind will reveal more of himself in a paper than the accepted form allows. Indeed, it seems well suited to this kind of political process. One indication of this is that scientists are very careful about where they publish. They must be seen to be using the accepted channels. It is one thing to publish popular articles about work that has already been acknowledged in the professional arena, under the guise of educating the public. It is quite another to first put forth one's serious professional product in a form which suggests that it might have appeared in a crank journal or a popular publication such as a newspaper. And it can be the kiss of death to go around speaking publically about what science is actually like. There are some exceptions, usually men who have unassailable reputations based on work they did so long ago that their papers no longer figure in current debates.

Given the circumstances, it is not at all unreasonable for scientists to adhere to the orthodox view of their work. The politics of public representation are a powerful force in their lives. Moreover, as I have noted already, any experimental scientist is stuck with having to make his experiments work, and as the following chapters will make clear, a person in this position can easily be forgiven for believing in an external and autonomous, or even vengeful, reality, that dictates the outcome of his research. It is not surprising that scientists take their
experiences very much to heart, so that there is a strong moralistic edge to the high value scientists place on objectivity. Peter Forbes, writing in *New Scientist*, calls this attitude "Scientist's Squint": "the Squint consists essentially of an inability to see that whereas science is always seeking to reduce disparate phenomena to one underlying principle, life usually travels in the opposite direction, blending simple ingredients to achieve subtle complexity." He calls Peter Medawar to task for saying, "Sciences not yet underpinned by theory are little better than kitchen arts." And just what is so bad about kitchen arts, Forbes responds? He feels uneasy about the contempt for life expressed in this statement, and suggests that scientists have trouble dealing with even the highly regulated reality of the industrial engineer.

It seems to me that the exclusion of messy particularity is a genuinely important ideal for scientists. Knowledge is not supposed to depend on the characteristics of people, or on what they do: its being infected with culture, religion and such things is wrong, in the full moral sense of that word. The attitude of many scientists I have known could be summed up this way: "The real world is what it is, dammit, and how we look at it does not change anything!" An extreme statement, perhaps, but

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not an unusual one.

**Summary of the problem**

I, for one, am inclined to believe that the world is whatever it is. I am not inclined to believe that the statements our institutionally supported professionals make are simply snapshots, so to speak, of reality. Scientists do not simply "see reality". They create and manipulate the material and the ideological world of their laboratory. Moreover, the work they do and the knowledge they produce are part of the way that the dominant institutions of our society transform our world. The ordinary affairs of scientific research are in no profound way separate or different from what goes on outside the lab. Rather, the daily activities of scientists are simply a special case of institutional life.

In the light of this let us consider what I have been saying in the last three sections. I have argued that there is deeply entrenched in the most authoritative and prestigious communication practices of professional scientists, a distinctive philosophical world view. The most important feature of this doctrine, for our purposes here, is mentalism. The essence of mentalism is that the appropriate way in which to discuss the technical/intellectual core of scientific inquiry is in terms of private mental acts in which a separate immutable reality is directly apprehended by the mind. This attitude
corresponds closely to the requirements of public debate, for it is imperative that a scientist be very cautious about how much of himself he associates with his claim to know. The credibility of his knowledge depends on its being almost entirely dissociated from him. Yet he must not allow this dissociation to be complete, for the knowledge must be his property, if only for a while, or his career will wither and fail.

The scientific research report, which I described earlier as being a simple and straightforward document, clearly has a complex significance in the lives of professional scientists. It is supposed to be like a mirror held up to reflect nature, a mere photograph of a fact which inhabits the real world. It is also supposed to be enough of a history of research that the author's proprietary rights are recognizeable. This, I believe, is one of the reasons that the Materials and Methods section is included. The report is an instrument of persuasion, intended to alter the thoughts and actions of other scientists. This process of public persuasion inevitably includes justifying the claims one makes, but the justification extends beyond intellectual matters. The paper also justifies the writer's career, his use of public and corporate moneys, his personal authority in the lab, and his teaching job at the university.

In this section, I shall examine some of our philosophical assumptions in the light of the way they are used in public

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29Forty, 1979, Introduction.

justification, and consider the significance of the public image of Science for a person who wishes to examine local scientific practice. Obviously, I am arguing that the world view inherent in the scientific research report is crucial to some of the uses to which the report is put. One of the reasons it is suitable, if I may be excused for using the word "reason" in a functionalist manner, is that this world view is widely accepted; it is familiar to us all. After all, people do not normally think of true knowledge as something that we humans produce or make. The usual belief runs quite the opposite way, holding that true knowledge has nothing at all to do with human artifice. It is an account of what is actually the case, independently of our knowing it, and any intrusion of the knower into this is only a source of inaccuracy. At the same time, most people also believe that they do actually know something, that this knowledge is a result of their experiences in life and is important to the way they now live. The contradiction here should be obvious, and it is found in many philosophies which are otherwise very different.

This brings us to the other "reason": the ambiguity of this world view. It creates the problem of explaining how this unapproachable stuff called knowledge could possibly be part of our lives, and therefore it can provide the narrative vehicle for such debate. This tradition enables us to discuss how we can get knowledge (for we must have it, or so we believe) and at the same time retain the notion that it will have this pristine
character, for without that, it is not true knowledge. This problem is especially pressing for experts, politicians and bureaucrats who must justify their authority before a public of some kind, even if it is only a public of their peers. What they claim to know and what they propose to do must be seen to be determined by the way the world really is, and not by special interests of their own or those of corporations and the like, which pay their bills.

Since the time of the ancient Greeks we have dealt with the contradiction between knowledge and action within the terms of a tradition which collapses the complicated process of inquiry into a private mental act, an impersonal contemplation of a separate reality. In the words of Dewey,

"Thought has been alleged to be a purely inner activity, intrinsic to the mind alone, and according to classical doctrine, 'mind' is complete and self-sufficient in itself. Overt action may follow upon its operations, but in an external way, a way not intrinsic to its completion." 31

Reflecting upon knowledge in these terms provides us with a way to discuss it which appears to be complete, but sidesteps overt action.

The escape is not a clean one, however. It produces problems that have dogged the Western rational tradition since its inception. Alisdair MacIntyre puts it thus:

"At the core of this tradition is a conception of knowledge as analogous to vision: the mind's eye beholds its subjects by the light of reason. At the same time, it wishes to contrast sharply knowledge and sense

experience, including visual experience. Hence there is a metaphorical incoherence at the heart of every theory of knowledge in this Platonic and Augustinian tradition.\textsuperscript{32}

This problem was transformed and carried forward into modern philosophy by such thinkers as Rene Descartes. Descartes is famous for his attempt to rid himself of his philosophical confusions. He decided that he would discard all of his beliefs until he could discover a presuppositionless first principle upon which foundation he could base a new system of belief. This principle was his famous "cogito ergo sum"--I think, therefore I am. According to MacIntyre, Descartes inevitably passed on the fundamental confusions of his predecessors when he built his "new" system because he did not consider his own habitual allegiance to the French and Latin languages and the traditions they carry and express. This cultural world was a product of, among other things, hundreds of years of Roman Catholic philosophical and theological debate. As a consequence of ignoring this, "...much of what (...) Descartes...) took to be the spontaneous reflections of his own mind was in fact a repetition of sentences and phrases from his school textbooks. Even the cogito is to be found in Saint Augustine.\textsuperscript{33}

Like most intellectuals Descartes confined his inquiries to what he recognized as the realm of theory, and did not think to examine the customary modes of communication of his society.

\textsuperscript{32}MacIntyre, Alisdair (1977) "Epistemological crises, dramatic narrative, and the philosophy of science", The Monist, v. 60, #4, pp. 458-459.

\textsuperscript{33}MacIntyre, p. 458.
which he habitually used, and which shaped his perception of the world. They provided the categories of thought that he recognized in the form of theoretical issues. Like Descartes, modern social theorists do not normally examine the way that they communicate with one another (specifically with other social theorists) when they are trying to understand the properties of their own theoretical systems.

Descartes changed the tradition. In his work, the separation between mind and body that was a major feature of ancient Greek philosophy is taken into the individual person; his mind is set over against his body. This philosophical transformation occurred in the wake of political and economic changes in Western Europe, which saw the development of mercantile capitalism and widespread acceptance of the notion of private property. This development could be seen as one in which philosophy "caught up" to the way people had come to live. It came to embody the new situation so that it could be consonant with the way people pursued and made sense of their lives. In the process, Descartes became a famous philosophical spokesman, and his work has continued to receive a great deal of attention.

That the philosophical tradition to which we presently subscribe has its roots in the time when private property was developed has, I think, some interesting implications for the role of mentalism in professional publication. Remember that in this view, knowledge and all forms of experience must be sharply disjoined and yet there has to be some connection between the
know and that which is known. On the one hand, that which is known must be uncontaminated by the knower and his affiliations if it is to be persuasive. On the other hand, knowledge is after all the property of the knower and the touchstone of his authority. In fact, his authority rests upon its having been he who got this knowledge. Something peculiar and intrinsic to him must be involved in this business; there must be something the authoritative one did. Knowledge would not be valuable if it were just randomly available to anyone. Both this disjunction and this connection must be embodied in the knowing person, since Descartes, and the Scientific Revolution of the Seventeenth Century. Collapsing inquiry into a mental act preserves both ownership and purity, providing one does not examine the justification for this too closely.

I am inclined to believe that this philosophical conundrum persists because of ambiguous ends in the way we understand knowledge: possession and credibility. It must be admitted that more than one hundred generations of philosophers have not so much resolved the dualism upon which this is based as they have worked out new and ingenious ways of hanging onto it. This is because of the great political importance of maintaining a tradition of discourse that can accommodate the ambiguity of knowledge and therefore of power.
Methodological implications

In our time, the touchstone of intellectual authority has been Science. There have been many who have wished to emulate what they thought science to be. Social scientists, bureaucrats, advertisers and even some of the more unconventional religious leaders have claimed that they, too, are Scientists, or at least that they had consulted Scientists. Some have done this because they sought clarity, others because they felt that the state should be administered rationally, and still others were driven by the need to sell soap or cars. They all have claimed to have a Scientific understanding of what is going on, and have used this claim to persuade people to heed their words. It should not be surprising that philosophers have debated the nature of the sciences for hundreds of years. From a purely intellectual point of view, it is certainly very interesting and important to have a way to account for this strangely reliable knowledge about the world. And for the person who must make public his knowledge and justify his claim to authority, there must be a body of sophisticated discourse in terms of which this justification can be conducted. There must be a philosophical vehicle for it. This vehicle must be well rooted in the culture within which he operates and it must be able to sustain ambiguity.

My view of the implications of the formal communication system of science for the study of research has been sketched briefly in bold strokes because I wish to move as quickly as
possible to my account of Dr. Williamson's research, but I have also wanted to make clear what is at stake here. The point of view that I have outlined should be explored in more detail, for there is much here that is contentious, but I shall have to leave that for another occasion. I would like to conclude my discussion of publication by stating several points that I think will withstand further examination. One of them is that any person who normally is obliged to write up his researches in a positivistic narrative form will be profoundly committed to a positivistic conception of his own activities as an inquirer. As a consequence, any person who writes in the way I described will have great difficulty in proposing a codified overview of inquiry, a guide to how it should be done, that is a coherent alternative to positivism. Finally, this literary/methodological commitment will dictate the fundamental character of the writer's view of the people he has studied; he will almost inevitably portray them in a way that is consistent with his portrayal of himself.

The really important point here is that methodologies are logically circular, and that this is especially important to the study of scientific research. Methodologies are generally based on some version of Science, which effectively excludes research. The epistemological assumptions which underlie the way a social scientist accounts for his own ability to know about the people he has studied will dictate the general outlines of the view of human life that he portrays. These methodologies do not deal
only with knowledge. They are world views. They include a
conception of the nature of world we live in, our relation to
it, and to one another. A vital link in my argument is that
this world view is an integral part of the politically central
publication system of the academic profession. Remember that it
is very common for sociologists, psychologists, and other social-
scientists to write in the way that Gusfield described. These
authors either implicitly or explicitly portray themselves as
empiricists, detached minds perceiving a separate
self-subsistent reality.

In view of this, it is extremely interesting that Anthony
Giddens has said that one of the main flaws in classical social
science is the absence of an adequate theory of action. The
lives of people, as social scientists portray them, do not
cohere. Social scientists, along with many others, often do not
see persons as knowledgeable, active, intelligent human beings,
who take stock of their surroundings, decide what to do, and
then must deal with the consequences of what they and other
persons have done. The active living focus of their lives, as
they build and sustain it, is not made visible. That kind of
historical development is effectively absent from the way most

34Hocker, Clifford (1974) "Synthetic realism", Synthese, v. 26,
pp. 409-497.

35Giddens, Anthony (1979) Central Problems in Social Theory. New
York: Macmillan. Chapter Seven.
social scientists represent human life. How people establish the order of their lives and thereby respond to and maintain the society they live in remains a mystery. Social scientists typically find themselves analysing a transcendental self-subsistent social, economic or political "reality", or else delving into the mysteries of private experience, or very short snippets of situated action. A coherent, viable way of understanding human experience has been an elusive goal.

Roberto Unger has described the problem as the evisceration of particulars and the reification of universals. Applying this terminology to science, we see that in scientific discourse, the author reifies the mathematical conclusions of his analysis, and he eviscerates not only the particulars of any one deer, for instance, but also himself as an inquirer. For he reduces himself to a mind, which is also reified, of course. The persons studied by social scientists suffer a similar fate. Insofar as the relationship between human individuals and society is concerned, the individual becomes what Harold Garfinkel has called a "cultural dope", a walking talking clone of an abstract social condition. Unger summarized the consequences in this picturesque way:

...abstract qualities take on a life of their own

For an exception to this, see Spradley, James P. (1972) You Owe Yourself a Drunk. Boston: Little, Brown and Company.


Woolgar, 1981.
because they are the sole possible objects of thought and language. Despite the acknowledgement that universals are abstractions or conventions, everyone talks and acts as if they were real things, indeed, the only real things in the world. From the evisceration of particulars and the reification of universals there ensues a spectacle that would be strange if it were not too familiar to be noticeable. Though it is the particulars that are supposed to have concrete reality, it is to the universals that thought and action are addressed. The ghosts sing and dance on the stage while the real persons sit dumbly in the pit below. One may be forgiven for wondering who is alive and who dead.

I doubt very much that either Giddens or Unger would argue that conventional social science has nothing to offer, indeed their work is nothing if not highly conventional itself. I am clearly not taking such a position either, for I have used such work to establish the argument of this chapter. I have taken the trouble to draw attention to these critiques of mainline social science because such they point to an underlying problem my project raises, one which I must consider explicitly if I am to outline the significance of examining research, even though I do not have the space here to explore this problem at length. What is at stake in my thesis is not simply the problem of how to portray scientists in relation to what they know. More generally, I have had to consider what has to be done in order to represent any kind of intelligent, sustained action whatever. Methodology, and the papers that professional scientists must write in order to secure their position, are crucial to this. A social scientist who must portray himself as an essentially empiricist inquirer will have great difficulty articulating the

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39Unger, 1975, pp. 136-137.
possibility that natural scientists are anything else, even though it is abundantly clear that they must be.

Theoretical discourse on the failings of social science has consisted largely of attempts to work out abstract specifications of the relationship between the social scientist and the persons he is studying, which seems reasonable enough. I have yet to see an analysis of the theoretical crisis of the social sciences that centers on an explicit, systematic account of the normal professional political activities of scholars. Yet these are an intrinsic part of the everyday business of scientific inquiry, a central feature of the daily process in which scientists decide what is the case, and their knowledge bears the mark of this in both the natural and the social sciences. Therefore, to a very significant extent they constitute the relationship of the knower to the known. Social scientists are beginning to make tentative jabs at the politics of inquiry, but they are a long way from incorporating such insights into their own public, conception of themselves. This in turn handicaps the efforts they might make to account coherently and systematically for what natural scientists do.

In the social sciences, there is a long history of polemics and apologetics which aim to demonstrate that the social and natural sciences share a common logical and methodological framework. I have come to the conclusion that the most important similarity between the two is instead political, and that the methodologies they have in common are rhetorical devices their
practitioners use in justifying their activities publicly, to their peers and others. Members of both groups of disciplines are practical political actors competing within a highly bureaucratic professional system within which they build and maintain their own research organizations, and from which they must obtain support. Formal knowledge as we know it emerges from their attempts to keep track of what is going on in their research, to understand it and direct it. It grows out of attempts to control practical action, and it later serves as a public justification of the work done, and a practical link to colleagues. The narrative form they use, and the view of life it affirms, are further confirmations of this involvement.
III. The field work: learning to ask what scientists do

If I wish to portray scientists who actively organize and conduct their research, who work together and influence one another, and who change their circumstances and are changed by them, then I must take a similar view of my own activities. I must have a methodology of involvement, change and the establishment of historical continuity. This places several special demands on me. I must first of all have some way to discuss the actual origins of my inquiries and not simply frame my questions in the terms of some presently fashionable academic debate. This does not mean that I should devote a great deal of space to discussing my own struggles, or that I shall be able to completely recapture my starting point. It does stand to reason, though, that I must acknowledge the changes of my own point of view that came about as a consequence of this research, if I am to have a way of accounting for the changes of the scientists' world that has any intellectual integrity whatever. I must discuss my involvement with the scientists I studied. I must also discuss my involvement with the social sciences, which occurred through the Department of Communication in which I was based. These are the basis of my claim to be able to represent and analyse Williamson's research. This I must do in such a way as to show that this professional system is not so completely determinate that I could not evade some of its constraints.
enough to get a critical perspective on what is done in order to maintain it. Finally, writing this thesis called for considerable thought about narrative form and explanatory device, which I must examine explicitly.

The next few pages are a very brief account of the origins of my questions, and the nature of my field experience. As you might expect, the field work and writing I did led me through a major change of world view. I shall not discuss that change in this chapter, for to do so I would have to outline my present view, which would anticipate the body of my thesis too much. Chapters Four through Eight will establish my present view and I shall discuss the change in the Conclusions. Thus part of my "methodology" will be at the end of the thesis, rather than at the beginning.

Starting point: the surreal character of Science

I began this thesis with a partial list of the various things that made up the everyday affairs of my ecologist friends. The task of the thesis is to show why these should be resolved into a coherent account of the production of knowledge, and then to provide that account. When I began this project, however, I was far from being able to write such a story of research, or even to see so clearly that this was my task. I was in many ways a product of my training, even though I realized that there was something fundamentally amiss with what I had
come to believe. My stance could perhaps have been called a kind of fatally flawed logical empiricism. Consequently, when I began my field work late in 1976, the combination of things that I listed earlier was for me a montage. It carried the flavour of a surrealist painting. As in surrealism, normal or fairly normal things were placed together in an abnormal way, a way which violated my cultural expectations.

"Threatening Weather", a painting by Rene Magritte, provides an example from art. It portrays a seascape on a clear day. In the foreground is a rocky beach, which leads to a series of promontories that fade into the horizon on the right. Above the sea float three huge ghostly shapes, possibly clouds: the nude torso of a woman, a tuba, and a straight-backed chair. They are all roughly the same size, and stand shoulder to shoulder, so to speak. All of the elements of this painting are relatively normal, the surreal quality is found in the way they are put together by the artist. It is very difficult to interpret Magritte's painting. The parts are apparently irreconcilable.

There is no well-known convention in terms of which you can put them together and give the painting meaning. Here, "convention" is not simply a matter of a consciously recognized set of rules that a person can knowingly apply, but more basically is a culturally conditioned predisposition to see certain kinds of relationships as harmonious and meaningful, and others as not so.
The collection of elements from the working world of my ecologist friends had a surreal quality because I was putting it together under the title "An Account of Knowledge". A voice within me called for this association, but deeply ingrained attitudes rebelled against it, even as I sought to establish it. The surreal quality was not something I wanted to celebrate or reveal in a work of art, but an epistemological symptom of alienation that I thought should be overcome. I saw the field work as an opportunity to deal with a split in my way of understanding science, and more generally, in my acculturated way of understanding people's relation to what they know.

I knew that the split had been produced in part by my scientific training. On the one hand, my classmates and I had spent years learning how to operate oscilloscopes, carry out polyacrylamide gel electrophoresis, and construct and read many different types of graphs, to name only three of the skills we knew were essential to research in various fields. In addition, I did several projects with my professors, which enabled me to spend time in their laboratories and become involved in the action there. And action was the key word, for these people were forever building equipment, reading papers, arguing and trying to decide what to do next.

On the other hand, we had all assimilated vast quantities of facts and theories. These were beautiful, awesome and penetrating: the shape of a protein molecule, and how it enables the molecule to act as a catalyst of chemical reactions; the
energy curves of reaction kinetics, and the orbitals of quantum mechanics; Einsteinian relativistic kinematics and the magnetic field of the earth. There was an endless stream of impressive and authoritative accounts of what the world is really like. They had a wonderfully remote quality. They were held to be independent of what we wanted or what we did; they had an almost crystalline inevitability, with their simplicity and solidity. There was nothing about them to suggest the messiness of research, or the amateurish fumbling in our instructional laboratories. I found that I was quite unable to imagine how such things could be the products of human design and endeavor.

For me, the activities of the laboratory were as remote from the kind of knowledge that supposedly came from research as the torso, tuba and chair were from the beach. They were immanent in the situation in some way I could never pin down, but their connection to our presence there was not apparent. I understood, toward the end of my undergraduate training, that this separation was something I had become used to. It had been part of my education.

I had read The Structure of Scientific Revolutions,¹ and had found Kuhn's view of science education completely convincing. Kuhn pointed out that science textbooks are written as rational "historical" narratives, in which the past luminaries of science are portrayed as having been working

toward precisely that understanding of chemistry, or physics, or whichever subject the text dealt with, that is held today. In textbooks history is revealed to be a logical engine. Early scientists are seen as having organized their understanding of the world in a way that is logically compatible with our present view. In fact, they are portrayed as having seen exactly the same reality that we now see, except that they saw it less perfectly. Misconceptions and superstitions got in their way, but in the course of time these human impediments were swept away by heroic figures and what was actually the case shone forth. The facts and theories that the text is meant to convey are told in re-enactments of classic experiments and theoretical formulations, so that the subject-matter of texts, their portrait of the real world, is inseparable from the positivistic conception of knowledge and method. The idea that knowledge is part of a cultural complex, all parts of which change profoundly with time, is made unavailable by the quasi-history of textbooks. Ironically, it makes any authentic human participation in science intrinsically unthinkable, at the same time as it appears to do the opposite.

I had committed myself to this world view in struggling to master the curricula of my courses and reproduce them perfectly

2 Examples of this kind of reconstruction abound. For one that is both well done and widely used, see Biology, by Helena Curtis, New York: Worth. (1979) 3rd edition. Chapters Twelve to Sixteen provide an illustration, conveying the basic facts of molecular biology by recounting a series of classic experiments that constitute the history of the development of the field.
on exam papers, so that I could get high grades and do well. I learned to see the facts of Science in the world, and not just in textbooks and laboratory exercises. Thus when I looked at storm clouds, I saw five hundred mile wide mid-latitude cyclones. Striated rocks revealed the glaciers of past ice ages, and I saw mile-thick sheets of ice and the changes of climate and vegetation that had followed their retreat. With equal facility I saw much more abstract things as well, the interlocking biochemical reactions of the tricarboxylic acid cycle of carbohydrate metabolism in an animal, the Second Law of Thermodynamics in a refrigerator.

But I did not see scientists in Science. The way I just naturally tried to "see" science itself, the view of it that came easily, in fact automatically, was Science, and there were no believable people in it. Of course there was a way of bringing out the human side of scientists. We all knew about Albert Einstein's violin playing, and young Charles Darwin's voyage on the Beagle. But these portrayals were part of the separation that I have been talking about. They are actually part of the way that the person is removed from Science. These men (and women, for there was Marie Curie's tragic death from radiation poisoning) were heroes, larger than life. Besides, we were not being given credible accounts of how they did their work. We were hearing about music, illness and the workings of the mind, which are important, but do nothing to distinguish scientific research from any other endeavour. I was trained to
reflect upon the nature of Science, and that actively impeded my coming to grips with how people do research and make Science. At the same time, I was strongly but inarticulately convinced that people do make scientific facts and theories, because these come from research and research is done by people.

I could no longer accept the orthodox view, but I had no alternative to it. I recognized that I was deeply habituated to it, and that it simply would not be possible for me to think my way out of the problem I faced. My habits of thought confirmed it, recreated it. I was in a kind of existential crisis, an epistemological crisis, if you will, and knew that. I also knew that my inability to accept the professional scientist's account of his accomplishments was just the tip of an iceberg. At stake was my faith in the rational institutions of liberal democratic society: universities, government ministries, and corporations. All of these are publically justified by their employees in the terms of an ideology that is essentially a simple-minded version of the positivistic world view. Like many people from middle-class professional families (my father was a university professor) I had been raised with a kind of unconscious faith in the ability of these institutions to provide the kind of order that is necessary for our society. I was told of their imperfections, of course, but was essentially uncritically committed to them. When I began to understand how these institutions are transforming our lives, how the 'knowledge' their agents have of this world is imposed on us in the form of
regulations, advertising and ideological hype, I began to have very serious doubts about the way this society is organized and about the way we are taught to think about such matters.

The surreal appearance of Science, which was the starting point of my inquiries, speaks to what James Clifford has called the modernist orientation toward culture:

(it takes) as its problem—and opportunity—the fragmentation and juxtaposition of cultural values. From its disenchanted viewpoint, stable orders of collective meaning appear to be constructed, artificial, and indeed ideological and repressive. The sort of normality or common sense that can amass empires in fits of absent-mindedness or wander routinely into world wars is seen to be a contested reality, to be subverted, parodied and transgressed.

I was in a state of crisis, and as Alisdair MacIntyre has pointed out, "a crisis in the self (is) a crisis in the tradition which has formed the self." Where one locates the crisis in the tradition is to some extent a matter of personal preference. The Kuhn-Popper debate, and its surprisingly high profile outside of the history and philosophy of science, made it clear that some kind of broadly based attack on the technical core of academic orthodoxy was under way. Behind this was the political and social backdrop of the world-wide unrest of the 1960's, a time when the credibility and moral authority of modern industrial society, if not its power, sagged visibly. The descendents of the millions of men, women and children who were

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4MacIntyre, 1977, p. 459.
murdered, mutilated and raped in two world wars were reliving, en masse, the sense of cultural bankruptcy that has dogged Western civilization throughout the twentieth century. The idealism of that time has faded, but the pervasive sense of unease is still very much with us. In fact, it has intensified into fear of global economic collapse and nuclear holocaust.

The particular focus of this experience in a given person's life depended on the kind of activity he or she was engaged in. In my case it was the scientific profession, and the focal point of my doubt was the supposed origins of scientific knowledge.

From my point of view, the sort of common sense that required me to learn a multitude of practical skills and then actually expected me to believe that the outcome of their application was completely independent of human action, and that the scientist's participation was to be subsumed under a mental act, was either moronic or immoral, and most probably was both.

Sidestepping the orthodox view of Science?

I reasoned that because my allegiance to positivism was deeply engrained in my habitual outlook, I could not simply discard it, and pick up another. To consider such a course of action would be to entertain an illusion, one which would leave my basic commitments unexamined. I would have to rework my commitments, in situ. If action was missing from my view of Science, then I should go to a laboratory, get involved with the
action there, get involved with the people and their experiments. I decided that if I developed non-verbal and pre-cognitive ties to the people and their work (in plain English, if I became familiar with them) it would disrupt my ability to maintain an orthodox viewpoint, provided that I was not committed to portraying their research the way they did. Therefore I opted for working in a lab as a participant-observer, hoping that the ambiguity of that position would enable me to understand the scientists' work in their terms, but then place those terms in the context of a different kind of story. At the same time, I had to make sure that in establishing my position outside of the lab, I avoided conventional social science, with its positivistic methodologies. If I were obliged to define my own work within such a tradition I could not hope to make adequate sense of what scientists do.

I made inquiries around the biology department where I had just completed my undergraduate studies. After considering several professors, I settled upon Dr. Williamson. There were several reasons for this choice. The first, of course, was that he did not entirely believe the view of science that he was obliged to take in writing reports, even though he was also obliged to defend it fiercely. He was curious to know what a study of research would turn up. This kind of interest is not uncommon, and it was not difficult to find scientists who were willing to be studied. In fact, since beginning the study, I
have been invited into several labs.

I chose Dr. Williamson over several other biologists largely because animal ecology is a relatively slowly paced field. Experiments take months to plan and execute, which suited my ends. I felt that I would be very confused at first, and that it would make my job easier if I were not in a situation where numerous short experiments were constantly being started, and where there was a mad scramble to stay in touch with the very latest developments. The second reason was that my own scientific background prepared me somewhat better for ecology than for any other field of biology. In addition, I have an aversion to the kind of extremely picky experimental procedures common to most highly technological fields of science. Williamson and his students worked out of doors with large, live animals, and that suited my temperament.

I spent two years working with the research group, studying them as they studied their deer, and studying the deer myself. From the beginning, my inquiries were directed by a few hunches I had developed while I was an undergraduate. I was particularly concerned to record how these people decided what to do, did it, and then dealt with the intended and unintended consequences of their actions and the deer's actions. It was very important that the deer be prominent in the account. There are things that deer will do, and things they will not do, and I wanted to see how the people moulded their daily routines and discussions to fit with the animals. By the same token, there are ideas and
procedures that the peers of these ecologists would accept and there were others they would not accept. I was curious to see how these two sets of requirements intersected, how the scientists manipulated the deers' lives and formulated explanations of them, so that the deer could be understood scientifically.

I wanted to see how the attempts to explain the deer, to figure out what they are "really like" fitted into and altered the course of the work, and came out in the formal research reports. I was also concerned to understand how this kind of discourse fitted into the discussion of such issues as funding, publication strategy and the career paths of the students. In sum, I wanted to know how the scientists mixed the deer together with the practical, intellectual and political requirements of the institutional world the scientists were rooted in, and I wanted to know how all this lead to knowledge.

At first, these general questions were merely hints, virtually empty themes I had developed while working with my professors and listening to them talk. I was quite unsure of their meaning, and because I was mesmerized by the textbook image of scientific knowledge, I simply could not bring them together in any coherent way. I knew that to fill them out and put them in place I would first have to become quite involved in the research. One of my first steps was to enroll in Dr. Williamson's graduate seminar in reproductive ecology, along with two of his new graduate students, David and Don, who were
also going to work with the deer. I had already taken a course
with Dr. Williamson as an undergraduate—field methods in
terrestrial ecology. I knew the department quite well from
having completed my undergraduate studies there. Nevertheless, I
still was very much at loose ends in the lab. There was nothing
for me to do and I felt a bit of a nuisance.

The prospects for my success brightened considerably when
Dr. Williamson obtained funding that enabled him to hire me as a
research technician for the summer of 1977, and again in 1978.
From then on I had a useful role to play, one that naturally
took me into the middle of things. Instead of hanging around
getting in the way and having to solicit special explanations of
the work, I was doing some of it, and was part of a routine-
conversational round in which David, Don, Dr. Williamson and a
few other people kept one another informed of the progress of
the research and tried to make sense of its results. I even went
so far as to ask Dr. Williamson to delegate some small part of
the summer's research to me, as my own responsibility, so that I
would have a more active role in the routine planning of it. As
a result, I was expected to keep track of what was going on so
that my work would fit with what the other people were doing. It
also meant that to some extent I was returning the favour these
people had shown me by allowing me to come into their midst to
study them.

In sum, this turn of events enabled me to establish the
kind of relationship with these people that I knew was necessary.
if I were to succeed at this task. Remember that I was there to learn how to ask questions appropriate to understanding research. I had to engage with Williamson and the others in such a way that I would be drawn through the experiences that would confront me with the extent and the subtlety of my commitment to a view of Science-as-Knowledge, and that would quite simply mould me and require me to develop new, habitual responses to the question, "What do scientists do?". Then, when the field work was over I could try to bring these new ways of relating to the scientific profession into a coherent order. I say this to emphasize two crucial things. One is the importance of my relationship to the scientists, that my inquiries were they-and-I-together. An adequate account of this research therefore must deal with both in order to deal truly with either. In contrast with academic convention this may seem self-indulgent, but I believe this impression stems from the metaphysical conceit of objectivism/mentalism that prevails in the profession. The second matter I wish to emphasize is the work I was obliged to do in order to make sense of my field experiences. The fact is that I am about to portray research on the nutrition and reproduction of captive deer in a way that I could not possibly have done while I was conducting the field work, even though large parts of the following chapters are virtually copies of sections of my field notes.

It is important to make these points quite clear because the metaphysical shift involved here is an essential part of
coming to understand scientific research. This is the reason that the inquiry necessitates an unusually self-conscious approach: one must find a way to account for one's prior commitment to a positivistic (or related) paradigm, and show why it is still with us, how people use it to establish order and meaning.

I shall complete this chapter by very briefly outlining the consequences of this self-conscious approach for the structure of this thesis, but first I should point out that the development of a new narrative, an explanatory device which achieves the things I have mentioned, is an ethical choice as well as an intellectual requirement. It amounts to advocacy of a specific kind of relationship between the person who knows and the the one who is known, which is a delicate matter, for it involves responsibility and trust. For my part, while doing this field work, I submitted to the demands of life in the lab. I participated in the activities there to be sure, but I can realistically claim that most of the work I did could equally well have been done by another person with much the same result for the other participants. I think that my impact as a participant-observer was not very great. The questions I asked about what the others were doing were not very different from the ones they asked one another in order to keep track of what was going on. An inquiry is after all an inquiry. Besides, Williamson and the others were extremely busy. They were the center of my attention, but I most certainly was not the center.
of theirs. Moreover, they were ultimately committed to a positivistic way of making sense of their experience, one that was at odds with my intentions, and so they were perfectly capable of screening out most of what I said, as they pressed ahead to their objectives. And I was, after all, confused. I did more listening than talking.

The crucial ethical issues at stake here are not centered in the lab experience, but rather have to do with the way I later portrayed and analysed the work we did. However much I may have fitted into the lab routine and done what any technician would have done, I also was committed to telling a very different story from Williamson's, to a very different audience from his. It should be clear from the previous chapter what might happen to Williamson if the members of his audience chose to pay attention to my account rather than to his. They could use it to discredit his work. In this case that has not turned out to be a serious problem because Williamson has left research and gone into administration, and soon his work will not have much currency in the debates of his former field. But this is beside the point. He is quite vulnerable to what I have written precisely because I took him and the others seriously as active, intelligent human beings, which oddly enough is not how they represent themselves in journals.

The ethical and intellectual considerations that I have raised have quite complementary consequences for the way my account is written. Both require that I introduce Williamson's
world on its own terms. The reader must learn to see that it had its own logic, that men and women who are as reasonable as you or I built this world and were content to work within its bounds. This is important intellectually because the reader must take seriously the issues of decisions, consequences and continuity, if he or she is to understand research. Ethically, it is crucial if these scientists are to be recognized as human beings—although I do not probe as far into their lives as a novelist would wish to see. At the same time, I do not accept the way they ultimately accounted for their experience, so I must in effect discredit their story. However, I am portraying something that all of us do, namely rewrite and rationalize history, when I take this step. It is the form of such accounts that concerns me, not the fact of their existence.

The last chapter might give the impression that I am about to embark on an expose, and it is true that I did this project because I objected to what I was expected to believe, an experience that made me quite angry at times. But Williamson is no more an example of this problem than are you and I. Indeed, it was my having the problem myself that qualified me for doing this kind of research in the first place, and most likely you are reading this because you are grappling with it yourself. I wrote the second chapter after the rest of the thesis was done and put it where it is, because I found that most of the people to whom I tried to explain my research are unaware of the nature, the depth and the consequences of their professional
commitment to a positivistic form of public representation. That chapter is there to acquaint you with your most probable commitments. If that seems arrogant of me, consider what the objective distance of the standard format of a professional paper says about the ideal relationship between a social scientist and the people he has studied.

The most difficult problem I faced in writing my account of Williamson's research was to represent that world in its own terms and show how the action in the lab developed and at the same time to have access to an outside point of view. This tension, the need to speak in several voices without creating a babel, determined the structure of what follows, and I shall conclude by summarizing it very briefly.

Chapter Four is an introduction to some issues in mammalian wildlife ecology. It consists largely of biological information that is essential to understanding the research project on deer that I was involved with. If you are to understand the decisions my ecologist friends had to make, and what was at stake in their work, you must have some grasp of the world of animals and ecology within which they situated themselves and upon which they drew. This chapter also deals with some of the politics of the profession of wildlife ecology. For the most part, it is written in the didactic public mode of Science. Some of it reads like an article in Scientific American, and most of the rest is based on the major articles Williamson published before he started working on deer. I discuss professional politics by
Within the general framework set by Chapter Four, Chapter Five recreates the basic outlines of the deer project as it existed when I joined it in the spring semester of 1977. It introduces some of the major characters, including the deer, and their contributions to the research and their relationships to one another. It does not detail the experiments done. This is left to the next chapter. Chapter Five takes the form of a history of the deer project from its inception in 1973 to my arrival. It is not meant to be a rigorous account of what actually happened. Rather it shows how this period was typically seen later by the participants, as they were making up the present by composing a story of the past. It indicates what they had learned and retained from earlier years. The chapter is based on taped interviews and recollections I had made notes of, so that it is told largely in the informal voices of the participants.

Chapter Six continues to fill in the early history, but does so in a very different way. Here I first give the formal public account of the first experiment Williamson and the others did, which was published and written up in a thesis. This is the backbone of what you might call the "official history" of the previous technical accomplishments of the project. It provided the underpinning of the work that was underway when I arrived. I recount this story and then I put together what I consider to be
the probable history of this experiment and the analysis of its conclusions. I go from speaking in the scientists' public voice to developing an outside point of view. Once I have done this, I show how Williamson and the others recounted the official history in the normal course of events in the lab, as part of the way they made sense of their situation. I show how formal public discourse is incorporated into apparently informal local conversation. This chapter establishes an ambivalent attitude toward the research scene in two ways. One, I establish myself to be inside it and outside of it; and two, I establish that the scientists are in this position too, for a public view of their work is an intrinsic part of their conversation in the lab. By creating this ambivalence, this chapter sets the stage for Chapters Seven, Eight and Nine.

These are written in yet another manner, that of a story. They complete the scientists' scene, introducing the remaining characters, who arrived at about the same time that I did, and then recounts the developing action of the 1977 research season. I did this by portraying successive incidents that were especially important or typical, running from January to November of that year. Throughout these chapters I heavily emphasize the practical decisions these people had to make, their negotiations with one another, their material manipulation of the deer, and the ways in which they tried to deal with the consequences of their previous actions and with what the deer did. Emerging from this welter, as an increasingly abstract
theme of their discussions and decisions, was an account of what
deer are "really like". A graph which gradually came to
encapsulate this theme was later published in a research paper.
Therefore I am able to trace the development of a formal factual
claim through the research process to the journal, which was
very fortunate for me.

Chapter Ten is concerned with what became of this claim. In
this chapter, I show how Williamson's research paper will most
likely be read by other animal ecologists. I have created an
hypothetical reader, one Dr. Curtis, whose interests and reading
habits are based on those of biologists I have known. This
chapter is mainly concerned with how this Dr. Curtis will read
the paper and deal with the limitations of the narrative form,
and place Dr. Williamson's work in the context of his own
research. With Curtis' reception of Williamson's work, we come
full circle, back to the public view of nature that is laid out
in Chapter Four and provides the background for the work that
Williamson did.
IV. The biological agenda

In order to understand the situation Dr. Williamson and the others were in, and the kinds of decisions they had to make, you must have some appreciation of their profession and of the world of plants, animals and changing seasons that their training and experience had brought them into, and in which they worked. Therefore, this chapter is a brief introduction to ecology. The material for the first half of it comes from Dr. Williamson's graduate seminars in reproductive ecology, his monograph on that subject, and conversations we had while working with the deer. The second half is taken from the professional papers that Dr. Williamson published before beginning his research on deer. This cannot be an over-view of the field of ecology. It is rather an introduction to Dr. Williamson's point of view, to the technical issues and intellectual resources that were central to the work I was involved in, and that informed the way Dr. Williamson, and to a lesser extent his students, perceived the natural world. Intellectually speaking it sets the stage and for this reason it is written in a way that confirms the view that science is a windowpane on the world, a growing repository of facts. This is much the same message that is given by textbooks and articles in such journals as Scientific American, through which novices are introduced to science.
Most of the basic issues that Dr. Williamson and the others concerned themselves with are interesting or at least well known, to most people—birth, death, sex, food and shit—although my scientist friends did not deal with them in their research in the same ways that we do in everyday life. The animals they used in exploring these matters were Columbian black-tailed deer, which are known technically by the name *Odocoileus hemionus columbiaeus*, although their thinking about ecology involved many other species as well. "Odocoileus" is the genus in which biologists place the two species of North American deer, the white-tails, which live east of the Rocky Mountains, and the black-tails, which live west of them. "Hemionus" is the species name for black-tails, and "columbiaeus" designates the subspecies of *columbiaeus* black-tails, which live on the west coast of the continent from Vancouver Island south as far as California. These are rather small deer: adult males range from 110 to 250 pounds in weight, and adult females range from 70 to 140. They live in forests where numerous open spaces let sunlight down to the ground, so that bushes and shrubs grow—which the deer eat. One of the major sources of open space in forests is logging and deer become very numerous in logged off areas for about 20 years after the logging has ceased, before the young forest becomes quite dense.

Williamson and his students were concerned with the effect of diet on the age at which female black-tails begin breeding and with documenting how much time young fawns spend suckling.
how much milk they drink, and how quickly they become
independent of their mothers. The experimental controls were
rather simple, having mainly to do with diets, and weighing
animals. The ecological theory that bore on this work is not
highly mathematical, although ecologists make extensive use of
statistics, and their work can be understood by people who do
not have well-developed mathematical skills.

The simplicity of the experimental controls and the absence
of sophisticated mathematical theories are for some people a
reason to regard physiological ecology as being "less
scientific" than other fields such as biochemistry or genetics.
There is no end to this sort of intellectual one-upmanship in
the competitive atmosphere of the academic world, and I do not
wish to contribute to it here. For our purposes, these features
of the work I studied are very positive, for they mean that this
research is far more accessible to a lay audience than the more
technically elaborate fields would be, that is, its technical
procedures are not so removed from ordinary experience. This
does not mean that the lives of wild animals are easy to
understand, but it does make it easier to introduce lay people
to the research setting.

Although we worked with deer, I shall deal with other kinds
of animals as well in this chapter. It is normal for ecologists
to shift from one species to another in their conversations, as
they try to figure out what the different kinds of animals have
in common. The complexity of life is such, however, that the
theoretical organization of ecology is rather chaotic. This chapter will reflect that state of affairs, as it moves from one topic and species to another. I shall begin with a general discussion of photoperiodism, the regulation of breeding season by daylength, and tie this in with some other basic issues in ecology that form the factual and theoretical background of Dr. Williamson's research.

I hope not only to provide the information needed to understand what comes in later chapters, but also to convey a sense of what is so fascinating about ecological research. The lives of these animals, the astonishingly complex and subtle adaptations they have made to their environments, are a genuine source of wonderment to the scientists who work with them. Scientific research is unspeakably tedious at times and can be confusing, poorly paid, mechanical, and competitive. You simply cannot understand scientists dedication to their work if you do not see that what they are studying is also absolutely fascinating to them. Only people who feel that way could work hard enough to succeed in such a competitive profession.

Physiological ecology

Human beings are very unusual animals, for we are able to reproduce at any time of year. Most animals are able to reproduce only at special times of the year. Furthermore, in most species, both sexes have to go through substantial physiological and anatomical changes in order to mate, changes
that are reversed after each mating season. For instance, the
testes of male birds swell more than one hundred fold just
before mating, and shrink the same amount afterwards. It seems
likely that this is because birds are stripped down for flying,
carrying no superfluous weight. It would serve no conceivable
purpose for male birds to lug big heavy testicles around all
year, and the same applies to the oviducts of females. In a less
dramatic way, the same changes occur in mammals. There are
marked behavioral changes as well. Birds engage in courtship
rituals, establish and defend nesting territories. In the case
of mammals, most people know of the spectacular battles between
males of some species, such as deer, and of the antlers they
grow for this purpose. There is much more involved, though. The
neck muscles of males swell enormously, their testicles increase
in size. The females go into heat, they ovulate and become
hyperactive and sexually receptive. In short, they enter what is
known as reproductive condition.

The changes always come at exactly the same time of year,
in a given part of the world. The timing of birth is crucial to
successful reproduction. The parents must mate at such a time
that the young will be born when conditions favour their
survival. In the temperate zone there is only a limited time
during which there is warm enough weather, and enough food
available, that young birds can be fed, grow, moult, and be
ready for migration, or prepared to survive the winter. Young
mammals must be born at a time when the mothers can find enough
high-quality food to produce milk. Lactation is an enormous burden on the mother, and again, the young must be able to take care of themselves by the time winter comes. Similar considerations often apply in other parts of the world. The sub-arctic and arctic zones are even more tightly constraining than the temperate zone. In tropical and sub-tropical areas there often are seasonal droughts to be avoided, since they produce marked variations in food supply, just as winter does in Canada.

Although there are very good reasons for animals to time their breeding seasons appropriately, it is not immediately apparent how they do it. A pioneer investigator of this phenomenon was William Rowan, who worked at the University of Alberta early in this century. He was fascinated by the breeding synchrony of the Greater Yellowlegs (*Tringa melanoleucus*), a bird not unlike the sandpiper. It winters in Patagonia, and breeds in northern Canada. Spring migration covers 8000 miles, through weather that varies markedly from year to year. Yet every year their eggs hatch between May 26 and May 29, which gives them just enough time to reproduce, grow new feathers and return south with the new generation, before winter comes. Climate is too variable to coordinate their reproductive cycles, and virtually all other environmental cues are linked to climate. The length of the day follows a very precise annual cycle, however, going from a minimum on December 21 to a maximum on June 21, and back again. Rowan became convinced that the
birds were tuned into the changes in daylength. Rowan eventually established that in the spring the length of the day, or the ratio of day to night, triggers some built-in physiological process that brings birds into breeding condition.

He did this by confining birds in windowless rooms and imitating the natural daylength cycles with artificial lighting. Working with slate coloured juncos (Junco hyemalis) he speeded up the progression of the cycle, so that some of his birds were living in spring lighting conditions while it was still winter outside. The rest of his birds were living under artificial lighting that adhered to the natural schedule. The birds which reached "spring" early went into full breeding condition. The control animals, under an artificial light regime mimicking the natural condition, were in perfect synchrony with the juncos living in the area where he was working.

It has since been established that some species of birds rely absolutely on daylength to time their breeding, while some others have very stable endogenous rhythms. In the former case, we have the white-crowned sparrow (Zonotrichia spp), which will not enter breeding condition without a photoperiodic stimulus, and will quite reliably breed if photoperiodically induced. The latter case is exemplified by the equatorial weaver thrush (Quelea spp), which can be maintained in any sort of artificial light regimen whatever, and will breed synchronously with free-living birds year after year. Some species of birds fall in between, having an endogenous clock that has to be "set" by
Photoperiod.

Mammals usually follow this last pattern. Photoperiodism has been studied far less in mammals than in birds, but some work has been done on deer and sheep. Sheep living in the Southern Hemisphere can be made to breed in synchrony with those in the Northern Hemisphere by placing them under artificial Northern Hemisphere lighting conditions. The effect of photoperiod on deer was discovered when they were shipped from the Northern Hemisphere to New Zealand, to stock the forests with game familiar to the white settlers. White-tailed deer shipped from the United States to New Zealand adapt to the local breeding season, but not immediately. They first attempt to breed, unsuccessfully, at a time between the Northern Hemisphere breeding season and that of the Southern Hemisphere, and the next time around are in step with the local animals. Reverse transfers have reverse effects. Incidentally, soon after being set free in the New Zealand forests these animals became major pests.

During the last ten years some progress has been made in understanding the specific means by which some bird species make daylength into a reproductive trigger. In the case of the mallard duck (Anas platyrhynchos), it has been shown that sunlight penetrates the feathers, skull and brain, and stimulates the anterior hypothalamus. This controls, among other things, the pituitary gland, which in turn controls the gonads. Optical fibres were inserted into the brains of these animals
with one end of each fibre lying near to the hypothalamus. The other ends of the fibres were fitted with light emitting diodes which shone light down the fibres and into the brain. The drakes were kept in continuous darkness, and their brains were illuminated according to the light/dark periods characteristic of various seasons of the year. When their brains were exposed to springtime illumination the drakes entered reproductive condition. They did so at any time of year except during what is called the refractory period. This is the time following breeding, when no animal will respond to photoperiodic stimuli.

The importance of evolution in ecological thought

A central feature of ecology is the attempt to explain the behavior, structures, and physiology of organisms in terms of their evolutionary significance. The ecologist asks such questions as: "Under what conditions did this trait evolve? To what circumstances is it fitted in the present? How much do those circumstances vary, from year to year, and from one place to another? In what ways does this trait vary from individual to individual in one place, and from population to population across the species range?" If we consider the timing of reproduction, it is clear that there are better and worse times for animals to bear their young. The more marked the differences between the seasons, the more important it is that the animal reliably begin its reproductive cycle at the time which leads to the young being born when their chances of survival are
However simple this general point may be, it is not a simple or obvious matter to sort out a given species' adaptations. An animal's present behavior must be seen in the light of its characteristic environment, specific habits, and presumed evolutionary history, which is based on the fossil record and theories of plate tectonics and paleoclimate. In general, mammalian young are born when the mother is most likely to be able to find enough food to produce enough milk to feed her offspring. In the case of temperate or high latitude species, such as black-tailed deer, the best food is available in early summer.

Columbian black-tailed deer are born in May, in southern British Columbia. Lactation load, the amount of milk the mother produces per day, peaks in late June when the young are large, growing quickly, but still mainly dependent on milk. Gestation lasts seven months so that breeding in November guarantees birth in May. At lower latitudes the deer breed earlier in the year and give birth correspondingly earlier; further north the reverse holds. These animals are tuned into the declining length of the short day of fall, which is far more reliably tied to the subsequent arrival of spring than are the temperature, rainfall, vegetation condition, or any other environmental cue available in the fall.

Not all species deal with photoperiodism in this way. Bears (Ursus spp.), for instance, exhibit photoperiodism in
conjunction with delayed implantation. Delayed implantation occurs when the fertilized egg does not implant immediately on the wall of the uterus, but simply sits in the uterus for a period of time, perhaps weeks or months. For this period it is called a quiescent blastocyst because, although it is fertilized, it does not develop until it implants. It is thought that the time of implantation is controlled by photoperiod in the case of bears, but this has not been reliably confirmed. Delayed implantation is thought to provide bears with more time to find one another and mate. Another important feature of the timing of reproduction in some species is a phenomenon called "induced ovulation". Females of carnivorous species like mink (Mustela vison) and, it is thought, bears, ovulate only when they meet males, so that the egg will be in the oviduct when the sperm enters the uterus. Bears are very large mammals, and partly carnivorous, which means that they are scattered thinly across the countryside. This makes it more difficult for them to meet at the right time for mating. If they allow a wide range of times in which to mate, they then will allow a wide range of times of birth, which could lead to some young being born too early or too late. However, if the fertilized eggs can be stored for a period, and all bears then implant at more or less the same time, then birth synchrony is assured within a small range. According to this line of reasoning delayed implantation allows bears a long period to meet and mate and ensures an optimum time of birth for all.
So far so good. The problem is that bears are thought to have evolved in the tropics and spread into the temperate and arctic zones. The fossil record shows the earliest bears appearing in areas which, from plate tectonic and palaeoclimatic reconstructions, were tropical. Then they seem to have radiated into the temperate and arctic regions. This creates a problem for our argument above because in tropical areas bears have no need for elaborate timing of birth. It does not actually seem necessary, and neither does delayed implantation. Yet they have these traits, which is a typical ecological conundrum.

I mentioned above that variations are an important issue for ecologists. As an example, I shall consider bears again. In the area around Yellowstone Park in Wyoming, female grizzly bears (Ursus arctos) reach puberty in their second or third year, and often have two and occasionally three cubs in a litter. In Alaska females of the giant Kodiak brown bears, of the same species, reach puberty at four or five years of age and are less likely to have multiple births. In both areas, there will be variations from bear to bear, in time of birth, time of implantation, number of offspring, age of puberty, and so on. The variations stem ultimately from genetic mutation and recombination. They are evidence of the flexibility of the species, and the source of its ability to maintain itself in the face of environmental change.
The concerns of physiological ecologists

In sum, physiological ecologists try to think in terms of physiological systems within individual organisms (I should mention that there is also a physiological ecology of plants) which were fitted through evolution to prevailing environmental conditions, and which respond to them now. The response to the environment determines a given organism's ability to produce viable offspring—ones which can themselves reproduce. The variation of a trait, and of a response to the environment, within a species is one determinant of the range of variation of environmental conditions that the species can tolerate in the present. It is a product of genetic mutation and recombination, and previous natural selection. Physiological ecologists do experiments on organisms in order to specify the physiological systems, the environmental conditions they are fitted to, and the variations of both. Running through this work is a concern for the evolutionary history of the organisms, for the adaptive significance of the apparent traits of the creatures they are attempting to understand. Ecologists make extensive use of comparative discussions in which different species' mutual similarities and differences are explored. Examples are always tied to species, for instance, grizzly bears, and to places, for instance, the grizzly bears of Yellowstone Park.

Dr. Williamson has usually worked on reproductive strategies—the ways that animals go about the business of continuing their kind. He describes his own work as follows:
My research interest has always been, 'How do they do it?' In other words... you must conceive, you must give birth, you must suckle your young, and it must go. There are certain fixed requirements; all mammals must do this. But they do it under a variety of different conditions, and my question is... what is the variation in their response in reproduction to environmental parameters? So, I would describe myself as a physiological ecologist, I'm interested in how the physiology, the reproductive physiology, of the animals kicks over in response to various environmental conditions. (Interview (5-1-20), September 1978)

Some biologists organize their research careers around one species, becoming very familiar with, say, arctic caribou (Rangifer tarandus), and becoming known as an expert on them, particularly on their migration, for instance. Another career strategy centers on mastering a collection of techniques, such as electrophoresis and radio-immuno-assay, and becoming known for working with these. Williamson viewed himself as having followed a general question, dealing with several species and techniques in pursuit of it. The result, one of his graduate students told me, is that his publications are scattered through the literature and that most readers would not come across most of them, for their reading patterns would be more narrowly focussed than Williamson's publishing strategy. This, he felt, meant that Williamson's reputation was less than it should be. Williamson, in turn, has a low regard for people whose work is dominated by techniques; "technological fiddlers", he calls them, with some asperity.

Dr. Williamson's field does not have exact boundaries. In his work on the reproduction of wild animals he puts his research together using materials drawn from several different
specialties. Most of what is known about reproduction and nutrition in large mammals comes from animal science, a branch of agricultural research. This work is done mainly on sheep, cattle and pigs. As a result, herbivores, and especially ruminants, are the mammals for which the most background data are available. This is because of the great economic value of these animals. By contrast, very little is known about carnivores, for excepting relative oddities such as mink ranching and zoo-keeping nobody raises carnivorous animals as a serious economic undertaking. Williamson also draws heavily upon mainline endocrine physiology. Much of the research in this field is done in connection with the pharmaceutical industry and deals mainly with small animals like mice, rats and guinea pigs. There also is work done on pigs and sheep, for in some respects they are more similar to human beings than are the other cheap and easily available creatures.

Field ecology is the remaining major source outside of physiological ecology itself. This is a relatively new branch of biology. It has recently emerged from natural history, which the people I worked with held in low esteem for being "unscientific". Dr. Williamson and his students were acutely aware that they needed to establish that they were doing "good science". "Good science", as they described it to me, was modelled on a very direct and concrete interpretation of Karl Popper's philosophy. They felt that one did science by setting up hypotheses and devising experiments which refute them, and
considered this approach to demarcate science from non-science. We shall see examples of this attitude in later chapters.

Natural historians, while they do interesting work, rely on anecdotal evidence. Field ecologists spend months or even years living around the animals they have chosen to study, conducting meticulous, systematic observations of their movements, courtship rituals, social structure, eating habits, and selective use of the habitat. Increasingly, these people consider it important to emphasize the scientific nature of their work, and do experiments in the field. For instance, David, one of the men I worked with in this study, has since done a field study of Stone sheep (*Ovis dalli stonei*) in northern British Columbia. His experiments involved burning the sheep's summer foraging areas early in the spring, which temporarily increases the amount of available food. The purpose of this was to determine the effects of increased food quality on the sheep's behavior. Physiological ecology sometimes involves field work, too. This style of field work usually entails shooting and dissecting a sample of the population of interest, a procedure which arouses little admiration among field ecologists.

Dr. Williamson and some of his students found that their standards of operation were at odds, in different ways, with those of both physiology and field ecology. Physiologists criticized the small numbers of animals used in each experiment and the lack of sophisticated controls. Field ecologists were
I was too controlled. In both cases the degree of control was the crucial issue in the assessment. These three fields, physiology, physiological ecology, and field ecology, strike me as lying on a continuum. Physiologists often work with large numbers of white rats, or some other specially bred animal, performing larger numbers of more precisely controlled and monitored experiments than Dr. Williamson and his students could do. Physiologists are more likely to repeat experiments and the details of their procedures will probably be more narrowly defined in relation to the publications of their colleagues.

Field ecology is almost the opposite. By going into the animals' environment rather than bringing them into the laboratory, field workers lose their ability to perform controlled experiments. Michael, one of Dr. Williamson's graduate students, got his MSc by doing a field study of bighorn sheep, and moved on to another supervisor and another kind of animal--polar bears--for his PhD. This is what he had to say about field ecology:

You can't imagine what it takes. We're taught to formulate hypotheses, to control and manipulate. And then there we are, in a situation where none of that applies, we're just out there. It's terribly hard to study ungulates out in the wild. Dave went to study sheep on two summer ranges, and when he got there, one range was completely unused, and the other only had nine sheep on it. Then heavy rains made one river swell up from twenty or thirty feet across, to more than a hundred, and deeper than a horse can ford. He couldn't get into his field site. Once, when he did get there, it started to snow--in the summer, yet--and he had to stay in his tent for days trying to figure out how to stay alive--literally. And when you start what I'm doing,
studying carnivores, you have all the usual problems plus some more. Carnivores are spread out much more sparsely than ungulates, and they're dangerous. I might get eaten for Chrissake! The kind of work that Williamson is trying to do just cannot be done out in the field because you don't have the control. Wildlife biology can't be a science because of this, and that's a problem when you get out there, because you've been trained to think like a scientist, to manipulate and control.¹

The lack of controls makes it difficult for the field ecologist to make sense of what is going on around him. He has to take things as they come, cannot manipulate them. There are not the same artificial regularities to fall back on, he can do little to stop the confusion.

Sometimes the conflicts of standards left the people I worked with feeling that they were caught between a rock and a hard place. David put it this way:

People are yelling at us for having our animals in artificial conditions, and other people are yelling at us for having too many environmental factors out of control. There is a whole range of workers from community ecologists to rat physiologists, and we lie somewhere in between. The people out in an ecosystem try to measure things they can barely find, and they get fucked, and the people in the lab try to measure a few simple things and they get fucked too. (Field notes (line 2323), 27/06/78)

Perhaps because of his greater experience, Williamson was less excitable:

...the real justification is that at the moment, these are the only ways you can find out...about it, and I'm perfectly happy to sit down and listen to a wildlife ecologist on one side saying, 'Ah, but you've got a captive animal', and an endocrinologist who's saying, 'Ah, but you need 483 rats', because I can turn around and say, 'Alright, in time maybe somebody will do this,

¹Reconstructed from notes taken on a conversation in the lab at Western University, July 1980.
have lots of deer, and not in a cage, but at the moment, in the present state of the art, I know more of the art, the endocrinology of deer, even though it is not very well known, than anybody else does. In other words, we know some information that is relevant to what will subsequently come along. (Interview (5-2-24), April 1981)

Clearly scientists attach a great deal of importance to these boundary disputes between fields. The disputes include harsh judgements of the intrinsic intellectual worth of whole specialties, judgements to which ecologists have always felt vulnerable. In the light of this, I think it is important to remember that many of the shortcomings that physiologists or other "harder" scientists see in field ecology and physiological ecology stem from a shortage of money. Williamson put it this way:

If someone from the NASA space organization said "Here's five million dollars, we want a study of puberty attainment in deer", I'm absolutely sure you could purchase all the technology necessary. You could go out in the woods and catch thirty or forty deer, monitor them continuously, have automatic blood samplers on them, as they're wandering around the forest doing their natural thing, and get almost all the information that you can collect in the pen. But there's a money limitation, and you therefore have to bring the animals into an artificial situation, knowing full well that that the proximity business, the nutritional business is not the same as what's going on outside. (Interview (5-2-22), April 1981)

Given enough money the differences between mainline physiology and some kinds of physiological ecology and field ecology would be much smaller. The people from the latter fields could take the sorts of measurements that lab scientists had developed on captive animals, under controlled conditions. There would still be other kinds of studies done--on migration, social structure, courtship and the like, but these too would be
infinitely easier if the scientists could afford enough radio collars, observation aircraft, and the like.

However, the money is not available and the conflict remains. The students in the lab resolved it in their own minds by deciding that lab studies have to be set up to check out phenomena that had been observed in the field, and should give results that can be useful in the field. They also made the competition between specialties into a joke, which is something that people often do when they must live with this kind of stress. David and Michael, two of Williamson's graduate students, decided one day that they were different from the other students in the department. The real biologists, they declared, study large mammals. All other creatures are bugs, and the people who study them are therefore entomologists. This little joke lead to the following conversation between David and Michael's new girlfriend, who was trained in biology herself:

Joanna: Who are those guys over there?
David: Frank and John, they're entomologists.
Joanna: What do they do?
David: Well, Frank's working on foraging strategies of crows, and John studies clams.
Joanna: I see.
At the time I was working with Williamson and his students, they were studying black-tailed deer and bighorn sheep, both major game species in British Columbia. Therefore, the work came under the rubric "wildlife biology", a field defined not by methods but by subject matter—game animals, species that people hunt. Wildlife biology has its own journals and a program of professional certification has been begun to increase the status of the field in scientific circles. This program was actively promoted at the annual meeting of the North-West Section of the Wildlife Society, in Vancouver, B.C., March 7-10, 1978. The major concern expressed at this meeting was that the wildlife on this continent, and everywhere else in the world, is vanishing rapidly, and that if wildlife biologists cannot do more to prevent this they will soon have nothing to do. Much was made of the fact that wildlifers have always had low status among scientists, and it was believed that introducing a program of professional certification of wildlife biologists would improve their standards of practice and increase their prestige and influence.

Not only is their raison d'être vanishing, but the wildlifers' constituency is changing. Hunting is coming under fire and the numbers of non-hunting hikers and photographers is increasing markedly. As a result, wildlifers are showing a new interest in non-game species and trying to come to grips with
what is called "non-consumptive use" of wilderness. At the
Vancouver meeting these people seemed to be unsure of their new
position. President-elect Les Pengelly summed this up at the
business meeting, "The world is on fire and people are bickering
about the kind of paper the Journal of Wildlife Management is
printed on. We have to act like pros if we want to call
ourselves pros." It was apparent, however, that getting
coordinated action from these people would be a difficult and
frustrating process precisely because these people were
professionals, with all of the institutional political loyalties
that professional work almost invariably involves.

Approximately one half of the biologists at the meeting
were faculty members of universities scattered across three
provinces and a dozen states. They worked on widely divergent
research questions and received funding from numerous different
sources. The other half worked for assorted state fish and game
departments, provincial environmental agencies, the Canadian
Wildlife Service, United States federal government agencies, the
Sierra Club, hunting guides and outfitters associations, and so
on. Rather than having a coherent political focus, they worked
for institutions and special interest groups that are often in
conflict with each other.

One of the divisions among them was between university and
government biologists. The latter groups frequently do not
publish the results of their research, which then cannot be used
by academic biologists or even by biologists from other
ministries or governments. The university biologists find this frustrating, for they often do not know of studies that would be valuable to them. Dr. Fred Bunnell of the Faculty of Forestry at the University of British Columbia vented his opinion at the podium in these heartfelt words:

Doing research, gathering data, and then not publishing them, is like standing on a streetcorner pissing your pants. It might give you a nice warm feeling, but it doesn't do anyone else any good.

There is more to this dispute than the sheer unavailability of information. An even more important issue is the usefulness of any research results that might be available. Academic wildlife biologists on the North American continent differ sharply from their government colleagues on the method of counting animals in the wild that they consider to be appropriate. Since the first order of business in any wildlife research is to determine how many animals there are, and where they live, this is clearly a crucial matter. Government biologists follow traditional methods that were developed several decades ago, called quadrat sampling and total counting. The leading academic biologists from Europe and New Zealand have over the last fifteen years advocated what are called transect surveys. The academics argue that transect surveys not only give results that are closer to the actual population, but also that one knows more reliably what one's margin of error is. Having a dependable estimate of error is crucial to any kind of statistical analysis. Whether or not the academics are justified in their position is not a point that I am prepared to argue
here. It is clear, however, that the two methods give very
different results. Government biologists in the Northwest
Territories of Canada have recently begun using transect surveys
to count caribou, and have found that this method gives
population estimates twice the size provided by total counts and
quadrat sampling.\(^2\)

Government biologists do not publish their research in
professional journals. Instead they write in-house reports or
simply file their data. Unlike academic biologists they do not
have to publish to hold on to their jobs, and the ministry that
employs them may want their work to remain confidential. The
upshot of this is that they are not required to justify their
work to a public audience of their peers. They do not engage in
the debates that take place in the journals, and need not worry
about keeping their work in step with the consensus that is
established there.\(^3\) Instead they ally themselves with the
traditions of their ministry and so the academic biologists have
little leverage on them. Some academic biologists consider the
government surveys of animal populations to be worthless in
addition to being very difficult to obtain. The conflict between
methods came to a head at the Wildlife Society meeting in


\(^3\)For examples of the papers on recent developments in censusing,
see: Caughley, Graeme, Ronald Sinclair, and Donald Scott-Kemmis
(1976) "Experiments in aerial survey". Journal of Wildlife
Management, v. 40, #2, pp. 290-300. See also, Sinclair, A. R. E.
(1972) "Long-term monitoring of mammal populations in the
Serengeti: census of non-migratory ungulates". East African
Vancouver, with the presentation of a paper on caribou population estimates. This was work that had been commissioned by the Fish and Wildlife Branch, and done by a university biologist. Apparently, the F&W people had expected this man's results to confirm their own but the opposite occurred. His work markedly contradicted their claims, showing that caribou herds in Canada add to their numbers more slowly than the Fish and Wildlife Branch had claimed. His employers were active at the meeting, attempting to cast doubt upon his results, which angered him considerably, if his harried expression was any indication. Since academics like Bunnell are training the people who go on to take senior posts in these governmental organizations, it is likely that this dispute about methods will be resolved in time.

Half of one day was taken up by a panel discussion of communication, of how wildlifers should make their pitch on environmental management to the general public. One speaker, from the Montana Fish and Game Department, discussed the very frustrating institutional communications problems that bedevil the implementation of any federal environmental policy. In the USA state governments have responsibility for wildlife, but unlike Canadian provinces they have no control over land or water use and lack authority to manage the habitat. He demonstrated the complexity of federal-state exchanges on this

matter with a poster 35 feet in length which displayed the names and connections of all of the agencies and subagencies charged with implementing one piece of federal legislation. The other three speakers stressed the very poor job that biologists typically do of communicating with the public. One stressed public speaking skills, another discussed the mistakes special interest groups usually make in their attempts to use mass media. The third, Mike Halleran, a well-known British Columbia film-maker and environmental commentator, gave a no-nonsense talk on how to leak sensitive information without being caught.

I think that on the whole Dr. Williamson stood apart from these debates. He considered himself to be a pure scientist. To some extent he justified his deer research in terms of its possible applications to game management, but in general he was pursuing his research problems for their intellectual interest to him. He was politically active outside of science, sometimes working for the New Democratic Party on its election campaigns, and he had considerable administrative ambitions, but his specific research interests, as I said, were separate from these for the most part.

Dr. Williamson's research career

When Dr. Williamson began working with deer in 1973 he saw them through eyes conditioned by 14 years of research experience. Naturally, the questions he thought worth asking, and the way he set about answering them, were based to a
considerable extent on what he had done earlier. Therefore, I shall continue filling in the biological background by going over his major papers, briefly summarizing and explicating them. This is meant to be a catalogue of the attitudes and technical resources he had built up over the years. It summarizes the experimental techniques and forms of analysis he had developed or become accustomed to, and the sorts of research problems he considered important and could pursue publicly. This cannot be a complete account, of course, and neither can it be a history in the strict sense, for the reasons discussed in the first chapter. Rather, I portray Dr. Williamson's work as he did publicly, and as I think it often looked to him in retrospect. This assumes, of course, that his view of his work was reconstructed in his own estimation as he reconstructed it for publication, and demonstrations of this will be provided at the end of this chapter, and again in Chapter Six.

Williamson's research began in 1959, with his doctoral work on kangaroos. It was reported in the Proceedings of the Zoological Society of London in 1965. He studied two species of kangaroo that live in a very arid part of Australia that receives its sparse rainfall in extremely erratic bursts. Desert vegetation typically responds to rain with very rapid growth.

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Williamson, Roger (1965) "Reproductive strategies of two species of kangaroo, Macropus robustus and Megaleia rufa, in the desert of northwestern Australia." Proceedings of the Zoological Society of London. (Author's name, and title of paper altered, and details of publication deleted, for reasons of confidentiality.)
flowering and seeding. It cannot rely on sustained rainfall, for there is no such thing, and must make the most of each storm when it occurs. The plants have evolved to be able to carry out their entire life cycle in a matter of a few weeks. When the desert "blooms" in this fashion, there is enough forage for female kangaroos that they can suckle their young successfully. If the young are suckling at any other time they starve because the mother cannot produce enough milk.

Two very interesting features of the reproductive physiology of these kangaroos emerged. One, these species reproduce all year round. That is, at all times some kangaroos were pregnant, some were in estrus, and some others were suckling young. The males were always in reproductive condition, and so any receptive female was able to mate at any time. Two, a female with a young "joey" in her pouch very commonly had a spare fertilized egg in her uterus—unimplanted. If she lost the joey, this quiescent blastocyst implanted. Since kangaroo young are born very early in development, she soon had another joey, ready to take advantage of any milk that might be available.

Williamson noticed this while working with caged animals, and after performing a few simple experiments, reported it in Nature, in 1960, but did not explore its possible ecological significance there. In 1965 he wrote, "The hypothesis is

6Williamson, Roger (1960) "Delayed implantation in some species of kangaroo: the Euro, the Tammar, and the Marloo". Nature, London. (Author's name and title of paper altered, and details of publication deleted, for reasons of confidentiality.)
suggested that the ecological role of quiescent blastocysts is to extend the sequence of parturition under drought conditions. A doe with a joey in her pouch when the rain falls will be able to take advantage of the bloom to suckle her young to weaning. The more often she has young, the greater her chances will be of having one when she is able to raise it. As soon as she bears a young she goes into estrus, mates, and retains a quiescent blastocyst as a backup. If she loses her joey to drought this is not a great physical strain, for kangaroo young are born very, very prematurely and are extremely tiny when they emerge. This makes the kangaroos' situation very different from that of the kinds of mammals we are used to, who have rather large young. The population has a very high rate of fecundity and fertility. This means that at any one time a high percentage of the does are in estrus or have recently given birth.

Correspondingly, there was a high mortality rate of young, for most of them were born at unfavourable times, and starved. Here is the theme I mentioned earlier—how do animals arrange to have young at a time when they can be raised to independance? Delayed implantation and short gestation help these animals to deal with random rainfall. As noted before, delayed implantation serves different specific purposes for bears. The timing of breeding provides another variation on this theme. Black-tailed deer, and all other temperate zone mammals are obliged to seek one narrow range of time for breeding because of the seasonal cycle. The kangaroos Williamson studied are equally compelled to
breed throughout the year in order to achieve the same general end under very different specific circumstances.

Williamson's post-doctoral work was done in Canada on the population dynamics of deer mice (*Peromyscus maniculatus*). He worked on a long-standing problem in ecology: just what controls the number of animals in the wild? Small rodents have been particularly intriguing in this respect. The migrations of lemmings and the cycles of rabbits are relatively widely known, outside of ecological circles, and population control of small mammals is an area of more or less permanent controversy within the profession. Williamson worked for a man who is well known for advocating that genetically based behavioral changes during the population cycle are responsible for regulating numbers of animals. He has argued that as population density increases succeeding generations of mice are more aggressive. This aggressiveness drives away young mice, which suffer high mortality on unfamiliar territory.

When doing population dynamics Williamson considered the composition of a free-living population of mice and the changes in numbers and composition of that population. The composition of a population refers to the following:

1. the age structure of the population: the relative and absolute numbers of animals in each age class: infant, juvenile, sub-adult, adult.

2. the sex structure of the population: the number of males and females in each age class.
3. the reproductive condition of the population: how many females are pregnant, lactating, or in estrus; how often have they given birth before, and to how many young; how many males are in reproductive condition?

All of these considerations are specific to the time of year. For example, he might ask what proportion of adult female mice are lactating in October, or what proportion of adult males are in reproductive condition in May? Two more central issues are recruitment and migration. Recruitment is the rate of entry of the latest generation of mice into the breeding population. Migration refers to the movement of mice into the study area from somewhere else, and to the reverse, as mice in the study area leave for other places.

The pre-eminent question organizing these concerns is: what processes govern the numbers of animals present in a given area? and the major methodological problem is the deceptively simple one of finding out how many animals there are right now. It is no mean feat to do a census of a population of three inch long nocturnal animals whose major skill is hiding. Williamson used grids and lines of traps, some captured the animals alive, some were conventional mouse traps, which killed them. He used the latter when he wanted animals for dissection, but as a rule the mice were captured, ear-tagged, and released.

Fortunately for Williamson, and other researchers, deermice are "trap-happy". Apparently, they like nothing better than to spend a night in a live-trap. Perhaps this is because of the
peanut butter and cotton wool thoughtfully provided for their comfort. In fact they are likely to die of exposure without them, even in summer, for they have extremely high metabolic rates and their tiny bodies lose heat rapidly. Ear-tagged mice could be identified when they showed up in the traps again, which made it possible to keep track of the local population. The key assumption here was that every mouse in the area (a hectare or so) would become acquainted with the traps and use them frequently. Mice which stopped coming were assumed to have died or migrated, and adult mice which suddenly appeared were assumed to have immigrated recently. When mice were caught they were examined for age, sex, reproductive condition, and parasites. Snap-trapped mice were dissected and females were examined for the marks on the ovaries and uterus that indicate previous ovulations and fetuses, and males' testes were sectioned and examined under a microscope for signs of spermatogenesis.

Williamson did standard population dynamics research, trapping in a forested area on the campus where he studied. Following upon the interests of his supervisor he did lab studies of aggressive behavior as well, developing a measure of aggressive interaction between mice. They were placed in a simple maze which required them to meet and establish their position with respect to one another. He tested male mice captured from his study area at different times of the year and found an increase in aggressiveness during the breeding season.
Juvenile male mice confronted by adult males at these times would often die of shock following the encounters. He reasoned that survival and recruitment of young were subject to changes explicable in terms of adult male aggressive behavior.

Williamson's next move took him to the Wellcome Institute of Comparative Physiology, which is associated with the Regent's Park Zoo, in London, England. There, as he puts it, he learned some physiology but no ecology. He found that a zoo is not a good place to do his kind of research because collections of zoo animals cannot be set aside from public display for long periods of time for experiments. He had to content himself with anecdotal reports of isolated events such as the induction of ovulation in a lioness, and to investigations of artificial insemination of exotic species. This work kept his name in the journals. Meanwhile, he was writing a review monograph on the ecology of reproduction which did a great deal to secure his reputation.  

The book is a selective review of the available information on domestic and wild mammals. I shall not analyse the book here, but I shall comment on how Williamson handled the subject areas that his work dealt with while I was with his group. He made no mention of precocious puberty in black-tailed deer, although reports of this phenomenon go back as far as 1943. In the section "The effects of nutrition on the onset of puberty", he

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cited evidence that free-living mule deer (black-tailed deer) breed at 17 months under good conditions in California, but not until at least a year older on poor range. Other evidence suggests that the severity of a deer's first winter governs whether it will breed the next fall or have to wait until later. He did not mention their breeding in the first fall, at 6 months of age, in captivity. Such a finding is so striking that if he had known of it he surely would have mentioned it. His own recollections confirm that he did know of precocious puberty in female black-tails when writing the monograph.

A later section, "The effects of nutrition from parturition to weaning", clearly stated concerns that were evident in 1977, and which also appeared in the mouse work of the early 1970's. Pregnant and especially, lactating, mothers must eat very heavily, and if they cannot, will drain their bodies of the necessary nutrients to maintain milk quality and quantity. This goes on for some time before lactation failure sets in. If high quality food does not eventually become available the mother does not allow herself to decline any further in condition and the fawns starve to death.

There is a sort of evolutionary choice point available to her. Almost nothing will terminate a pregnancy. The 'choice' comes later, during lactation, which is a much greater strain than pregnancy. Some of the strain of lactation appears to be unavoidable. In some species the period of lactation is very short, and intense, and the mother simply cannot eat and digest
enough food to match the output of milk. She must draw upon her own reserves of calcium, proteins, fat and carbohydrate and recharge them later. The seal is an extreme example of this, for she does not eat at all during lactation. Mice are in the opposite position. They have very little in the way of reserves and may eat as much as four times the usual amount while lactating. In 1977 we found that black-tail deer increase their food intake by about 70% while lactating, but still lose some weight even when they are well fed.

If conditions are poor the mother must terminate lactation at some point rather than going downhill so far as to seriously jeopardize her chances of surviving the winter and reproducing again. Over many generations this is balanced against the possibility that she will be pulled down by a predator in the meantime, or die in an exceptionally severe winter, which means that she should make the most of the present chance to produce viable young. The food shortage, after all, may be temporary. One of Williamson's strongest research interests is in determining the energy efficiencies of these kinds of strategies, and the point at which it is better for an animal to leave one strategy and go to another. If the doe shuts off milk production too early, if the food shortage is temporary, she may be throwing away a chance to raise her young to independence. Whether she shuts down or continues, she has done something that will have definite consequences: she raises the young successfully or the fawns die, or she and the fawns die. The
assumption that ecologists make here is that the point at which the doe breaks off lactation is affected by her genetic endowment. The animals who live long enough to produce offspring which in turn survive to reproduce will pass on that genetic inheritance which favours continuing lactation in the face of food shortages up to the point that has worked best in the past in that area. Life, it seems, is a bit of a gamble.

When I worked with him Williamson speculated often on the efficiency with which mammals convert their food intake into biomass of young. This would influence choices such as the one mentioned above. A crucial point in this regard is whether or not the young produced are viable, whether or not they reproduce too. An organism which produces young which do not themselves grow up and reproduce has not continued its genetic line, and is an evolutionary failure. Black bears (Ursus americanus) provide an example of this. A sow who produces four cubs instead of two may be knocking herself out to raise undersized, somewhat malnourished young that are not strong enough to survive the winter, reach puberty, and reproduce viable young. She may not have "beaten the odds" by producing more young if the chance of any one of them surviving is too small. A litter of two would produce larger, stronger cubs, each of which would have a higher probability of survival than any one of the larger litter.

Clearly there is a trade-off between producing many vulnerable young and a few high quality young. Different kinds of animals are found at different points on a continuum between
these two extremes. Mammals, in general, follow the small numbers/high effort option. An example of the opposite would be mosquitoes, which lay vast numbers of eggs and leave them. Within a species the same choice is sometimes available, within a limited range, and it is these individual "decisions" which eventuate in evolutionary trends. Williamson wanted to know what sorts of previous adaptations and present conditions combined so that an animal would follow one possible strategy instead of another.

The questions he posed in 1977 fitted into this pattern. He wanted to know how much energy it took to raise a fawn, and how much of it came from the mother. He also asked: when, during the summer, does the fawn become able to survive on solid food only, what is the youngest age at which the fawn can be nutritionally independent? Furthermore, if this age is different from the usual age at weaning, why is it different? This question differs in detail from the one outlined earlier, in being more narrowly focussed. But it is relatively clear that by 1969 he considered this sort of issue to be important. Working with it called for an input-output analysis of energy in organisms' lives, and this he began to do with mice in 1970.

Williamson returned to Canada in 1969, to take up a teaching appointment at Western University. He remained there until 1981, when he took a government research director's position in another country. His first research at Western was a small population dynamics study on deer mice. It was conducted in
the forest on the university campus, and gave results similar to
the trapping work done several years before, but did not include
lab work. It was published in *Syesis* in 1970, at the same time
that an analysis of some data from his post-doctoral work
appeared in *Animal Behavior*.

In 1970 he began a far more ambitious project, one which
combined the skills he had developed in reproductive physiology
and ecology with those in population dynamics. He studied deer
mice in three different habitat types in a coniferous forest
located in a forestry research station that belongs to a
neighbouring university. "Habitat" refers to the kind of
environment that an organism typically lives in. There is no
simple definition of the term because what counts as a habitat
depends on which species you consider. For some species of
lichens, the north side of a tree is a very different place from
the south side of that tree. In the Northern Hemisphere the
north side is cooler and damper, the lichen can live there, and
it is a habitat, while the south side is inhospitably warm and
dry, and therefore is not a habitat. A deer living near that
tree will not be affected by a move of a few inches, but may
find life on the north side of a mountain to be different from

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*Williamson, Roger (1970) "Reproductive activity of the deermouse
* _Peromyscus maniculatus_ on the Western University campus".
*Syesis*. (Author's name and title of paper altered, and details
of publication deleted, for reasons of confidentiality.)

*Williamson, Roger (1970) "Dominance hierarchy in the male
deermouse (*_Peromyscus maniculatus_*). *Animal Behavior*. (Author's
name and title of paper altered, and details of publication
deleted, for reasons of confidentiality.)
life on the south side of the mountain. The north side of the mountain, in the northern hemisphere, will be cooler and in relatively dry climates will be more densely forested than the south side because rainfall evaporates less quickly on the north side. The deer would find these to be quite different habitats, and would likely spend more time on the more open southern slopes.

The habitat types Dr. Williamson chose were mature coastal coniferous forest, recently logged forest, and young forest. He set up traplines in each habitat, and examined the deer mouse populations. The papers coming out of this research portray him looking at reproduction, energy expenditure, nutrition and population dynamics, trying to see if the mice behaved differently from one habitat to the next. In other words, are these environments which seem very different to us, different to mice?

The first major paper coming out of this research appeared in the Journal of Reproduction and Fertility in 1973. It addressed speculations to the effect that mouse breeding seasons are determined by food supply, that if they had enough food mice would breed all winter. This means, among other things, that they are not under photoperiodic control. Williamson and his

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10 Williamson, Roger (1973) "Input/output analysis of the protein and calories required for breeding by wild deer mice (Peromyscus maniculatus)." Journal of Reproduction and Fertility. Written in collaboration with two graduate students. (Author’s name and title of paper altered, and details of publication deleted, for reasons of confidentiality.)
students found that there was not enough food in any of these habitats to permit the mice to breed all winter. He worked out estimates of the energy requirements of the mice, in pregnancy, while lactating, and for non-pregnant mice. These estimates primarily concerned females; for the numbers of fertile females determine the birth rate, and the numbers of males are less crucial. They produced estimates of energy consumption and compared them to the estimates of energy expenditures.

I shall outline the means used, to give an idea of the kinds of procedures and detailed concerns that go into this sort of research. Lines of live-traps and snap traps (ordinary mouse traps) were set up. At times the live traps were fitted with electrical switches that went off when a mouse entered the trap. This caused a light to go on, on a map of the trapline that was located in a trailer where a graduate student kept vigil. Seeing the light go on, the person in the trailer would go out to the trap line and release the animal after a specific period of time. Frequency of trapping provided a measure of activity. It also gave an indirect measure of food consumption. For a time the mice were left in their traps for an hour and the feces they deposited during that hour were collected and weighed. A laboratory study was done on some captured mice to determine the correlation between food intake and excrement output, which turned out to fall in a consistent-ten-to-one ratio. This permitted an estimate of the rate of food consumption of the free-living mice which had been caught in the traps. The stomach
contents of snap-trapped mice were examined, and the digestibility of the wild foods determined by laboratory analysis.

The reproductive condition of the animals was monitored. When animals were removed from live traps the females were checked for estrus, pregnancy and lactation, and the males for active testicles. Snap-trapped animals were dissected. The females' ovaries were examined for enlarged ovarian follicles, a sign that ovulation was soon to occur, and for corpora lutea, scars left behind by previous ovulations. Their uteri were examined for the scars left where previously borne fetuses had been attached. The males' testes were dissected, and examined for signs of spermatogenesis. The metabolic requirements of activity were estimated from body size, body temperature, and ambient temperature. The metabolic requirements of pregnancy and lactation were taken from estimates in the literature for other species of mice. All of these criteria of breeding condition, and most of the metabolic considerations were carried over into the work on the deer.

Following this paper were two others that appeared in the same issue of the Canadian Journal of Zoology, comparing the three study sites.11 The first, on population dynamics, found

11 Williamson, Roger (1974) "Deermice (Peromyscus maniculatus) in a coastal coniferous forest, I: population dynamics". Written in collaboration with a graduate student. And, "Deermice (Peromyscus maniculatus) in a coastal coniferous forest, II: Reproduction". Both in Canadian Journal of Zoology. (Author's name and titles of papers altered, and details of publication deleted, for reasons of confidentiality.)
two sites similar, and the third different, apparently because a competing species of mouse lived there. This paper covers much the same kind of ground that I described with regard to the first papers on population dynamics, so I shall not delve into it further. The other paper was an account of the reproductive status of the mice on the three plots over the four breeding seasons of the study. The timing of breeding was examined and all three sites were found to be in synchrony. That is, breeding began and ended at roughly the same time on all sites. Breeding was found to be independent of population density changes, and independent of parasitism of the mice by bot flies. This last appears because some other workers had published the claim that these flies, which lay their eggs in the tissues around the nose, significantly influenced the lives of the mice. In fact, it was believed that such parasitism was associated with greater longevity of infested mice, an assertion which Williamson refuted. The three sites were found to have similar breeding status to one another through changes in rainfall and food availability.

This kind of close examination of habitats which do not appear to be very different may seem rather odd to the non-biologist, and the conclusions, that the mice did not vary much from one site to the other in their life cycles, obvious. One must remember, however, that the theory of evolution by natural selection postulates that small changes of adaptive advantage, of fitness, will eventuate in the genetic type of one.
small local population spreading throughout a species. This sort
of theoretical position makes it reasonable and worthwhile to
try to pick out small differences among the ways that different
populations of a species deal with somewhat different habitats.
Ultimately, scientists do try to test their theories, but such
tests cannot be conducted until techniques have been developed
which can discriminate the sorts of phenomena upon which the
test would depend. Williamson's papers indicate that the state
of the art he practiced did not then show this species of animal
to be behaving noticeably differently in different related
habitats. Doubtless he hoped for more interesting results, and
although his work produced publishable papers, it represents a
gamble that did not pay off as well as he had hoped.

The last paper coming out of this research was a methods
piece, on the usefulness of stomach content variation as an
indicator of variation in food consumption in the wild. That
is, can one find out how much food consumption varies from month
to month or from place to place by looking at how full the
stomachs of mice are, from place to place or from time to time.
The major technical problem with this kind of work is obvious as
soon as one tries to measure such things as food consumption and
movement of tiny, free-living, shy, nocturnal animals. It is
extremely difficult to do this on large animals like moose.

Williamson, Roger (1974) Deermice (Peromyscus maniculatus in a
coastal coniferous forest, III: stomach weight variation". Canadian Journal of Zoology. (Author's name and title of paper altered, and details of publication deleted, for reasons of confidentiality.)
bears, or deer, but mice are even worse. The paper on stomach contents showed the technique to be unworkable. A large majority of the mice had stomachs that appeared to be one-quarter full. It was impossible to tell what a full stomach might be, in a live animal, by looking at the excised stomach of a dead one. Variation in content weights was found to have no discernible relation to the onset of breeding season, or to reproductive status. This was the last paper Williamson wrote about the mouse study and by the time it came out, the deer project was under way.

Switching to deer

The events of the transition from one project to the next are for the most part impossible to verify, but some of the reasons for stopping the work on mice are clear. In an interview with Williamson in 1978, I asked him how he got involved with the deer work:

I know I made one conscious decision in my research career... when I was working on mice, population dynamics, reproductive studies on mice, particularly environmental effects on reproduction. I became very aware of some distinct limitations to the sorts of questions that I wanted to ask, answer, which you simply couldn't do with mice because they were too bloody small, simple as that. Let me give you a very good example. I was working on what was regulating the breeding season in mice. What were the environmental factors which altered the breeding season? And we came down to, after four years work, to a consideration of two things, the variation in the seasonal food supply, plus the effect of low temperatures on food demand, for these animals. In other words it looked as though they stopped breeding, stopped lactation when it got so cold that they were out feeding all of the time to get enough food for their own metabolism, and they couldn't afford
to get extra food for lactation. Now this meant then measuring quite accurately the temperature of the environment that the mouse was in, and that is a bloody difficult thing to do. It means you have to put little transmitters on the mice, and trace them down their burrows, and work out the amount of time they spend running around on the surface against the time they spend in their burrows. At that time there wasn't any physical way of doing it. The smallest radio transmitter you could buy was two grams. Well, the bloody mice are twenty grams, you can imagine putting a two gram transmitter on its back! So I also thought generally, a little bit, um, what's the word, a bit, I got disillusioned, I got sort of sick of working on mice. (Interview (5-1-2), September, 1978)

To proceed any further with the mice he would have to be able to follow their activities very closely indeed, and that was not technically possible at the time. He had reached an impasse, and could go no further. The technical substance of these remarks echoes what he wrote in the journal article on stomach weights, which appeared in 1974, when he was already working on the deer.

If stomach weights are to be used to determine, indirectly, consumption rates in wild rodents, it is necessary to measure the factors influencing their variation. This will require detailed field observations of diurnal patterns of feeding activity and their relationship to rates of passage of food through the alimentary tract in free-living animals. At the moment, no techniques appear to be available to accurately measure these two variables in small nocturnal rodents, such as the deer mouse. Therefore, stomach content weights must be considered a somewhat deficient method of indirectly determining consumption rates. We have shown elsewhere that the rate of faecal production is a useful indicator of consumption rates in the wild, but that technique also requires good independent measurements of diurnal activity patterns.  

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13Williamson, Roger (1974) "Deermice (Peromyscus maniculatus) in a coastal coniferous forest, III: stomach weight variation". Canadian Journal of Zoology. (Author's name and title of paper altered, and details of publication deleted for reasons of confidentiality.)
This of course leaves out another message that came through in the interview. He had gone the course with mice. He was fed up with those questions and disappointed with the banal results of the comparative study. It was time to move on.
V. Creating the scene: the people and the deer

In this chapter I shall introduce the people who built up the deer project. They are Dr. Williamson, his graduate student Karl Voelker and the animal care worker Elaine Garfield. The purpose of this chapter is to portray the network of connections among them that had been established by the time I arrived, and simultaneously to sketch in the outlines of the project that was embedded in and partially constituted by the evolving social scene. Critical analysis of the way these people explained the origins of the deer project will be left to Chapter Six. Before approaching it I shall reconstruct the setting I entered in 1977, in which the stories about the project’s origins were told to me. In this chapter I shall portray the physical and social setting, and in the next chapter the experiments that were done and the conclusions drawn from them as this scene was built up by the people working in it.

Karl

The deer project came about because of a collaboration between Dr. Williamson and Karl Voelker. Karl arrived in 1972 to do a PhD on large mammals, but as is not uncommon with graduate students, had no particular species or project in mind. His background was unconventional, lacking the usual BSc, MSc
progression. He had been trained as a veterinarian in West Germany, graduating in 1963, and had then worked as a zoo vet in Liberia, West Africa. As an extension of his work in the zoo, he had begun to capture wild animals. In 1966 he came to Canada and helped to establish a major game farm. He then worked on another game farm and moved from that to his position as a doctoral candidate. He was of an age with Williamson, well versed in exotic animal husbandry, and a rather independently-minded man. It seems unlikely that he wanted to begin a career as a researcher. Rather, according to Williamson, he wanted to be a zoo director, for which he needed high-level professional certification in biology.

Karl did not begin with work on the deer, but on a totally different and unrelated project, lungworm parasitism of a herd of bighorn sheep (Ovis canadensis californicus) in southern British Columbia. Lungworms are just what their name suggests, worms that live in the lungs. They are also known as strangulates. Free-living sheep, moose, deer and the like usually are infected with them, often quite heavily, but usually without obvious adverse effects, although lungworms can be associated with a form of pneumonia. It was thought that the sheep in this herd might be heavily parasitised, and that this might have something to do with the herd's low lambing rate. The herd's reproductive rate is the lowest of any in the province, and for some years the Fish and Wildlife Branch of the Ministry
of the Environment of the province of British Columbia¹ has been trying to determine why this is so. Bighorn sheep are the highest profile game animal in the province, and the Branch aims to increase the hunter harvest of these animals. This herd has a lambing rate of about 20 per 100 adult females, compared with rates of from 40 to 75 per 100 in other herds. The Branch has purchased the grazing rights for the sheep's winter range, and excluded cattle from it. They have burned the range to improve its quality and supplemented the sheep's feed in winter. Studies of the forage have been commissioned. So far they have spent an average of $100/sheep/year for some fifteen years on this herd without notable success in either raising the lambing rate or finding out why it is so low.²

Karl went out to determine the severity of the infection of the herd with lungworms. It turned out that all the animals were infected, but only lightly, which eliminated the possibility that the low lambing rate could be a response to heavy parasite load. He also found it very difficult to locate the alternate host of the worm, a species of snail. To cap it off that year a publication came out from the Wildlife Research Centre in Fort Collins, Colorado, which scooped his work. He and Williamson decided to abandon the problem. Fortunately, Karl was only a few months into this work and so did not lose very much time, but it

¹Formerly known as the Department of Recreation and Conservation.
²MacDonald, Michael (1980) MSc. thesis defense.
was still too late to develop another project in time for the 1972 breeding season. This meant that any project on reproduction would have to wait for the next year. While all this was going on, Williamson was winding down the mouse research. The graduate students who worked with him had finished their dissertations. Williamson was casting about for new directions.

Starting the deer herd

He and Karl decided to establish a breeding herd of deer for experimental purposes. In the spring of 1973 the two of them went collecting fawns from the wild to form the nucleus of their herd. The technique for collecting fawns is quite simple, and depends on two well-known habits of deer. Firstly, they move about most at dawn and dusk, and are most likely to be seen moving across open spaces at these times. Secondly, fawns rely upon camouflage and stillness to protect themselves from predators. They are covered with spots that make them hard to see, are virtually odorless, and when frightened, they fall to the ground and huddle motionlessly, no matter what happens around them. This huddling is called the fear posture. When a doe senses danger, she bolts and the fawns drop (she usually has twins). If all goes well, the mother returns to retrieve her young ones later. Catching fawns was largely a matter of cruising the deer's habitat at dawn and dusk to flush does with
fawns, and then searching the undergrowth where the doe was first spotted, hoping to find the fawns. The men drove along logging roads in a selected district of Vancouver Island, hoping to encounter deer crossing the road. One man drove and the other rode standing in the box of the truck. When he saw a doe he signalled to the driver, who stopped and went to the place the deer had been seen, under the direction of his partner in the back of the truck, who kept his eyes fixed on the spot. If the driver found a fawn he would bring it to the truck, put it in the cab, and continue driving.

It has been my experience that fawns do not hold the fear posture indefinitely in the cab of a truck. After ten minutes or so they begin to seek greener pastures, sometimes vigorously. With their cargo of jumping jacks the two would return to the small-town hotel where they were staying. They weighed and ear-tagged their charges and put them in makeshift cages behind the hotel. At frequent intervals they hand-fed the fawns and recorded the amounts of milk consumed. The deer attracted a great deal of attention from the hotel clientele and other locals. Deer traditionally are a very popular animal with the general public, even without the assistance of Walt Disney and his protege, Bambi. In addition, deer are the most commonly taken mammal game animal on the continent, which is reflected in the large volume of research done on deer, much of it in the name of wildlife management. This was an important consideration for Karl and Williamson, for it meant that there was
considerable background information available. The high profile of deer means that money has been more readily available for studying them. It was also the focus of some criticism at the Wildlife Society meeting mentioned in the previous chapter, for deer are considered by some wildlife biologists to be over-studied, to the detriment of research on other species.

The animals were taken back to the city where Karl hand-raised them in his backyard. He kept regular records of their weight and milk consumption. As well they were given free access to commercial dairy feed and alfalfa hay, for the fawns begin eating small amounts of solid food long before they are weaned. This is crucial to their post-natal development, for they are born with a single-chambered stomach such as we have, and cannot develop the multi-chambered stomach of the ruminant without the stimulation of solid food. A ruminant is a cloven-hoofed animal such as a sheep, deer or cow, that has a stomach divided into four inter-connected specialized chambers. The first chamber downstream from the mouth is the rumen, for which ruminants are named. In it live bacteria and protozoa, the rumen flora. These single-celled creatures secrete cellulose digesting enzymes that mammalian digestive tracts do not produce. Downstream of the rumen, there is normal enzymatic digestion, such as we have, that breaks down proteins, fats, and carbohydrates other than cellulose. Having a rumen enables a deer to get more from the food it eats, for the cellulose that is digested would otherwise pass through unaltered. The rumen
flora lives in a symbiotic association with the deer. They live off the food the deer eats, and in so doing break some of it down into molecules the deer can absorb. When a deer is born it has an undifferentiated stomach, and for a limited time after birth this stomach will respond to the presence of solid food by differentiating into four chambers. If the fawn gets only milk throughout this time the stomach will remain as it is, and the animal will never become a functional ruminant.

In order to hand-raise the fawns Karl had to settle upon an adequate substitute for doe's milk. It might surprise the reader to know that there is a scientific literature on the bottle-feeding of captive deer. One of the features of scientific research that must be understood is that minute details of the experimental situation are subject to examination and control to a degree that may appear either sublime or ridiculous, depending on the attitude of the observer. Many combinations have been tried by other workers, and Karl chose to use evaporated cow's milk, diluted with water when the fawns were quite young and given straight when they were older. Cow's milk is higher in sugar (lactose) and lower in fats than deer's milk. The fawns on Karl's diet were prone to develop an infection of the digestive tract, called bacterial scours, because the bacteria which normally live in the lower intestine, Escherichia coli, multiply very rapidly when deluged with the excess sugar in the milk. This population explosion of bacteria irritates the lining of the gut, causing diarrhea and
dehydration. The young of any species are very vulnerable to dehydration and will die quickly if the diarrhea is not stopped.

Karl combined old with new by giving the fawns chamomile tea and chloramphenical, an antibiotic, in a treatment that was successful in all cases.

Health care was a constant background concern in maintaining the herd. Some diseases are more common in captivity than in the wild because of crowding. An example is coccidiosis. This is another digestive tract infection which struck the fawns, killing some of them. It is caused by single-celled organisms called coccidia, which live in the digestive tracts of some adults. These pass out of the animal in its feces, and are picked up by the fawns when they eat the adults' feces. This is how the young ones acquire their rumen flora, but it also passes on any pathogenic organisms that might live in the adults' tracts. Adults can tolerate coccidia without much difficulty, but the young develop diarrhea, with its attendant dehydration.

In captivity one adult infected with coccidiosis can spread it to all the fawns. Karl treated them with a sulfa drug but some fawns died.

Elaine

Over the years Karl and other people associated with the project established customary ways of taking care of the deer. Karl's major partner and opponent in this was Mrs. Elaine.
Garfield. She was hired early in 1974 to be the animal care attendant, to feed and water the deer, clean out their pens, and keep an eye on their condition and report any problems to Williamson and Karl. She carried out these duties with extraordinary devotion and meticulous care. This was crucial because reliable, high quality animal care is absolutely essential to maintaining the stable, controlled situation that makes research possible. Over time the developing routines of animal care devolved upon her. In fact, she still tends the deer, the only person from the formative years of the herd working on it now. When Williamson left research he turned the deer over to the administration of the Research Forest, who passed it on to other professors.

Someone who is very concerned with the welfare of the animals and settled in her own habits is ideal for this routine but indispensable work. Elaine is a long-time resident of the area immediately adjacent to the research forest. A woman of strong character and even temperament, married and the mother of a grown son and daughter, she took a marked and lasting interest in the animals. Tending them was not merely a source of income for her.

McKegney: Once you had someone like Elaine Garfield in there, to maintain the herd was not difficult.

Williamson: Well, she wasn't the first person we had. We had another girl before her, who created all sorts of problems. I think she was on drugs, actually. She was just totally unreliable. We were very lucky indeed to get Elaine very early on. Since then it's been easy as pie. These people have a very important part in projects like this because simple day-to-day maintenance of
animals is time-consuming and boring, and can be tedious. Unless you have a real interest in the animals it's not a job you'd recommend to anybody, yet she's really good, she has a great interest in the animals. And is an excellent person to work with. (Interview S-1-4, September 1978)

Her involvement with the project was a family affair—a bonus for Williamson. Elaine got the job because her son Tom had worked with Williamson's graduate student some years before, helping him to catch mice. When Williamson advertised for a deer attendant, Tom applied. Williamson was surprised, for he had not thought Tom would be interested, and had given the job to the woman mentioned above. When she became unreliable Tom took over from her until he finished high-school and got a full-time job. Then his mother took over from him and has been there since. Her husband Cameron helped Karl build the deer pens and the two became friends, staying up late at night and drinking wine together, talking about their experiences in the bush. Elaine, Cameron, and their son and daughter were instrumental in returning the deer to their pens in 1974, when they were released by persons unknown who smashed the locks on the pens. Williamson was in Australia and Karl in Africa at the time, and without the Garfield's initiative—retrieving the deer was no simple matter—the project would have foundered.
The special world of research

Elaine and Cameron are not scientists and their attitudes to the deer and the work done on them contrast with those of Dr. Williamson and his apprentices and other helpers. These latter people were in the business of transforming ordinary common-sense animals like deer into arcane objects of scientific interest. Much of this transformation is inherent in the special circumstances they created for the deer to live in and the special uses to which the deer were put. In a very concrete way they created a special world for the deer, and for themselves. This world took several years to build, and for each new research project it had to be adjusted, added to and then repeatedly reconstructed and streamlined while the project was underway. Its characteristics were frequently examined and re-negotiated. I consider this to be typical of scientific research. In fact, in most fields of biological research the organisms, or parts of organisms, under examination are studied in far more contrived and minutely controlled environments than was the case with these deer. Strangely enough, it is the practitioners of such fields, molecular biology is an example, who feel that they have the strongest claim to knowing what the world is really like, what it is like independently of their attempts to know it. As we have seen, scientists from such fields would say that the world Karl, Emily and Dr. Williamson had created was not controlled rigorously enough, which means in
FIGURE 1: Two views of the deer barn.

a:

- pole
- pole
- asphalt
- doe pen
- fawn pen
- work area
- scales
- narrow door to fawn pen: open
- wide door to doe pen: open
- covered feedbox
- feedbox
- chainlink enclosure
- roofline

b:

Scale:

0  6   12
(feet)
effect that it was not artificial enough, while others would say that it was too artificial. Leaving these squabbles aside, let us consider how this special world looked to non-scientists. In this way we may be able to see how much material control and regulation is a part of scientific research.

The contrast of attitudes is immediately apparent in the case of the deer barn. There are diagrams of the deer barn on the facing page. Note, in Figure 1 (a), that the doors which lead from the central work area to the individual pens are quite low and narrow. It is just the right size to be completely covered by the door of a deer shipping-crate. Deer could be moved into and out of the pens easily, without their escaping, and they could be weighed by putting them in a shipping-crate and weighing the crate in a spring scale suspended from a rotating overhead arm. The arrangement is ideal for handling the deer, which are extremely agile, and can squeeze through startlingly small gaps. It has no other merits whatever. In fact it was a serious hindrance to any human being more than four years of age who wanted to enter or leave a pen. Elaine and Cameron were vocal on this subject:

McKegney: Well, did Karl actually build those pens?

Elaine: I think he did. He designed them, because we used to kid him about how poorly they were designed, anything but logical, because you had to squat down to get into the pens.

Cameron: They were built for the convenience of deer, but they certainly weren't built for the convenience of people.

McKegney: I practically had to get down on my hands and
knees (to get in).

Cameron: I don't think there was a person there, when they started, who didn’t wind up with big bruises all over their heads and back. There wasn’t a place you could wheel a barrow in, everything had to be carried.

Elaine: He said he designed it, we told him he shouldn’t be so proud of it. (Laughs)

Clayton: Matter of fact, the first time I saw them, that was my first criticism, because I used to do a little designing, houses and that sort of thing. I said, WHO is responsible for this. And there was a long silence. And I thought afterwards, maybe he did it. (Interview (E-1-9), March 1980)

Emily viewed the animals differently than did most of the other people working with them, although Karl, the husbandman, was much like her in this. The deer were her charges, her "babies", as she put it. Some of the animals imprinted on her, that is, at an early age they formed a highly specific attachment to her, and would come to her although they would approach no other human being.

Cameron: If you get to working around them, you see their personalities. They get so they know you and you know them. A stranger comes, they reject the stranger and they'll come to you.

Elaine: I had one there that wouldn't eat for anyone else but me.

Cameron: Yeah, it was stuck on Elaine for its entire life, the imprinting didn't leave when it became an adult. (Interview (E-1-5), March 1980)

One consequence of this, and Elaine's main substantive impact on the research was that she very much disliked any kind of research that harmed the deer or required that they be killed.

Elaine: They sent fawns to Edmonton.
McKegney: To the game farm.

Elaine: No, they were testing them for something to do with the kidneys, injected them with something.

McKegney: Edmonton, University of Alberta has some people who are pioneers in kidney transplants.

Elaine: And these deer, they injected them with something, to see what their reaction was, to see if they could stand.

McKegney: How do you feel about that sort of thing?

Elaine: I don't like it. I wouldn't have raised them, at the beginning. And then I raised two of them (for that work) and I was really upset. (Interview (E-1-4,5), March 1980)

Karl's interests in the animals were often different from Elaine's, but he shared her basic affinity for them and her dislike of harming them.

McKegney: Karl wasn't into the kind of experimental approach that Williamson had, being willing to sacrifice animals for...

Elaine: No he didn't believe in sacrificing them until he had to. That was always a disagreement between them.

Cameron: Williamson's approach was strictly an experiment, let's conduct the experiment. Karl said, let's conduct the experiment but don't harm the animals. (Interview (E-1-6,7), March 1980)

Karl and Elaine were both willing to accept confinement, altered diets, controlled breeding, X-ray examination, and blood sampling, but they drew the line at killing the deer. Dr. Williamson was very careful in his treatment of the animals but, not unreasonably, he saw little difference between killing an animal in a slaughterhouse and killing it in a laboratory. He felt that his moral obligation primarily required that he keep the animals in good condition and free from undue harassment. In
fact, they were better treated than most farm animals I have seen. It is worth noting that neither Elaine nor Karl are vegetarians, and that they both follow the typical high-consumption urban life-style responsible for the widespread degradation of the environment that poses the main threat to the survival of wildlife. This contradiction is rather typical of the confusions surrounding issues such as these. When scientists study electrons, rocks, or slime moulds, things we do not feel any sense of identity with, they routinely smash, reconstruct, manipulate, and generally interfere with them as an intrinsic part of trying to understand what they are. No-one objects to this, and as we have seen, the typical way of thinking about science does not include any effective way of considering these actions. Although these manipulations and controls are central to scientific research, it is not until scientists begin to study mammals that this aspect of the enterprise becomes apparent to most people.

Such concerns are remote from the everyday routines of the research, unlike the unpleasant and immediate task of killing an animal. All of us found it extremely disagreeable to dispatch even an incurably sick deer. Moral inconsistencies notwithstanding, Elaine's and Karl's objections to killing the deer in research were a major impediment to Williamson's doing some kinds of work typical of physiological ecology, some of which he wanted very much to do. As I pointed out in the previous chapter killing the animals is necessary in some kinds
of procedures, and it is routine in major parts of endocrinology and animal science.

Scientists have a passion for writing things down, and this became an important job of Elaine’s as well. At Dr. Williamson’s behest she developed her own system for recording the daily events of the research situation. In fact, she became the official recorder of certain specific kinds of information.

Cameron: Elaine kept notes from when she first started, she kept little notes, and then they expanded as she thought the need. She kept expanding the notes until she got the whole concept of what was going on. (Interview E-1-15, March 1980)

In fact, Elaine’s notes took up different specific interests from those of the others. Dr. Williamson and his students did such things as measure an animal’s physical dimensions, weigh it, sample blood and do radioimmunoassays of reproductive hormones, and weigh food boxes to determine daily food consumption. They also had access to the records of a small weather station about 100 meters from the deer enclosure, a Stevenson screen set up for forestry research. Elaine kept track of the births, deaths, illnesses and treatment of the deer. She also recorded extremes of weather, feed shipments, and arrivals and departures of animals. The note-taking amounted to a few lines a month for the first nine months, and then expanded to half a page per daily entry after that.

Bruno Latour and Steve Woolgar have pointed out that what they call “literary inscription”, that is, taking written records of events and then using these extensively, is a hallmark of scientific research. See Laboratory Life, pp. 45-53.
Elaine also took a great interest in her charges' eventual fates. Deer are very expensive to maintain and surplus animals were released into the Research Forest or sent to zoos, or to other universities. Releasing them into the Research Forest was part of the original rationale for putting the deer pens there. Shipping animals to a zoo gained no income but the zoo's management paid all the costs, and Karl had many contacts with zoos because of his previous work. Elaine noted the departures and destinations of all the animals and recorded the sightings of deer released in the area.

Elaine's notes were valuable to Karl and Williamson, like the rest of her undertakings. They referred to them when writing up their research, and came to rely upon her to continue to keep her notes.

Cameron: Karl borrowed Elaine's notes when he wrote his thesis.

Elaine: And he used to phone me, constantly phoned me.

Cameron: ...and he knew Elaine kept notes all the time, so he'd come back and he'd say, "Did you get this, did you get it all?" (Interview (S-1-15), March 1980)

Williamson gladly acknowledges this:

...its a hell of a good day book. It was wonderful for referring back to something... she got newborn fawn weights,... when she put fawns out in the (big) pen, she put all that information in the book. I subsequently several times recovered those original weights. Also, if an animal is sick, or showing some sort of peculiar behavior, she's very good at making little ... anecdotal notes. And sometimes she'd say, 'Well, that animal hadn't fed properly for a week', or, 'I saw it coughing four days ago'. You know what I mean, that's very useful information. (Interview (S-2-20), May 1981)
Williamson was well aware of how much he depended on both Elaine's and Karl's initiatives.

To be quite honest with you, Karl, with his knowledge of disease... and Elaine, for just her bloody love of those animals, were crucial to the whole thing, absolutely crucial. Lots of other things contribute positively, but in their absence you could have gotten around it. I would have had enormous trouble. Without Karl's knowledge of... the hand-rearing problems, and without Elaine's just absolute reliability of doing her job and looking after the animals and keeping them fed. (Interview (S-2-21), May 1981)

He did find himself in between them from time to time, however.

He was the final authority on most decisions affecting the research, and often he had to deal with the conflicting priorities of the people working for him. He shows this here, and I shall raise the issue again.

Elaine had some wonderful rows with Karl. I just used to think, 'My God!', to begin with, when they didn't really understand or know each other, or recognize each other's worth. They really did toward the end. But at first... (he laughs, gestures in amazement). But I think the point about it is that they both had, in a very different way, a very real feeling for the care of their animals... And that was the common thing they had. If they hadn't had that, I don't think they would ever have talked to each other. (Interview (S-2-21), May 1981)

The important point here is that both Elaine and Karl were trying to do what they felt the deer needed done. Their personal commitments sometimes clashed, but were essential ingredients of the research process, and so was their ability to come up with a workable resolution of their differences. Others around them helped with this. Cameron's becoming friends with Karl doubtless smoothed the passage, and Williamson's institutional authority played a central role.
I never lost my temper with Karl. I did with lots of other people, but never with Karl. I just sat him down in that bloody chair and said, 'When it comes down to the end, I can kick you out of your office tomorrow', and he would bristle, and get all... (gestures angrily), and the next day he would be as happy as can be. (Interview (S-2-21), May 1981)

The boundaries of the project

Much of what went on in Williamson's working world did not enter into the deer project and much of what went on down at the deer pens had little lasting significance. Much of it was never meant to be. For instance, a pest management student tested chemical sprays designed to keep deer away from orchards. A couple of forestry students tested the animals' preferences among different types of browse. Williamson's relationship with these people was that they were free to use his facilities so long as they did not interfere with the other work being done. Their access to the deer was a consequence of the deal that Dr. Williamson made with the management of the Research Forest, which I shall discuss at greater length in Chapter Six. Some other projects simply did not get off the ground. A proposal for a suckling study involving a psychologist, an endocrinologist, and Williamson, was not funded. One graduate student injured her back and had to move to work that did not require picking up large animals. Another was unable to work out a suitable research program with Williamson, and went elsewhere.

At no time did the deer project occupy all of Williamson's graduate students. There was always at least one student around
who had no significant connection with it. He also had graduate students in the pest management program, a professional program aimed at training managers, not researchers. The rest of us did not know most of these students well, and we had not even heard of some of them.

Shortly before Karl arrived, Lucille, an MSc. candidate, came from Quebec. She was interested in aggression and population control, and wanted to work with Williamson because of his post-doctoral research. He wanted her to do field work on mice, following up on his previous lab work, but this proved to be impractical. Instead, Williamson and a representative of the Alberta Department of Agriculture developed a project to study the effects of a chemosterilant on Richardson's ground squirrels (Spermophilus richardsoni), also known as prairie dogs. The Department of Agriculture was providing money to test the chemical, which was to be used to sterilize ground squirrels and control their numbers in agricultural areas.

The project did not proceed as planned because of bureaucratic inertia in the Department of Agriculture, although this did not become known to Lucille until she arrived at her field site ready for work. This presented her with the opportunity to use the chemosterilant in a field study of social structure and breeding, which she much preferred doing. She used it to sterilize selected female ground squirrels, to determine the effect of their sterility on the social structure of colonies in which they live. This was possible because the
Department of Agriculture did not actually specify very closely the sort of work she was to do, apparently more through bureaucratic oversight than out of a desire to support pure research. Lucille had an NRCC graduate scholarship, and so was largely financially independent of Williamson. Her project was interesting to him, but completely set apart from his current research. She transferred to the PhD program, spent three summers doing field work in another province, and had very little contact with the deer project.

Another student did an MSc on habitat selection in beavers (*Castor canadensis*), a project very oriented to his developing career in environmental consulting. He was quite unusual, coming to Williamson with his own research funding and a very specific research proposal. His work was entirely separate from the deer project. Later, when I was present, there was a doctoral candidate working on habitat selection in moles (*Scapanus orarius*), and a masters candidate doing a field study of the same bighorn sheep herd Karl had started on. Although Dr. Williamson supported all three of these projects they were well removed from his own research at the time.

It appears that Karl's early work on bighorns would fit into this pattern! When he came Williamson was finishing off the mouse work, and had not begun a new project of his own. When the sheep project fell through Williamson helped Karl get started on the deer work and likely played a role in setting up the first experiment. However, after the first experiment, he went on a
year's sabbatical leave in Australia and worked on marsupials. Furthermore, it was not entirely clear in the early stages how long the project would last, something that Dr. Williamson had cautioned Elaine about when he hired her to take care of the deer. With time, however, Williamson became progressively more involved with the deer, and by the time I arrived in 1977, his research and the deer project had completely coalesced.
VI. Taking apart the scientists' story

The previous two chapters have addressed the situation of Williamson, Karl, Emily and the deer in the terms that the people themselves used. It is now time to look at one of their key stories very closely, and place it in critical perspective by filling in what it leaves out. I shall begin this chapter by describing the first experiment that Williamson and Karl did before my arrival by giving a precis of the paper they published on it in 1975. Then I shall provide an alternate reconstruction which stresses how the analysis of the experimental results was done, and how the public image of the work was created. This reconstruction is based on information I got from Williamson and other participants, but which did not fit into their dominant public account. This appears to be information they had not reorganized systematically. They had not reworked it into a public account, or made into a group consensus. At the same time, no-one seriously questioned this material. Thus it is not a serious alternative to the public account, so far as the normal business of the lab was concerned, even though I got the information from group members.

My version of the early work contradicts Williamson's and Karl's in a number of ways, but it does not dispute the assertion about deer which they made in their paper. I am content to believe that assertion insofar as it is a statement
about deer living under the kinds of conditions these people imposed. However, in addition to being a statement about deer, it was also used by group members as a history of the work that was done, as a kind of origins myth. And it is one that proposes that an ahistorical account which is an idealized image of an immutable reality also be treated as a true history of the work these people did.

You will remember that I argued in the second chapter that formal research reports fill a double role, they are descriptions of external reality and also mentalistic "histories" of inquiry. The two fit together, form a complementary pair. To accept such a history uncritically, when my aim is to provide a coherent account of what its tellers were doing while making up and using ones like it, would be an odd undertaking. But I do not think this means I must consider Williamson's and Karl's claims about what deer are like to be false. I do reserve to myself the right to judge their relevance and wisdom, however. My intention is to establish a critical distance from the way this formal rationalized story was recounted by the group members in the course of their work. I wish to deprive it of its sacred character and in the course of doing this I must necessarily step outside of the scientists' world. Once profaned, the story can be portrayed as a part of what these people were doing, as something other than a frozen insight into the true workings of the world.
Before giving an account of these experiments, I shall discuss puberty briefly, for it is a central technical issue in this work. The most important criterion of puberty was birth of viable young. Other criteria are, first estrus (i.e., first ovulation), first mating, and first successful fertilization. None of these others need culminate in the birth of viable young, however. Deer are known to have what are called silent heats, ovulation not accompanied by mating behavior or receptiveness to bucks. Eggs can be fertilized without implanting and developing into a fetus. Fetuses can be aborted or resorbed. Resorption occurs when a fetus dies and rather than being expelled is assimilated into the mother.

The potential importance of precocious puberty lies in its possible effect on the intrinsic growth rate of the species, the rate at which, barring predation, unfavourable weather, starvation, and disease, a population of animals can produce new members. These new members are, of course, subject to selective mortality because the environment is far from ideal in its treatment of any one species, but a higher intrinsic birth rate gives the species more possibilities. If precocious puberty did not eventuate in viable young, then it might be of interest to endocrinologists but it would have little or no ecological significance. From the standpoint of a game manager a higher birth rate means more animals are available for hunting, and
this work was, at least in passing, justified in these terms as well as in terms of its scientific interest. If precocious females produce viable young, ones which can mature and breed, then precocious mothers who succumb to predators, winter starvation, or disease in the second year of life, will have had a chance to reproduce, and the ones who do not succumb then will produce more than they would otherwise be able to. This will presumably have some ecological consequences and constitute a different reproductive strategy from the one that actually is carried out by this species. An issue worth pointing out here is that only the puberty attainment of females is thought to be worthy of attention in this study. It is commonly believed, on the basis of observation of the rut, that only a few male deer breed in a given year. These are the large, dominant ones who win the spectacular battles of the fall. The bucks reach puberty long before they are large and experienced enough to win these fights. Therefore, variation in the age of attainment of puberty by males probably is not highly important to the reproductive potential of the species.

The first experiment: establishing _precocicus puberty_

The first experiment was an attempt to establish the relationship between the weights black-tailed fawns achieve by the late fall and the likelihood of their getting pregnant at that time. This experiment lasted from the fall of 1973 to the
spring of 1974. Early in 1975 a paper was written in which the experimental data were combined with crucial information acquired from two other sources after the experiment was completed. It was published later that year and also appeared as an appendix to Karl's dissertation in 1977. In this paper information about black-tailed deer that came from three widely separated sources and was gathered at very different times, was combined to create an account of a unified series of events that was implicitly held to have occurred in the fall of 1973, at the pens at the Research Forest. I shall examine this paper here, for the event it recounts was crucial to subsequent research. The paper is also an example of the way that the group's involvement with the world beyond the university was incorporated into its technical practices, but was also largely hidden. First I shall relate the events recounted in the paper and then I shall explore its sources.

In the paper the problem of precocious puberty is defined as follows. For all species of deer, the age at which free-living individuals reach puberty depends on range conditions. That is, it depends on food supply. Good range conditions are accompanied by earlier achievement of puberty, and poor conditions by later achievement. It has been observed in a few cases that female black-tailed deer fawns have given

1Williamson, Roger and Karl Voelker (1975) "Achievement of precocious puberty in female black-tailed deer (Odocoileus hemionus columbianus)". Theriogenology. (Authors' names and title of paper altered, and details of publication deleted for reasons of confidentiality.)
birth in captivity, while the evidence shows that they virtually never do so in the wild. Accordingly, Williamson and Karl propose a critical weight hypothesis, that is, that a deer must reach a certain weight before it can enter puberty. They propose that free-living female black-tails fawns do not achieve this in their first fall and usually range conditions are such that yearlings do so by the second fall. Under captive conditions with unlimited food and maximum growth rates, fawns could achieve this critical weight, and with it, puberty. This experiment, as it was written up, tested the critical weight hypothesis by comparing the weights, in November, of free-living doe fawns, with those of captive doe fawns at the same time of year.

In the fall of 1973 seven light female fawns and seven heavy female fawns were chosen from the animals captured that spring. They were placed in individual pens on October 1, bred on October 28, and on November 1, the two groups were placed on different diets. The heavy animals were allowed unrestricted access to food, the light animals were kept to 70% of what they had eaten in October. According to an abundant technical literature this is a time when the quality and quantity of

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3 For a report on similar work with kine, see The Book of Genesis, 41.
FIGURE 11: Weight in November of captive and free-living fawns.

EXPERIMENTAL FAWNS (N = 13)

N.W. BAY FAWNS (N = 314)

TOTAL BODY WEIGHT (kg)

2 S.D.'s
forage available in the wild are declining, and because of this the animals are, according to the terms of this experiment, prevented from achieving the weights that allow successful reproduction. As it turned out, the dietary constraints did not widen the gap between the weights of the two groups. After six weeks of the different diets, they were put into the large enclosure with the other deer, and had unrestricted access to food and to the bucks. The following spring 5 of the 7 light fawns, and 4 of the 7 heavy ones were found to have conceived. One non-pregnant doe died of misadventure and the pregnant animals gave birth to single fawns early in the summer. 3 other fawns, not used in the experiment, gave birth as well, yielding a total of 12 for the herd. This gave an overall rate of precocious puberty of about 50%. Seven of the fawns born to precocious mothers died while still suckling, which was thought to be due to the mothers' not producing enough milk. 4

Following this description of the experiments the weights achieved by the captive fawns by November are compared with those of free-living animals in November. The graph of these results is reproduced as Figure II on the facing page. 5 Note that a large proportion of the wild fawns, which were known

Why this should be is not clear, because in 1977, the fawns of precocious mothers had no such problems. It is possible that the group's standards of husbandry had improved in the meantime.

5 Voelker, Karl. (1977) "Precocious Puberty in Captive Female Black-tailed Deer (Odocoileus hemionus columbianus)". PhD dissertation, Department of Biology, Western University. (Author's name, title of dissertation and location altered for reasons of confidentiality.) p. 132. Used with permission.
never to get pregnant, were heavier than a significant number of
the pregnant captive fawns. Thus the critical weight hypothesis
was refuted. The captive fawns show a pregnancy rate of 50%, but
this result does not seem to be a consequence of their having
had a higher growth rate than free living animals. When I worked
with the group these results constituted a fact, an unquestioned
premise of the work we were doing. It was their story of the
first technical achievement in this project. As Dr. Williamson
said in an interview in 1978:

...we did one or two years work and showed quite
conclusively that simply by bringing them into captivity
and feeding them like hell, we can get them to breed.
But it isn't as simple as that because when we looked at
the body weights of our animals, compared to wild fawns,
our animals were not persistently heavier than the wild
fawns were. (Interview (S-1-3), 1978)

Now let us consider the other sources of information
besides the experimental results that went into the paper, to
show how connections to the world outside the group were built
into its local discourse through their role in the creation of
this story. These outside connections were not simply sources of
information, but affected the course of the research from the
beginning. To do this research Dr. Williamson and Karl needed
deer, funding, and background information on black-tailed deer
and closely related species. Without all of these the project
would not have been viable. To get deer they needed the support
of the Fish and Wildlife Branch of the Ministry of the
Environment of British Columbia. This is because, in accordance
with the British North America Act, wildlife are the property of
the provincial Crown. Individuals must get permission to capture and hold wild animals. This was easy for Dr. Williamson because he was an established animal ecologist with a university post, and furthermore, black-tails are a plentiful game animal, which meant that he would not deplete the population by setting up his own herd. Furthermore, his work had potential game management applications. The Branch regulates hunting in the province and is in charge of game management. They gave him a collecting permit and provided $18,000 of the roughly $95,000 that was spent on this research between 1973 and 1979. Background information was available because some research had been done on black-tails in recent years, of which more will be said later. There is as well an enormous literature on white-tail deer, as we have noted before.

The Branch had a substantive impact on the research from the beginning. I learned this when I asked Dr. Williamson why they had collected their experimental herd where they did:

...the Fish and Wildlife Branch...made several inquiries through various regional offices. They asked Conservation Officers about good places, and that was an area on which they had good information, and in which the Conservation Officer was very cooperative. And, basically, they gave me a permit to collect there. They really made that decision. (Interview (S-2-6), April 1981)

This decision did a great deal to determine the course of the first experiment, and the way it was represented in a professional journal, as we shall see. The permit allowed them to collect fawns at Kelsey Bay, on Vancouver Island. Kelsey Bay is about 20 miles from Northwest Bay, where a considerable
amount of research had been done on black-tails during the 1960s. Kelsey Bay was in roughly the same stage of logging in the early 1970's that Northwest Bay had been at that time, and so was a roughly comparable habitat.

The situation is as follows. "Northwest Bay" refers to an uninhabited mountainous watershed where MacMillan-Bloedel, a massive forest products corporation, had been logging since 1939. From 1954 to 1966 MacMillan-Bloedel cooperated closely with the Fish and Wildlife Branch in studies of the deer population in this area. There is only one road into Northwest Bay, built and maintained by Mac-Blo, as it is known locally. The Fish and Wildlife Branch put a hunting check station on that road whenever the area was open for hunting. They did this in the fall, it being government policy all over the continent to allow deer hunting at that time of year. The Branch is legally empowered to do this anywhere in the province. At the check station they weighed the carcasses the hunters brought back, aged them by tooth eruption and wear, and recorded their sex. In effect, they were using the hunters to sample the deer population. They also conducted spring and fall fawn counts and a fawn tagging program. Ear tags were put on fawns so that their survivalship and dispersal could be determined. In addition, a


Thomas.
number of graduate students and professors from the University of British Columbia did research in the area in the 1960s and 1970s.

This sort of cooperation is quite common and hardly surprising. Deer are of interest to logging companies because they browse on young trees, sometimes hampering reforestation. Logging influences deer populations because the vegetation that springs up after the forest is removed provides very good habitat for deer for from 5 to 20 years after logging. This causes a large increase in the deer population during that 15 year period. This, of course, is very interesting to the Fish and Wildlife Branch. There are numerous personal contacts between the universities, the Branch, and the logging companies because some of the people working for the latter two were trained at U.B.C. Specifically, some of the Conservation Officers and other senior people at the Branch were trained by professors who supervised the research on black-tails that was done at Northwest Bay, or had done some of that research themselves. Dr. Williamson had access to this network because he was an ecologist working in this area, and also because he had done his post-doctoral work in the Zoology Department at U.B.C.

The place where they collected the breeding stock for the experimental herd later became very important, because the wild fawns whose weights were compared with weights of captive fawns

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Gates, B.R. (1968) "Deer Food Production in Certain Seral Stages of the Coast Forest". MSc thesis, Department of Zoology, University of British Columbia.
had been shot by hunters at Northwest Bay and were recorded by the Fish and Wildlife people at their roadside check station. Two points are of crucial importance in regard to this information. The first is that to do this kind of absolute comparison of weights Dr. Williamson and Karl had to have animals from roughly the same geographical area living under roughly the same conditions. This is because animals of a species vary in size, weight, rate of maturation and numerous other attributes across their species range. Blacktailed deer are found from Alaska to the chaparral of California. The deer used in this work were collected on an island, which is significant because members of island races are almost invariably smaller than their mainland conspecifics, and the smaller the island, the greater the reduction in the size of the animals. The largest black-tails on Vancouver Island are found in the logged areas of the southern quarter of the island. The deer further north, at Kelsey Bay and Northwest Bay, for instance, are smaller. The deer on the mainland coast are larger than the Vancouver Island deer, and inland from the coast we have mule deer, which are almost twice as heavy as coast deer.9

Therefore it was necessary to have the data from the Fish and Wildlife study at Northwest Bay in order to perform the comparison. This was not foreseen when the research was undertaken. An alternative would have been to go back to Kelsey

Bay and shoot some fawns in November, but doing this would have been awkward:

...you see, the ideal condition would have been to go back to the field and shoot a lot of fawns and weigh them...But that would have been a very difficult thing to do...politically, the problem of shooting twenty fawns is not easy, you know, in a management sense. (Interview (S-2-9), May 1981)

In other words, hunting fawns is one thing, but shooting them for a study would probably have lead to objections within the Branch, as well as from some members of the public, and produced political difficulties for the Branch.

The second point is that the Northwest Bay fawn weights were completely useless at the time the first experiment was done, for these were weights of eviscerated carcasses. It was not until after October 1974 that a way of converting eviscerated weights to live weights became available with the publication of an unanticipated morphometric study of mule deer. This was called *Growth and Morphometry of the Carcass, Selected Bones, Organs and Glands of Mule Deer*.10 This came out in the monograph series of the *Journal of Wildlife Management*, a publication that Dr. Williamson and his students read regularly and published in. This study was useful to Karl and Dr. Williamson because black-tails are a subspecies of mule deer. Dr. Williamson described the study to me in an interview:

This was part of a study involving several hundreds of mule deer, in which he was looking at age dependant and

season dependent variation of relative organ weights. He shot a lot of animals at different times of year, and did nothing but weigh and measure livers, hearts, lungs, froze the carcass and cut off the fat, then cut off the meat... to get regression lines against things like body weight and size for different ages and seasons. These are very useful tools because it means that somebody can subsequently come along, who may be interested in protein metabolism and looking at livers. He has a sample of livers and wants to know, are these livers' sizes and weights truly representative of healthy, or unhealthy, poorly or well fed mule deer at such and such a time of year?... Basic morphometric studies are the most incredibly boring thing to do. But very, very useful, because, unless somebody does that sort of measurement, if you have an abnormal animal, or you want to follow a seasonal change, where's your base?

McKegney: Yes, that paper on puberty attainment is absolutely dependent on the information from around Kelsey Bay, and on that formula.

Williamson: I couldn't have done anything like the work without it. (Interview (5-2-8,9), May 1981)

Underlying studies like these is the assumption that although the members of a species will vary across their range, in size for instance, the overall body pattern of the species will not change. The relationships between the parts of the body will be more or less constant. This monograph contains graphs and regression equations relating eviscerated weight to age, and bled weight to age. Bled weight is the weight of the animal, minus the blood that escaped from the gunshot wound. It provides a low approximation to live weight. There was no formula relating bled weight to eviscerated weight so Dr. Williamson wrote to the senior author of the monograph to ask if he had computed such a formula. He replied right away saying that he did not but that his new computer assistant would work one up right away. The formula arrived three weeks later.
The data from the study at Northwest Bay became information when the formula arrived. Until then, they had been only numbers without significance. Before they were converted, it was necessary to be confident these were actually the weights of fawns and not of older animals, the accuracy of the aging had to be confirmed. Animals are aged in the field according to the eruption and condition of their teeth, just as you could tell a one-year-old human child from a two-year-old by looking at his or her teeth. If a number of yearlings had been classified as fawns, then the data would show a misleadingly large number of quite heavy fawns, and fawns, remember, were assumed not to have reached puberty. These data were sifted and examined very closely before being incorporated into the analysis.

Williamson: Shall I tell you another thing that's a problem here?

McKegney: Sure.

Williamson: ...relying upon other people’s data. ...One real problem was...whether some of those animals were fawns at all. And Karl and I had to go through those things very carefully indeed, looking at the weights, looking at how they'd been aged, whether they'd looked at teeth patterns, and this sort of thing. And to be quite honest with you, I was never 100% happy with some of their categorizations of what they call fawns.

McKegney: You think there might have been some yearlings in there?

Williamson: Yes. That's the other side of this. You don't collect the data yourself, and so you have to be very, very careful in its interpretation. (Interview (S-2-9,10), May 1981)

The eviscerated weights were converted to bled weights, giving low approximations to live weights. Comparison with the
captive animals showed that even with this underestimation, a large proportion of the free-living fawns were heavier in November than the captive fawns which had become pregnant had been. Until this comparison became possible Dr. Williamson and Karl could only compare two not very different weight classes of captive fawns which had effectively identical pregnancy rates. These results indicated only that about half of the captive fawns had become pregnant, but cast no light at all on the reason. The information from Northwest Bat, Colorado and the deer pens was collapsed into an account of one unified series of events that occurred in the deer pens. This account was written in accordance with the dictates of Popperian methodology; it was portrayed as the test of an hypothesis which Williamson and Karl were not in fact considering when the experiment was conducted, one which they probably could not have formulated until the experiment was over.

Regardless of its value as history in the strict sense, this rational reconstruction did make adequate sense of the information available, and its form met the requirements of the journal editors. Moreover, the information from Northwest Bay made for a much more interesting paper, and it had some straightforward practical consequences for its authors. The results of the experiment showed that precocious puberty really does occur in captive female black-tail fawns, and the analysis showed decisively that a critical weight hypothesis could not account for this anomaly. The specific form of the object to
which they later referred was determined by three things: what
deer are like, institutional policies, and the organizing
efforts of this specific group of scientists. Once produced,
this object was ploughed back into the practices of the
subsequent research, for Dr. Williamson and Karl had a genuine
scientific problem on their hands and a very decisive—the word
"decisive" is important—indication that they would have to
develop new hypotheses and experiments to explain it. They had
to develop new special conditions to place the deer under, which
meant new routines would enter their own lives as they conducted
these experiments. There would be concomitant changes in their
thinking and the topics of their conversation, and more efforts
to find funding for their work. In sum, the lives of the deer,
and the activities and thoughts of the researchers were altered
materially because of the results of this analysis. I shall now
discuss the work that was done in the wake of this analysis.
Then I shall consider the place of the formal written accounts
of the early research in the affairs of the group while I was
with them.

Milk analysis: a filler

The first experiment was completed in the spring of 1974
and reported in Theriogenology in 1975. The next experiment was
not begun until 1975, for this kind of work must follow the
annual reproductive cycles of the animals. Dr. Williamson
himself found this irritating, but not everyone felt this way. Michael, one of his graduate students, liked working in this field for precisely this reason. He liked being out in the bush to do his research, and he liked following the animal's schedules.

Karl kept himself busy during the summer of 1975 by analysing the milk produced by the deer. Before describing this work I shall digress briefly to provide some more biological background. As I have mentioned before, one of Dr. Williamson's long-standing interests, evident in his input-output analysis of the energy requirements of mice, is the energetic economics of reproduction. This is a reductionistic approach in which the animal's energy expenditures are partitioned and determined, and then added up. On the one hand, there is the food the animal eats, and on the other hand the waste products like feces and urine, and the secretions, such as milk. In between, so to speak, we have the animal and the nutrient it absorbs and uses. Here we have the carbohydrates, fats and proteins that are absorbed into the bloodstream and used by the cells. In the cells much of this food is broken down and used to maintain life, the waste products from this being eliminated through the lungs as carbon dioxide, through the kidneys as urine, and also through the walls of the large intestine. Some of the food is retained in the body, is broken down into constituent molecules and then used to build up tissues.
All the while, the animal does many different things like running away from predators, feeding, taking care of the young. Underlying these very obvious activities is what you might call metabolic work. "Metabolic work" is a very general term that I am using here to describe biochemical and physiological processes such as digestion, secretion, and muscular action. In thermodynamics, these processes can be analysed in terms of chemical work, the amount of energy they consume, just as a mechanical process, like moving an automobile, is analysed in terms of the mechanical work done against gravity, air friction, and the like, which consume energy. If you add up all these different kinds of metabolic work, they should equal the difference between the available energy in the food eaten, and the available energy in the urine, feces, milk, and whatever else is eliminated from the body. By "available energy", I mean the amount of energy that can be obtained from the food, the feces, or whatever, by combining the complex chemicals in it with oxygen to break them down into simple chemicals like carbon dioxide and water. This is what happens when any organic compound is burned in air, and there is a sense in which the cells of the body "burn" the food they absorb from the bloodstream, breaking it down into such simple end products. The energy obtained by this oxidation of food molecules is used to power the muscles, digest food, and synthesize the special molecules that build the body and constitute the milk and other secretions.
It would be possible to go into a great deal of detail in tracking these things down—trying to determine, for instance, how much work a black-tail doe does to synthesize milk proteins and secrete them. Such a project would be extremely expensive and require different kinds of skills from the ones Dr. Williamson had developed. Instead, he and his students asked questions like—how much food must an animal eat in order to produce viable young, ones capable of growing to maturity? Or, more narrowly, how much food must a doe eat in order to produce enough milk to suckle her young to independence? They would consider the animal's total energy intake, minus the waste expelled, the energy that goes into basal metabolism, the energy in secreted milk, and other broad categories like these. In the case of deer, so little work has been done that a reasonable, if not terribly exciting, starting point was simply to find out what a doe's milk consists of, how much butterfat, lactose and protein there is in it, and how the composition and quantity of the milk vary during the course of lactation. Karl and Dr. Williamson asked, in effect, just what is the fawn getting from the doe's milk? I suspect, in addition, that Dr. Williamson suggested this work to Karl as a safeguard for his degree:

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11Information about this is available because scientists have put deer, and other large animals like cows, sheep and human beings into sealed insulated chambers, with controlled temperatures, oxygen flow, and the like, to find out how much energy an animal consumes at rest, simply to breath, pump blood and maintain metabolism. This is called basal metabolism.
A graduate student's research should always consist of two parts. It should consist of what I call base, the collection of hard baseline data which, no matter what, he can write up into an MSc thesis or a PhD thesis. Do you know what I mean- its there, its core data. Its often the tedious stuff, boring. In addition, it should consist of something which is sufficiently speculative that it may not come off at all. It might be a complete bloody risk, but, if it does come off, if he does get results that are innovative and exciting in terms of the previous information, he can make himself a reputation. And of course, being totally non-altruistic, the reputation of his supervisor. (Interview S-1-7, fall 1978)

In the summer of 1975, Williamson and Karl kept three does with young in individual pens, instead of releasing them into the large enclosure after parturition. The does were milked 3 times a week for the first month, and twice a week thereafter, for a total of 34 weeks. The does were immobilized for milking with a drug called succinyl choline which inactivates the nerves governing the skeletal muscles, relaxing the animal completely. The milk obtained was analysed into fat, protein, and carbohydrate, at outside laboratories specializing in dairy and food science. One doe was chosen from each age class of interest. There was a precocious mother (one year old), a two year old who had been bred at the usual age of puberty (18 months), and an older animal who had been a mother before. All three produced very similar milks. Through the lactation period, all three increased in protein and fat, and decreased in carbohydrate. The results were published in The Journal of Mammalogy, in 1977.12

12Williamson, Roger and Karl Voelker (1977) "Changes during lactation of the fat, protein and sugar content of the milk of black-tailed deer". Journal of Mammalogy. (Author's names and title of paper altered, and details of publication deleted, for
and the graph summarizing them was used that year in planning that summer’s research, as we shall see in Chapter Eight.

The second experiment: a new hypothesis

That fall the second feeding and breeding experiment was done. Where the first had dwelt on the weights of fawns and overall diet in relation to precocious pregnancy, this one focussed more narrowly on two possible causes of precocity: the amount of protein in the diet, and a hypothetical phenomenon called maternal inhibition, which I shall explain below. Two groups of six fawns each were placed on different diets in the fall and winter. One group was on a high protein diet, the other on a lower protein diet with the same overall caloric content. The low-protein diet was meant to approximate the decline in the quality of food available to free-living animals, which occurs in the fall. Each group was composed of 3 hand-raised fawns and 3 raised by their mothers. At the time the experiment was undertaken, it appeared that all the precocious pregnancies were among hand-raised animals. This allowed the possibility that precocious pregnancy followed from a disruption of the fawn-doe

(cont’d) reasons of confidentiality.) It is worth noting here that Dr. Williamson had the following arrangement with his graduate students, regarding the order of authors’ names at the head of a paper. First author was the one whose name came first in the alphabet, second author was second in alphabetical order, and so on. In this case, “Williamson” comes after “Voelker”, but the men’s real names are actually in the reverse order, which I have preserved.
relationship. If this were so, then perhaps some hormone in the doe's milk, or an endocrine response of the fawn to the physical presence of the mother, would be delaying the arrival of puberty for another year. Such a relationship between the mother and her young is not difficult to imagine. We have seen the case of induced ovulation in which animals show a marked endocrine response to the presence of other animals, one which may be mediated by pheromones, but may not. How such a relationship would be investigated might be a thorny experimental problem, but if only hand-raised fawns showed precocious pregnancy, then such experimental work would be justified.

The animals were kept in individual pens, and fed separately from early September 1975 to mid March 1976. They were bred in their individual pens. None of the low-protein group gave birth. Two of the does on the high-protein diet were killed by domestic dogs, and of the survivors of this group one produced a fawn. She was a hand-raised animal, which sustained speculation on the maternal inhibition hypothesis. As a consequence, when a new graduate student, David, arrived in the fall of 1977, he was encouraged to design an experiment to test this hypothesis. It was resoundingly contradicted in the spring of 1977, shortly before David's research was about to begin, when some mother-raised fawns were found to be pregnant. As a consequence, Karl's dissertation did not discuss maternal inhibition and David had to begin a new research project quickly, in order to be ready for the 1977 research season.
The official history of the project's origins

From the fall of 1976 into the early summer of 1977, Karl was writing his dissertation under Dr. Williamson's supervision. When completed, it was a formal summing up of the work that had been done on the deer from the inception of the project in 1973 to the refutation of the maternal inhibition hypothesis in the spring of 1977. As Karl finished his dissertation he progressively withdrew from the deer work, and following its successful defense he left for Iran, to work on the Shah's massive game farm. Much of the content of the dissertation came from the previously published work, which is included as appendices to the text. Apart from the account of the second experiment this story about the research and especially the formulation of the research problem and its conclusions, formed the background to the work on suckling and fawn maturation that Dr. Williamson pursued and supervised from 1977 to 1980. This account was being written up at the time the summer's work for 1977 was being planned. It was the way Karl, under Dr. Williamson's supervision, decided to make sense of their work at that time. It was also the background for David's further work on the causes of precocious puberty in 1977 and 1978.

Probably because of its foundational role the story of the origins of the project that is contained in the dissertation and the paper it overlaps with became the dominant account of the
project within the group. In my experience Dr. Williamson's new
generation of graduate students never seriously questioned this
account of precocious puberty, although they rejected Karl's
conclusions from the second experiment.

The introduction of the dissertation begins with a
definition of puberty—the age at which reproduction first
become possible for young mammals, and a list of factors known
to affect it: nutrition, season of birth, social interaction
with animals of the same species. Deer of the genus Odocoileus
(American deer), which are the white-tailed deer (O.
virginianus), and the black-tailed deer (O. hemionus), breed in
the fall, usually from November to December. In seasonally
breeding species such as these puberty must occur during the
breeding season, which means that an animal that does not reach
puberty one year must wait a full year before doing so. Usually
deer reach puberty in the second breeding season of their lives,
the only consistent exception to this being the closest relative
of the black-tails, namely the white-tails. Karl then examines
the literature on black-tails to show that there is good
evidence to indicate that female black-tails almost never breed
in the first fall, and will even delay puberty to the third fall
if living on poor range, that is, if they have a poor diet. By
contrast, a few anecdotal reports suggest that a significant
number of female fawns may reach puberty and breed successfully
in captivity.
At the end of the introduction, Karl brings the problem to a focus:

It seems strange that a species would have, and maintain from generation to generation, the capacity to breed at an early age, but that this capacity would be realized so rarely within the extensive natural range of the species.\textsuperscript{13}

Then he states the three questions around which the approach to the problem is organized in the thesis:

1. Does puberty at under one year of age in captive female black-tailed deer only in exceptional cases of reproductively anomalous animals, or does it occur regularly under certain conditions of captivity?

2. Is a nutritional factor responsible for precocious puberty?

3. If a nutritional factor is indeed responsible, would this be manifested in differential weights of breeding and non-breeding fawns?\textsuperscript{14}

The first question was answered very clearly, as we have seen. Precocious puberty occurs quite regularly in captivity. It was not at all definitively demonstrated that increased protein consumption was responsible for this. Karl tried to argue that it was, but there were too few animals in his study to support the very marginal results of the second experiment, and his argument is not convincing. The bulk of the discussion was concerned instead with an analysis of the growth curves of the precocious and non-precocious fawns, for which he drew upon all

\textsuperscript{13}Voelker, 1977, pp. 8-9.

\textsuperscript{14}Voelker, 1977, p. 9.
the fawns they had worked with throughout the four years. I shall not analyze this argument because it bears very lightly upon what is to follow.

In this account, the research was done because of the contrast between the ages of puberty attainment in captive and free-living black-tails, especially in the light of what happens to white-tails. This puzzle is the starting point of the Science reported, and the specific measures taken follow from it. This story became Dr. Williamson's way of accounting for the origins of his work on deer. In September 1978, after working with Dr. Williamson for almost two years, I asked him during a taped interview how he came to study deer. In his answer, he traced a continuous development in his interest in puberty and lactation back to his work on mice at the Research Forest:

In regard to the mouse project, I'd not say I'd solved the problems but I'd come up with a reasonable hypothesis as to what regulated the breeding season. So I wanted to move on to something bigger: I also got very interested just how much a drain lactation was on these mice. This strain was quite obvious from our calculations of their metabolic rates, and the amount of feed they were getting, and some observations we made on the mice, which are normally nocturnal creatures feeding in the middle of the day, when they were lactating, so obviously showing that there was a real metabolic strain.

Then I started to look at... puberty attainment and lactation. Because, in the mice too, there were very considerable variations in the time of year, and the age, at which they would reach puberty, and nobody had much explanation of this. And, they weren't good animals to do this on, both for size reasons and other reasons. So, I decided I wanted to move on to something bigger, to do more work on, and for straight mechanical reasons, when you're thinking of choosing any animal, any project, you have to think if someone's going to finance it. I then thought of deer, because it's an animal which is of considerable importance to the wildlife people.
And the other thing that fascinated me always, with the local deer, the mule deer, was this very peculiar phenomenon, whereby other people had reported that in captivity, sometimes, the fawns bred, whereas in the wild they never do so. Whereas, compared with white-tailed deer, their closest associate, similar animal, not unsimilar environment, the animals frequently do breed as fawns in the wild. This is nutritionally dependent, on good range they'll breed as fawns, if they are not on good range, they won't breed as fawns. But mule deer also live in the wild in a variety of different habitats, sometimes on good feed, sometimes on bad feed, yet they never breed in the wild (as fawns). Yet they retain the physiological ability to do so. Why? (Interview (S-1-2,3), September 1978)

I have divided Dr. Williamson's remarks into three paragraphs, to make my point clearer, although he made them in one continuous narrative. The first paragraph follows selectively from the publications on mice that were discussed in Chapter Four. The second paragraph is about some practical matters in the transition from the mouse work to the deer work. He says he wanted to look at puberty attainment and lactation, the two concerns that were uppermost in his mind at the time he made the statement. The resemblance of the third paragraph to the argument of the introduction to Karl's thesis is quite striking. Here, Dr. Williamson states that he had always been fascinated by this contrast between white-tails and black-tails. The impression he gives is that this and the precocious breeding by captive black-tails sparked the question in his mind that lead to his developing a research project that was specifically aimed at finding out about this latent breeding capacity. The phenomenon they eventually wrote about, first in the paper we discussed earlier, and later in the dissertation, is said to be the thing they were specifically looking for at the time they
began the research project. Black-tail deer possess this capacity, and Karl and Dr. Williamson knew from the start that they were investigating.

This story is a more or less continuous historical narrative. It echoes the linear cumulative view of the development of knowledge presented in Chapter Six. In it, Dr. Williamson patches together several themes he hadarticulated in his papers along with some unpublished material, to show his latest work following logically from his earlier project. To a considerable extent he described his own scene to me in terms he had used to describe it to an outside audience.  

This story was deeply entrenched. Two and one half years later, in 1981, I asked Dr. Williamson when he first heard about precocious puberty in black-tails, and he told me this:

...that's a difficult question. I know how I found out, I don't know exactly when. How I found out was when...I first started thinking about working on deer. I looked at the literature and there were three or four colloquial reports of very early breeding of some of these animals, but they were always animals which had been pet animals, or fed in alfalfa fields, or something slightly artificial. Yet, the thing that always fascinated me, in white-tails, a very similar species, there were very distinct and definite reports that fawns bred moderately frequently on good natural habitats, which had never been described in mule deer, and still

15 This inversion of the research process is closely related to what Latour and Woolgar have called "split and inversion". They use this term to refer to the change in scientists' attitude to the conclusions of an extended public debate, when the debate is settled. The outcome of the debate, the technical claim made by its principles, is treated as having an antecedent existence, once the debate is concluded. In fact this change of attitude effectively is the conclusion of the debate, it is the establishment of the claim as fact. See Laboratory Life, Chapter Three.
I mean, mule deer naturally do not breed as fawns. (Interview (S-2-3), April 1981)

He draws upon the same themes here as in the previous quotation, the contrast between white-tails and black-tails, and the contrast between captive and free-living animals to account for the origins of the research, establish the question that drove it.

David, a graduate student of Williamson's who started his work at roughly the same time I did, provides another example of the entrenchment of this story. It prefaces his research proposals of the fall of 1976 and the spring of 1977. He used it in the spring of 1978 when he gave a talk to the weekly seminar group organized by the graduate students, and it appeared in his progress report that year. It appears in the introduction to his thesis as well and came up in conversations between us at the Research Forest. His informal discussions and opinions were stabilized and directed by this formal account.

At about the same time as I asked the question above, I asked Karl how the work began. According to Karl:

When I began the work on black-tailed deer, it was not understood from the beginning that I was going to look at precocious puberty, not at all. Dr. Williamson was leaning towards a study of deer versus tree in forest economy. And the study of precocious sexual development in deer really developed as I was first accumulating a group of captive deer and later on began to collect data on this group. It became apparent that I had this phenomenon of precocity in this group, and I began to take a close and hard look at that. (Interview (M-1-3), August 1980)

This is rather interesting, because in the conversation in 1978 Dr. Williamson had tried to fit Karl's contribution into
the historical scheme which had the deer work beginning after a
decision had been made to study puberty:

I wanted to work on deer. And actually Karl, no he
didn't. When he first came, he wanted to work on bighorn
sheep. (Williamson then talks about Karl's adventures
with the sheep) And then he decided to switch over, and
I wanted him to work on puberty. And he had a lot of
expertise in handling and raising these animals. And
that's how the two got together. He was very expert at
husbandry, very expert indeed. And catching, he'd caught
them in the wild before. (Interview (5-1-3), Sept 1978)

This is too neat, making the research look from the beginning
like an explicitly intended attempt to work on the problem as it
eventually came to be defined. Karl's story cannot be accepted
uncritically either, because it is apparent from the precocious
puberty paper that as soon as the first hand-raised deer were
weaned, they were used in an experiment that obviously had
something to do with puberty attainment. The fawns were bred
under controlled conditions, divided into two size groups that
received different diets, and rounded up in the spring to see if
they had given birth. The hypothesis that Dr. Williamson and
Karl had in mind when they organized this work was not the one
that organized their public account of it. Karl's progress
report of 1973 states that the first experiment was done "to
follow up on the hypothesis that the correlation between body
fat and total body weight may determine the onset of puberty."16

Much material was left out of the published account, but there
is no reason at all to believe that the experimental routines
were not performed at the times indicated, or were significantly

16 This is Frisch's hypothesis. See Frisch, 1974.
different from what was reported.

What we seem to have here are two different accounts of who was in charge. Cameron, Karl's friend, took his side:

...Williamson didn't want to start the project, that's what I was told. He had nothing to do with it. Karl had to do some tall talking to him, to convince him. Williamson wanted to stick with mice, little animals that didn't cost anything. (Interview (G-1-12), March 1980)

This seems unlikely to me, given that in 1973 Williamson submitted for publication a paper in which he indicated that the sort of work that his research had led to could not be done with mice for technical reasons. Karl, however, did the majority of the work of building up the facilities and the deer herd and organizing their daily operations, although Dr. Williamson did a great deal as well. Lucille, the woman working on ground squirrels had this to say:

As far as the design of the experiments goes, I would say that probably Williamson had the large part in it. Karl likes to do things, and he is very good at it, at doing things, and personally, as far as the designing goes, I think that Williamson had a lot to do with it. (Interview (L-1-5), March 1981)

She also pointed out something that was obvious to all involved, that there was an ongoing contest of wills between the two:

Karl came and Karl was much older, so Williamson couldn't do with him what he did with (his other graduate students) that were all young guys you know. He was older than Williamson! I remember Williamson coming to me on this, asking, "How am I going to handle that?" And Karl is very (she demonstrates his manner by pounding her fist on the table and looking very determined, and saying) "That's the way it is." (Interview (L-1-4), March 1981)

/There had to be a tradeoff of power for Karl and Dr. Williamson
to be able to work together. Such things are to be expected when people have to decide what course of action to follow, and have different backgrounds and ambitions. In situations like these a central point of contention is likely to be found in the question: "What are we looking at?", "What do we actually have here?", and "What should we do about/with it?". The different accounts Dr. Williamson and Karl gave of the origins of the project are extensions of the negotiations they were repeatedly involved in while they worked together.

In this case, you might say that Dr. Williamson "had the facts on his side". That is to say, his version of the origins of the project is literally a gloss on the introduction to Karl's dissertation and the paper they wrote before that. Dr. Williamson used the fact of precocious puberty, namely the history provided by the paper and the dissertation, which was the history he had to stand behind if anyone questioned his present work. Moreover, at the time I spoke with him, he was also doing and supervising research which takes this fact directly as its foundation. Karl's remarks were made (via tape recording) while he was working in a zoo in Australia. He had narrowly escaped the minions of the Ayatollah Khomeini when the shah was overthrown (Karl is susceptible to adventures), and had no commitment to the official line, as you might call it. David and the others accepted the basic story, that there was a roughly 50% rate of precocious puberty, which is not explained by a critical weight hypothesis. The specifics of the origins of
the work were not crucial to us, but we knew of them.

In my opinion, this highly rationalized story provided stability, an unquestioned premise upon which other work could be based and which cut off questions that might undermine the present work. Dr. Williamson's use of the fact of precocious puberty, which I have no reason to believe was atypical, was an integral part of his political strategy. With time, mind you, he probably became so accustomed to that story that he likely would use it now if asked about the origins of the project, as indeed he did in 1981 on the eve of his departure for a new job as a research director in a different country.

Karl and Dr. Williamson agreed on some practical matters and I suspect that the origins of the project can be clarified by examining these. In an interview Karl explained how he had come to study deer. After the failure of the sheep work he first considered working on sea lions or seals, and spoke with a professor at U.B.C. who was studying these animals at the time. This man strongly advised him not to move into the area because of the great expense of maintaining the animals in captivity. Dr. Williamson had run into this problem himself several years earlier, when he had wanted to work on killer whales, and discovered that there was no money available to him, all of it being taken up by a few established researchers. Karl then approached another professor and an official from the Fish and Wildlife Branch about working on deer. They had worked on black-tailed deer and their doctoral supervisor, Dr. Ian
McTaggart-Cowan, had done the pioneering work on the species. All three of them had been connected with the work done on black-tails at Northwest Bay, typically as members of the supervisory committees of graduate students who were doing the research. They encouraged Karl to work on this species because, he says, background work had already been done:

And then the decision was made between myself and Dr. Williamson, who was of course supporting this research, to start studying black-tailed deer, because they were an ungulate, a large mammal that was abundant. It was an important animal for hunting and an important animal in general in the forest of B.C. It was also an animal that we could obtain ourselves without too much problem, and an animal we could maintain without too many problems, and at a reasonable cost. These were all factors that were considered when this decision was made to start studying deer. (Interview (M-1-3), August 1980)

Williamson's account, given earlier, includes similar considerations:

...for straight mechanical reasons, when you're thinking of choosing any animal, any project, you have to think if someone's going to finance it. And I then thought of deer, because it's an animal which is of considerable importance, to the wildlife people. (Interview (S-1-2), September 1978)

Lucille put it this way:

...they decided on deer because it is an animal that is easy to capture, and Fish and Wildlife is not sitting on your back you know. (Interview (L-1-5), March 1981)

The Fish and Wildlife Branch personnel provided deer collection permits, some funding, and unpublished information. The opportunity to study deer existed within the institutional setting. This was reinforced by the situation at the Research Forest, where Dr. Williamson had done his work on mice. There were already a couple of deer in captivity at the Research
company for shooting wildlife scenes. Dr. Williamson made a deal with the administration of the Research Forest:

They basically wanted, they've really profitted enormously, from having penned deer immediately around the forest gate. They want to be seen to be involved in multiple use forestry. And the animals that people see in the forest, they don't even know that the mice and the moles and the other animals are even there, but they do occasionally see the deer—the visible animal in the forest. And people like seeing animals like that. So they were very interested in having...a little zoo, a little display of animals. And they also had plans, which never actually came to anything, to do feeding studies in regard to forestry repellants, the effect of browsing by deer on trees, this sort of thing. Now, they never had anyone, no student in forestry ever came along to look at that, which is a great pity. But the deal I made with John was that if any student came along who wished to do that, we would provide the deer, he could work in the pen. (Interview (S-2-12,13), May 1981)

The bulk of the funding came from the National Research Council of Canada (now NSERC), which has traditionally backed academic researchers to do whatever sort of research they have established their competence to do—subject to peer review. The Canadian Wildlife Service provided some funding for a study of the public's response to the captive deer. The biology department provided occasional funding.

It seems most likely that they elected to study deer because Dr. Williamson was looking for an alternative to mice, Karl needed a large mammal project, and deer were institutionally and practically feasible. There was probably no dearth of research questions pertaining to ecology and reproduction that they could have undertaken on any animal they chose to study. Once they selected deer, they undoubtedly set
about reading the literature, and began formulating specific, practically approachable plans. Dr. Williamson's interests naturally carried over into this new situation, as did Karl's. Eventually the project came to be defined in terms of lactation and the dietary causes of precocious puberty. In the situation I entered the origins of the project were defined by the rational reconstructions I have just described, and with Karl's departure, this story was uncontested. This became history, and the deer project had become Dr. Williamson's preserve by the time I arrived.

Summary

In the last three chapters, I have endeavored to tell enough about the situation these people created that the action I portray in the next three chapters will be intelligible. In writing this thesis it was essential that I bring out the important features of the research setting as they were perceived by the group members because my ultimate aim is to show them in action there, and especially to show the place of their deliberations, perceptions and intentions in making that action go the way it did. Therefore, I had to portray the important features of the setting in a way that is compatible with the way these people saw them.

In Chapter Four, for instance, I began with considerable background information that Dr. Williamson and his students were
all familiar with, presented in a version of the didactic style that scientists use when they write textbooks, monographs and articles for *Scientific American*. Then I discussed the profession of studying game animals by showing how its members discussed it at a conference. Following that I gave a summary of Dr. Williamson's research career, as he represented it to his peers. In Chapter Five I tried to present the research facilities, routine procedures, and the relationships among Dr. Williamson, Karl, Emily and the deer, as a cultural package which these people created. I accomplished much of this through group members' recollections of the earlier days of the deer project. It is common for people to define their present situation by recounting a history of it, to establish what is here, now, by telling the story of how it came to be that way. That is, one makes up the present by making up the past. I tried to duplicate the way the group members might have constructed their view of the setting in 1977, as they worked in it, by drawing on interviews I did with them around this time. Here I take these peoples' stories more or less uncritically on their own terms.

Eventually, however, I had to deal with the formal research reports that Dr. Williamson and Karl wrote about their work, with the biological facts they produced. In doing so, I told their story as they published it and later recounted it, and then stepped back from it in order to fill in what was missing, and to explicitly place it in context, to show how and where it
was used in the current affairs of the group. Filling in what
the published paper leaves out demonstrates its ambiguity as
history, shows how it appears to describe what the researchers
did, while actually achieving something quite different: a
rational reconstruction of the research that both creates and
justifies its results, and shows them to be determined by the
true nature of the world, not by what the authors did. During
the time I have studied biology and biologists, I have heard
frequent mention of Popperian methodology. This is the reason I
used Medawar's comments on publication back in the second
chapter. I have often heard that voice: Popper described how
ture science is done, his is the true account of research.
Williamson's belief in this was exemplified by his use of a
Popperian version of the orthodox publishing format, which I
think was done in good faith. The alternative account I have
presented, based on what he said about his own work, shows that
the hypothesis which this experiment supposedly tested was not
formulated until after the results were in and being analysed in
the light of further information. I do not think this is either
unusual or necessarily reprehensible. It is simply an example of
the sort of repeated reformulation that is typical of scientific
research, or any other kind of inquiry. Popperian methodology is
no less ahistorical than conventional logical empiricism, and
serves exactly the same purposes: purity and ownership.

My alternative account also provides a vehicle for
portraying the institutional connections that supported this
research, and how they shaped Dr. Williamson's and Karl's technical concerns and the outcome of their work. Treating the fact of precocious puberty as a story told in the course of organizing and analyzing research enables me to make my point about the delicate, elaborate interweaving of rationality, action, group structure and deer that is the main topic of this thesis.

Now that I have set the stage, so to speak, the task at hand is to introduce the remaining characters and their projects, and then to show all of these people at work on their research. The next three chapters consist of a series of vignettes which are taken very directly from my field notes. They are meant to portray the developing action of the deer research from January 1977 until October of that year, as the summer's work was planned, set up, and carried out. The people I worked with will be shown dealing with the deer, one another, their equipment and facilities, bureaucratic institutions, information, and their own explanations of what is going on—all more or less simultaneously. I shall try to show how they wove together disparate elements to try to create a coherent fabric of research and constantly reorganized their routines and their understanding in order to bring about stability in the face of change and constantly impending disorder. This will come together in Chapter Nine, where we shall see how a formal representation of the fawns' milk consumption began to emerge from these proceedings.
VII. Professional standards and the new students' projects

Bringing the public world to the deer pens

Dr. Williamson bent over and looked at the deer through a hole in the pen door. She was standing alertly in the cage, staring at something outside the enclosure. "Still standing". He looked at his watch. "We'll give her ten more minutes, and if she doesn't go down, we'll up the dose on the next one." His voice echoed in the small unheated room at the centre of the deer barn. Around us, there were occasional sounds of deer moving and breathing. I stamped my feet and walked around, for it was winter, and cold.

January 22, 1977. My first trip to see the deer. I was trying to insinuate myself into the affairs of the group, and had volunteered to help them with their work. Today's task was to test a new sedative drug, called Rompun. Karl and Dr. Williamson were taking turns examining a paper published in Germany which described tests of this drug on European species of deer, at the Hamburg zoo.¹ They had come out today for the sole purpose of finding out how it affected the black-tails. What dosages would knock them out? How fast would the drug take

¹Reference lost.
effect? What would the indications of its action be: slow loss of coordination, or sudden collapse? They started with the smallest dose reported in the paper and were going to increase or decrease it from there, once the effect of that dose was known. My job was to catch the animals and help with the injections.

There being nothing to do but wait for the deer to fall down, Dr. Williamson regaled us with biologist jokes and other stories. There were these two brothers, see, named Craighead, wildlife biologists who studied grizzly bears in Yellowstone National Park. I recalled them from their articles in the National Geographic. They were interested in hibernation, and wanted to know how bears' body temperatures vary during the winter, how much their metabolisms slow down in this process. They decided to follow common clinical practice and use rectal temperature as an indicator of body core temperature, and take the bears' temperatures as they slept in their dens during the winter. Armed with thermometers, Vaseline, and considerable faith in the groginess of hibernating bears, they sallied forth. The thermometers and Vaseline acquitted themselves well, but as is often the case in research, some of the critical premises of the expedition were not supported by subsequent experience. One of the bears became aware of his participation

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in this adventure at the frontiers of knowledge, and arose to check it out. No injuries were sustained by either side, but they never did get that thermometer back.

It is, perhaps, the price of fame that the story is attributed to these men. David told me much later that he had heard of a graduate student at the University of British Columbia who had done this. Apparently he was the sort of man who just might do such a thing. David referred me to a "popular" book by Michael's favorite field biologist, in which the incident was reported. Recently I looked it up, and this is what I found.

My good friend Chuck was remembered at the university (where we obtained our doctoral degrees) for crawling into bear dens and sticking thermometers into the unmentionables of hibernating bears. Chuck had indeed crawled into numerous dens to discover if hibernating bears were in deep sleep. They were not. Every bear would awaken and groggily look over at his new den companion. Chuck went ahead anyway and tagged the ears of the bears. A few of them resisted. They would either chase him out or try to escape themselves—flooring Chuck in the process. This competent, silent man, working without flamboyance, without an audience on nationwide television to hold its breath and applaud, and without the slightest pretense, produced a memorable study of black-bear biology. He was no self-styled, drummed-up hero, yet he had the courage not to kill a wild, awakened bear, but to embrace it.3

"She's down!", Dr. Williamson exclaimed, and he rushed to examine her. Out cold, she was, relaxed, completely sedated. She had not fallen suddenly, as animals do when they are shot up with succinyl choline, the drug these people normally used.

Rather, she had slowly drooped then relaxed completely. Succinyl choline works by preventing the transmission of nerve impulses from the brain to the voluntary muscles, causing them suddenly to go slack. It was used to immobilize the animals for medical care, transportation, and milking. Unlike Rompun it is an on/off drug with no sedative effect. The animal is either active or paralysed. There are no stable intermediate stages in which the animal is mobile, but docile. One of the things we were looking for was the doses of Rompun that would produce these intermediate stages.

Now that the animal had gone down we had to wait and see how soon she recovered before we injected the next one. This gave me time to observe what was going on. I was just feeling my way around. Several months before I had approached Dr. Williamson about studying the research done in his group. Intrigued, he had agreed. He did not know what I intended to do, but then neither did I really. This spring I had enrolled in his graduate seminar in mammalian reproductive ecology, along with two of his graduate students, and had begun to spend time in the laboratory which served as the students' office on campus.

Things at the deer barn picked up with the arrival of David Simpson, one of the new students. I knew him from the lab and the seminar, and he eventually became my closest associate in the group. He was at the Forest to measure does for what I eventually realized was a sort of brassiere, although he called
it a harness. He explained, for my benefit, that he wanted to restrict fawns' access to their mothers' milk. The work done so far by Karl and Dr. Williamson suggested that only hand-raised fawns would achieve puberty in their first fall. One of the hypotheses on this subject was that there is a hormone in the does' milk, in the absence of which puberty can occur earlier than usual. David's proposal for checking this out was to select does with twin daughters, hand feed one, and let the other suckle at the mother's udder. Most of the time the mother would wear this harness, which would permit both fawns to be with her almost all of the time, but enable David to control the kind of milk they drank. This specific restriction was important because another possibility was that the sheer physical presence of the mother, or her social involvement with her fawns, or both, would be inhibiting factors. David wanted to disrupt this as little as possible, and so intended to keep the mothers and fawns together almost all the time. If the hand-fed fawns showed this 50% pregnancy rate and the others did not, the next step would be to ask what there is in the milk, and how it affects the endocrine system of the fawns. The fawns of the remaining does in the herd would serve as controls in this experiment.

David measured the deer we had drugged. Meanwhile, Dr. Williamson and Karl took up the subject of harnesses. Would David have to make custom-fitted harnesses, or would one size fit all? Should he use Rompun while fitting the harnesses, or use succinyl choline? What should they be made of? Being a
skydiver, David favoured tubular nylon and velcro. There were routine discussions of the health of particular deer in the herd. Some tales were told of an exceptionally powerful sedative drug called M99, which is ten thousand times more powerful than morphine. This drug is so potent that a few drops of it spilled on the skin will kill a human being. Apparently an English veterinarian working in a farmer's field had had just this accident, and passed out before he could get back to his van to inject a dose of the equally powerful and fast-acting antidote. He died. The usual practice with this drug was to keep a loaded syringe of an antidote on hand in case of accidents. A graduate student working with grizzly bears had mixed up his syringes on one occasion, and instead of giving a wakening bear a further dose of M99, had injected the antidote. He spent the rest of the day in a tree. Because of its potency the use of M99 is forbidden in Canada, although it is the best drug available. This irritating restriction reminded Dr. Williamson and David of their other problems with governments. For instance, scientists working on bighorn sheep are hampered by jurisdictional disputes between the Canadian Wildlife Service and the B.C. Fish and Wildlife people. The former organization controls one herd of bighorns, the latter another herd. The people working in these two organizations routinely engage in typical bureaucratic turf wars, which makes studying the bighorns difficult at times.

Such topics came and went, interwoven with practical matters of immediate importance. For instance, they discussed
how to fill in for Elaine who was going on a holiday soon, and could not take care of the deer for two weeks. From time to time during this pattern, deer were weighed, injected, and watched. It became apparent that black-tails are more sensitive to Rompun than are the red deer and fallow deer described in the paper, so that it took smaller doses to bring them down. The paper had indicated that an excited animal might over-react and die if given this drug, so we chose the most agitated animal in the pens, and injected her. She reacted no differently from the others. The remaining task was to find the dose that would sedate an animal but leave it standing, but that was left for another day, and we went home.

Several days later we heard from the endocrinologist down the hall that the deer had still been groggy the next day when he had gone to the Forest to take blood samples. This augured poorly, for these long term effects can be a hindrance, and for all its faults, succinyl choline dissipates almost as quickly as it acts. In time, we did use Rompun for some tasks, such as sedating deer to measure them and take blood samples, but we still used succinyl choline as well.

**Professional standards: passing on the Word**

Don Dempster, another new grad student, was giving a seminar on nutrition and pregnancy. Topics like these can be very frustrating ones to discuss because it is very difficult to
find out exactly what a wild animal eats—how much of which specific foods at which times of year it consumes. A deer, for instance, eats many different species of plants, but it eats only specific parts of them, and the food value of a given plant type varies widely during the year, so that the animals are constantly shifting from one species to another as the year goes by. Whitetail deer which eat the terminal shoots of berry bushes in the spring will feed almost exclusively on acorns in the fall, if they are available, and may resort to coniferous branches when they are starving in the winter. These technical difficulties spurred Dr. Williamson to make the following statement about the reliability of studies published in the literature.

He said that there are basically two kinds of ecological studies. In the first kind the hypothesis is set before the observations or experiment are done. He proposed as an example the hypothesis that lynx will not breed during a very cold winter, simply because the mother cannot find enough food to cope with the energy drain of pregnancy, in addition to that imposed by extreme cold. The investigator can go out and gather data pertaining to the problem thus defined. If lynx do not breed in an extremely cold winter, it may be that energetic ...
considerations prevented it. If they do breed you can throw out the idea altogether, and move on to something else. This Popperian kind of work meets with Dr. Williamson's Good Lab-Keeping Seal of Approval, and he insists that his students organize their work around testable hypotheses. What usually happens, he said, is that data are collected for one reason or another and patterns are seen in it. Some explanation is sought, a statistical correlation is established between two things, and everyone goes home happy. His comments on this kind of work are more than unkind.

Dr. Williamson and David, especially, felt it was very important to follow what they considered to be the scientific method. I found this to be typical of the ecologists in the department. One even took the step of defining the scientific method in his undergraduate lectures, in accordance with the dictates of Karl Popper, something I had never seen a physicist or chemist do, for instance. Ecologists are quite impressed with the idea that it is very important for them to be "scientific" in their research practices. They have been quite determined to get away from the "soft" image of their predecessors, the natural historians. Like the wildlifers, they feel that their work is considered to be rather loose and crude by the "harder" scientists such as biochemists and geneticists, as I discussed.

On another occasion Victor, the PhD candidate working on moles developed this theme further for me. He gave me an example of faulty reasoning and its consequences. Regression analysis of field data shows that the number of species of birds in a forest is strongly positively correlated with the number of species of trees in the forest. That is to say, in a forest with many species of trees, there are many species of birds, and vice versa. It has been claimed on the basis of these data and regressions, Victor said, that the number of bird species is controlled or caused, by the number of species of trees. This argument has then been backfilled with ad hoc explanations. For instance, if there are more species of trees, there will be more kinds of seeds, insects, hiding places and nesting sites available to birds. In ecological terminology, there are more niches. All well and good, but nothing resembling a proof has been provided. The argument could equally logically run the other way, with the number of bird species controlling the number of trees species. Vic spoke rather strongly, apparently feeling the issue to be a crucial one. It distressed him that a correlation would be accepted so easily by ecologists as if it were a proof, giving what is merely a suggestive argument the spurious appearance and validity of scientific proof. Only a t-test, or an analysis of variance could be regarded as proof. On top of this, arguments like the above could become ensconced in the ecological literature and, once accepted, stand in the way.
of other investigations of the same phenomenon. Vic suggested that someone applying for a grant to look into the effect of microclimate on birds might have his suggestion waved aside with the remark, "Micro-climate doesn't regulate number of bird species, Jones showed that number of tree species does."  

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6For two examples of the effect of entrenchment of supposedly proven theories in biology, see, Rosin, R. (1980) "The honey-bee dance language hypothesis and the foundations of biology and behavior". The Journal of Theoretical Biology, v. 87, #2, pp. 457-481, and Caughley, Graeme (1968) "Eruption of ungulate populations, with emphasis on Himalayan thar in New Zealand". Ecology, v. 51, #1, pp. 53-72. Both contain criticisms of classic theories that are shown to have been "proven" by inadequate means. Rosin's paper is about the famous dance-language hypothesis advocated by Karl von Frisch, which holds that honey bees communicate the locations of food by means of dances performed at the hive. Rosin takes the very unusual step of arguing that the entrenchment of this hypothesis as a classic in textbooks down to the highschool level, and its endorsement by the Nobel Committee, has posed a serious hindrance to inquiries into its shortcomings. He quotes Thomas Kuhn's The Structure of Scientific Revolutions to support his contention that sheer inertia and custom are among the reasons that this theory has not been disposed of. In professional scientific journals, one is normally expected to argue a case in terms of its technical merits, not its political affiliations. Caughley's argument refutes the classic case of the eruption and catastrophic crash of the deer population of the Kaibab Plateau in the United States, following reduction of the wolf population. Caughley, too, points out that the Kaibab case is a classic in textbooks, but does this only in passing. He is well known among wildlife biologists, however, for his opposition to traditional methods for counting populations of wild animals, particularly those employed by government biologists, which date back to the classic days of wildlife biology.
The force of evidence: David's project collapses

At the roundup on March 25 David looked anxious, even angry. His research project was in jeopardy because its fundamental premise, that mother-raised fawns do not achieve precocious puberty, had been resoundingly contradicted several days earlier. The fawns bred the previous fall had been examined with an ultrasonic probe, and fetal heartbeats had been detected in some mother-raised fawns. David's entire research project, and a significant part of Karl's dissertation, depended on this finding. A great deal of work would have to be flatly discarded if the result of the examination were real, so Dr. Williamson ordered a re-examination, using a different method. These does were to be X-rayed at the office of the local veterinarian they usually patronized. At this point in pregnancy, the fetal skeleton is sufficiently calcified to throw a shadow on X-ray film, providing a definitive indication of pregnancy.

Before this test could be done, all the deer had to be rounded up again, and the does in question separated from the rest of the herd and sent to the vet. Round-ups took a lot of people, and were among the few occasions that brought the people working for Dr. Williamson together at one time. Sometimes, even those who did not work on deer would be asked to come. This time there were Karl, Emily, David, Don, Brenda, Michael, and myself. Don, the another new graduate student of Dr. Williamson's, was to play a large role in the summer's work on the deer herd.
MAP I: The deer pens.
Michael was a senior undergraduate who intended to work with Dr. Williamson on bighorn sheep. I had been with these two in Dr. Williamson's course in field ecology the previous summer, when Don and I were finishing our undergraduate degrees. Brenda was a technician working for the endocrinologist down the hall. He had worked with Dr. Williamson on the deer and still used them occasionally. He paid off this debt by lending Brenda to Dr. Williamson from time to time to work on the deer.

To round up the deer we flushed them out of the wooded far corner of the large enclosure and then formed a line by stretching long wide strips of black plastic between us. There is a map of the deer pens on the facing page. This formed a barrier all the way across the enclosure which moved toward the middle pen, herding the deer before it. Occasionally, a deer would turn and bolt through the line, and one time all the others followed suit, so that we had to start over again. We yelled and shouted to herd the deer, which were frightened and bolted nervously back and forth once we got them into the middle pen. Sweating profusely, we ran after the deer, trying to cut individual deer out of the herd (we could identify them by their ear tags), and funnel them into the entry passage of the deer barn, and from there into individual pens. Deer are very agile and fast, and when frightened they rush about blindly, which gave the scene in the pen some of the hectic character of a Hollywood cattle roundup. In addition, and this may be only my

retrospective 'tinting' of the situation, there was a vague sense of anxiety and foreboding, because so much was at stake, and the unfavourable conclusion was almost certain.

This was a major setback for David. He had been working on the maternal inhibition hypothesis since his arrival in September 1976 from another province. He says that on the day he arrived at the university Dr. Williamson had taken him down to the cafeteria to discuss their work. David came assuming that he would work on jackrabbits for, when he had written to Dr. Williamson asking to study wildlife under him, Williamson had written back saying that he expected money to be available from the Fish and Wildlife Branch for studying jackrabbits. If David was interested in this, he was welcome to come. Over coffee, Dr. Williamson explained that the money for jackrabbits had not materialized, but that there was an interesting development in their deer research, concerning maternal inhibition of precocious puberty. Dr. Williamson suggested that he look into the maternal inhibition hypothesis, and develop an experiment to test it. David agreed, and set about reading the literature and familiarizing himself with the deer with his usual efficient enthusiasm.

Things were now rather upset for him. If the mother-raised fawns were confirmed to be pregnant, then David's project got the axe, it was as simple as that. He would have to invent a new research project and get it underway very soon indeed, or he would lose an entire year of work. If he switched to a
completely different problem or species, he would even lose the value of much of the reading he had done so far. He was effectively committed to studying precocious puberty in these deer, and had to make something of it. If David was tense, so was Karl. He was well into writing a dissertation that depended in part on the maternal inhibition hypothesis, and he was partly responsible for David's beginning to work on this problem.

The people who came to round up the animals spoke sometimes as if these fawns were already known to be pregnant, and speculated on what David might do next. David said, months later, that things had actually worked out for the better—and I believed him—but that is not how he felt on the day of the roundup.

We left the forest as soon as the fawns we wanted were in cages. They needed time to settle down before we could sedate them and transport them to the vet's office. When I rode back to the university with Don he speculated that David might work on nutrition, as Karl had done. He suggested that I work on nutrition as well. Earlier I had mentioned to him that I wanted to take on a small project within someone else's work, in order to get myself more involved in the daily affairs of the research. Don felt that he, David and I could form some sort of triumvirate and coordinate our work. I told him I'd think about it. Several days later, I heard that the mother-raised fawns had been confirmed to be pregnant. X-ray photographs of them had revealed fetal skeletons.
Don's work: research and management

Dr. Williamson hired me as a research technician from May until August of 1977. He explicitly placed me and the other summer technician, Kirstin, under Don's supervision. Don had completed his undergraduate degree several years before, and gone to work for environmental consulting firms. He returned to university to get a masters degree in order to pursue his career in consulting. As nearly as I can determine, Williamson had wanted someone to do a behavioral study of lactation, and when Don came to him and indicated his interest in entering graduate school, Dr. Williamson assigned him this project as part of his qualifying work. Don did a study of lactation in the summer of 1976, and now, as part of his graduate research, was going to do an expanded study of lactation which included measuring the milk intake of the fawns, as well as recording the behavior patterns of suckling. His work in 1976 now served as a pilot study for the work of 1977. The expanded study was going to involve around-the-clock observations and measurements, and Don was going to need a great deal of assistance from the rest of us.

The milk consumption work fit in with Dr. Williamson's long standing desire to know how much load lactation imposes on the mother. The study of suckling behavior came about because of a specific outside connection. A prominent ecologist, specializing in mountain goats and mountain sheep, had made a theoretical
claim that involved the amount of suckling young animals do. Valerius Geist proposed that populations of animals that are expanding their range are quite different from those which have a stable range. Individuals in the expanding population will be larger and more vigorous than those in the stagnant population. They will die younger because predation and accidents will be more frequent. Their young will be more playful and will suckle more. Such theories are relatively easy to formulate and test with respect to mountain goats and mountain sheep because they live in small groups in open areas, shunning forests. The goats and sheep rely on their vision, and the combined watchfulness of the herd, to avoid predators. This makes them easy to study, for they can be followed easily with a spotting scope. By contrast, deer are extremely difficult to study in the wild, because they use the cover of the forest for protection.

One of the things field ecologists watch for is the relationship between the mother and the young, because this is so crucial to the survival of the species. They keep records of suckling, recording the time and duration of each bout. This immediately raises the question—is milk consumption actually related to suckling time? Is time spent suckling a reliable indicator of the amount of milk drunk? If these expanding populations are characterized by high suckling frequency, is this because they are getting more milk from their mothers, or

do they simply have closer social relationships to their mothers? In short, suckling was of some interest to field biologists, and Dr. Williamson had a chance to see if there was a connection between the time spent suckling, and the amount of milk consumed. This was not the primary consideration, though, most of his interest went to the energetics of reproduction. The measurement of milk consumed by fawns suited this end as well.

The first month of work, May, was almost entirely given over to preparing the facilities at the Forest for the summer's work. I worked there almost every weekday with Don and Kirstin, Kirstin was a biology undergraduate who had heard of this job from Don, her laboratory instructor that spring in a course taught by Dr. Williamson. In some respects, our job was not unlike working for a gentleman farmer who keeps horses. We re-roofed the hayshed, improved the drainage ditches around the pens, put down bark mulch in the middle enclosure, and fixed fences. Elaine was there almost everyday, taking care of the deer unobtrusively, feeding them and hosing down their pens.

There was a major discordant element in this scene, the scale. The fawns' milk consumption was to be measured by weighing the animals before and after suckling. For this we needed an extremely sensitive scale, which could weigh large animals to within a very few grams. It had to have an extremely stable platform, for standing on a moving surface makes deer extremely nervous and hard to weigh accurately. In the fall of 1976, Dr. Williamson committed funds to a miniature truck scale
that would measure objects of up to one hundred kilograms mass to within two grams, that is, to within one part in fifty thousand. After this order had been made, Don came across a different method in his reading. In this method, the does are injected with radioactive caesium, which substitutes for calcium in milk and bones. The rate at which it comes out in the doe's milk can be determined by analysis, and the amount of caesium in the fawn's body can be found out from a full-body scan with a radiation counter. The fawn's rate of milk consumption can then be determined from this information together with the fawn's rate of excretion of caesium. Technically, this method was superior to the weighing, but the funds had already been committed. Furthermore, it would probably have been impossible to get permission to use radionuclides in the Forest because some caesium would have gotten into the ground water as a result of the deers' excrement being washed away by rain. It is difficult to get permission to release radioactive materials into the environment unless you are likely to do it on an extremely large scale.

The scale manufacturers were behind schedule in May because certain parts of the scale sensor mechanism had not been delivered by a subcontractor. Toward the end of May Don and Dr. Williamson began to get quite agitated. The fawns were due to be born very soon, and if the scale was not delivered in time, most of the summer's projected work could not take place. Eight months of planning would go down the drain. I became agitated.
Even if the scale did arrive in time, we still had to learn how to use it. It was new to all of us. We were going to be using this machine at the limits of its sensitivity and had no idea what "bugs" it would contain. In fact, the scale caused us problems all summer, and the sense of "getting ready" that we had in May never really left us.

I became aware of how the research was being organized when Williamson called me into his office in mid-May, and told me about the project he wanted me to take on. Don, he said, had done as most graduate students do, and bitten off more than he could chew. Accordingly, he was going to delegate part of Don's project to me. He wanted me to take responsibility for recording the solid food consumption of does and fawns. This meant devising a way to feed them separately, although each doe and her fawns would be in the same pen. As well I had to see to it that the feed boxes were weighed at the right times and replenished when necessary. Afterwards, I would have to transform and graph the data so that the others could use it. All very low-powered stuff, but it was in the middle of the research activities that summer, which is what I wanted.

He placed this in the context of his own project. This summer he wanted to find out the age at which black-tail fawns are able to maintain their growth rate on solid food alone. He called this the achievement of nutritional independence. He felt that this point of independence would arrive some time before the fawns were weaned, and would indicate the youngest age at
which the fawns could survive without their mothers. To
determine this age meant separating fawns from their mothers for
short periods of time, a day or two, feeding them only solid
food, and keeping a very close record of their weights with this
super-accurate scale. If a fawn's growth curve dipped slightly
and then went back to normal, then he assumed that it could get
by without its mother. Broadly construed, this work included
following the milk and solid food intakes of the fawns to
document the changeover from an all-milk diet to and all-solid
food diet. Most of this was Don's work on milk consumption and
the remainder was now mine—solid food consumption. Dr.
Williamson assigned me this job unilaterally. Don knew nothing
about it until I told him.
VIII. Formalizing action and talk, 1: the organizational meeting

Setting out the hypotheses

Dr. Williamson stood at the blackboard of the lab with David. He had drawn a graph on the board, and was explaining it to David in his penetrating voice, gesturing briskly with his hands. The graph had recently been developed by Karl, and Dr. Williamson appeared to be showing it to David for the first time. It is a model of puberty attainment in relation to protein intake, which later appeared in Karl's dissertation. David was working very hard to prepare a research project for the fall breeding season. It was to be a test of the hypothesis that precocious puberty is caused by the higher caloric value of the fawns' diet in captivity, in comparison with the diet available to free-living animals. Karl was at that time arguing in his dissertation that protein was the specific dietary element responsible. David's picking calories was not unrelated to his feelings of anger at being misled by Karl on maternal inhibition. After the x-rays mentioned in the previous chapter, the records of the herd had been gone over closely, and Karl discovered that he had overlooked one mother-raised fawn, not used in any of the experiments, who had gotten pregnant in her first fall, and had given birth the following spring. If Karl
had not overlooked this, the whole maternal inhibition episode would not have occurred. Dale was quite put out that he had been put on a wild goose chase, and told me quite often that he held Karl responsible for this. Consequently, he followed Williamson's explanations with some interest.

When he and Dr. Williamson observed the last of us entering the room, they stopped their discussion, and David sat down at a laboratory bench with his notebook. The rest of us took seats here and there in lab, and Dr. Williamson remained at the blackboard. The impression I had when entering was that we were to settle among ourselves what had to be done, and to make up schedules and the like to coordinate our efforts. The major items on the agenda had been established well in advance, but we had not decided upon the specific arrangements that would make it work. In addition to David and Dr. Williamson, Don and Kirstin and I attended, and Karl was in and out of the lab during the afternoon. Dr. Williamson had asked him to be available if needed, but since he was completing his dissertation, his role in the summer's work would be small, and he did not have to take part in the planning.

"We have two projects," Dr. Williamson announced, "which are closely related." He turned and wrote the hypotheses of one project, David's, on the blackboard:

\[
\begin{align*}
H1: & \text{ Puberty attainment is energy (calories) dependent.} \\
H2: & \text{ Wild fawns cannot obtain enough energy from browse to attain puberty in their first fall.}
\end{align*}
\]

He proceeded to write down a flow chart for David's project, but
FIGURE III: Growth of fawns separated from their mothers at various ages. (1)
said that we were not going to discuss it at length, because it was to be done in the fall. Our objective here was to organize the arrangements for the summer, making sure along the way that they did not interfere with David's preparations for his research in the fall. Accordingly, he went on to the summer's problems. Turning to us he said, "The first stage in any scientific investigation is to decide which question or questions you want to ask." Then he drew on the blackboard the graphs you will find in Figure III, on the facing page, and wrote down the hypothesis:

H: The age of nutritional independence of fawn from doe is determined by the level of maternal milk production.

Williamson wished to determine the age at which a fawn can survive and continue to grow without its mother's milk. He intended to do this by removing fawns from their mothers and feeding them only solid food, while keeping very close track of their weights. Fawns that were too young would presumably lose weight quite quickly, as in Figure III(a). Those which were nearly autonomous would continue to gain weight, but do so more slowly than they would if they were suckling. This is shown in Figure III(b). Nutritionally independent fawns would keep on growing without any marked change in their rate of weight gain, as in Figure III(c). These last fawns might still be suckling, but Williamson was fairly sure that suckling went on after the fawns could do without it. Such suckling, he said was "only social".
Don, whose work was a major part of the deprivation project, challenged this statement. He asked if the age of nutritional independence might not be a function of the rumen development of the fawn, proposing that as a fawn's digestive tract matured, it would need progressively less milk. Presumably, Dr. Williamson had meant that the mother's milk production was timed by her to decline at the time when the fawns would not need it any longer. Don's counter-proposal was that as the fawn matured, it would need less milk, and because it would take less, the mother's production would decline. You could take this to mean that the fawn "leads", as it were, with the mothers' patterns of milk production being an adaptation to the fawns' changing needs. Would the fawns' independence not be a consequence of the maturation of the gut? And if this were so, what external features of the animal could be used as indicators of the maturity of its digestive tract? Weight? Age?

Dr. Williamson referred him to Aaron Moen's book *Wildlife Ecology.* It showed the rumen development of white-tail deer, he said, and Don could use it as a guide. Then he suggested, offhandedly, that perhaps Kirstin could do something on rumen development, dissecting the fawns we lose, and tracing the development of their digestive tracts. This rather casual assignment had quite significant consequences, which will be discussed in a later section of this chapter. He went on to say

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that he did not mind killing fawns for research, if we were going to lose them anyway without special care, although he was aware that Karl and Elaine did not agree with him on this.

Dr. Williamson reconsidered his hypothesis after the discussion of rumen development. A study of roe deer, he said, showed that the amount of milk deer produce varies greatly from individual to individual. Furthermore, fawns stimulate the let-down of milk, which presumably leads in turn to the production of more milk. They probably will take as much milk as they can get, for as long as they can get it, because it is such good food. In addition to all this, a doe may have twin fawns or just a singleton, and the fawns may be taken at any time by predators. It follows, then, that does' milk production must be quite elastic up to a point. He reworded his hypothesis to:

H: What physiological "state" correlates best with the onset of nutritional independence of the fawn?

This was now just a question, not a testable hypothesis, but that was not crucial at the moment.

Resources and schedules

This action was a sort of warm-up period and we went on very quickly to work out the basic plan of action for the summer. On Wednesdays there would be an observational study of suckling behavior. For twenty four hours we would watch the...

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fawns in shifts, recording, and timing the durations of the suckles. On Thursdays the fawns would have access to their mothers for only short periods of time, to suckle. They would be weighed before and after suckling, to determine how much milk they had ingested, and the suckling events would be recorded as they had been the previous day. On Fridays the mothers would be weighed, to record the changes in their weights during lactation. On Mondays they would be milked, to keep track of the changes in milk quality. The solid food consumption of the animals was to be measured for three consecutive days, in accordance with standard agricultural research practice, Dr. Williamson said. Food weighings would be done from Tuesday to Thursday.

One intractable problem faced us all. The fawns were not yet born, and we had to be ready to go full tilt once they appeared; our arrangements for them had to be complete by that time, whenever it might come. However, the specific arrangements for the work depended on how many male fawns, and how many females, there were. David needed a large number of female fawns, and Don wanted both males and females in his work. David did not want his females to be involved in Don's work, because it might have some unpredictable influence on their development, and on their chances of achieving puberty that fall. On the other hand, during the summer David would be weighing and measuring his fawns, and taking blood samples from them, which meant they would be largely free for Don's work. Dr.
Williamson's deprivation study would be far too disruptive of David's fawns' development, and Williamson was not intending to use females in his project.

There were 18 pregnant does. For the purposes of planning, Dr. Williamson, Don and David assumed that there would be equal numbers of male and female fawns, and that all does would have twins. The assumed distribution was nine male-female, and four and one half male-male, and female-female pairs. This was necessarily impossible, but would do for planning purposes, since no-one could predict the exact result. Don wanted five male-female pairs. He sought to have equal numbers of females and males because it is well known that males grow and mature more quickly than females, and so the results for males are not representative of the species.3 Even if the sex ratio were as assumed, David would still not have as many unmanipulated females as he wanted, if Don used females in his study. Dr. Williamson took the position that the difference between males and females so far as this study was concerned was only one of rate of growth and development, and that Don could get results representative of females by extrapolating from males. This difference between rates of maturation is a well-established technical point, but in this case more was at stake than a matter of fact. The negotiation of the distribution of scarce

3Bandy, P.J. (1955) "A Study of Comparative Growth in Four Races of Blacktailed Deer (Odocoileus hemionus columbianus)". PhD dissertation, Department of Zoology, University of British Columbia.
resources had come to pivot on the relative importance of this fact, compared with the unknowable possibility that Don's work might interfere with David's. The fact took on a political significance that was intrinsic to its consequences in practice. Dr. Williamson resolved the issue for the time being by stating that the phenomenon Don referred to simply did not make enough difference to warrant risking David's work. The issue would not be finally settled, though, until enough fawns had been born that the work had to begin. For the time being, it was decided that David would get all the female-female pairs, and that Don would get the female-male pairs and the male-male pairs, unless there were a shortage of females, in which case Don would have no special claim on females.

With this settled, the remaining matters fell into place fairly easily. Blood was to be sampled on Mondays, and does to be milked were to be separated from their fawns on Sunday evening. Dr. Williamson drew more and more heavily on Karl's expertise in this phase of the meeting. How long would the does' feeding be affected after they were immobilized with succinyl choline, he asked? For only a few hours, Karl replied. How early can the deprivation work begin, would it be dangerous to begin with four week old fawns? Karl pulled some old theses and dissertations off the bookshelf, and went through them. These were the products of the early work done at the University of British Columbia, mentioned in Chapter Six. One of them showed that males had been weaned at the age of three weeks on a diet
of grass clippings. Presumably the high silica content of the grass had strongly stimulated the development of the rumen. However, these animals had subsequently grown rather slowly. Williamson and Karl concluded that deprivations could begin at four weeks, provided that the animals' weights were watched very closely, and the fawns were returned immediately to their mothers if the weights continued to drop. Would the fawns be rejected by their mothers after a two-day separation? Not once they were past the age of forty-eight hours.

After this Dr. Williamson left, and Karl gave us a half-hour long formal lecture on the health care of the deer. Dale had been given primary responsibility for the health of the herd, taking this over from Karl, but we all were supposed to understand the issues. Karl detailed the diseases to which the animals were subject, and the treatments he had used. With that the meeting adjourned, and we all wandered off, rather dizzy from information overload. Several days later, Don distributed a schedule summarizing our plans. A xerox copy of this handout is found in Appendix I.

The scale: a crucial piece of equipment and its quirks

We got our much anticipated new scale in early June. Introducing this new piece of equipment proved to be difficult, for it immediately gave notice that it was not going to cooperate with us. Assigning malevolent traits or mere
willfullness to a mechanical device may seem unscientific, but most people who have worked with computers, delicate machines, or horses, know that whether these things are inanimate or merely non-human, they possess a "will" of their own, or might as well, for it has the same effect. A more acceptable term than "will" might be "unknown properties". The fact remains that the scale did not work at first, and from time to time thereafter it went on strike without notice, thereby showing itself to be a truly Canadian piece of equipment. It became a substantial part of the subject-matter of our inquiries, which is normal in scientific research.

The scale was a custom-built, miniature version of high-technology industrial scales used to weigh very heavy objects, such as loaded trucks. In such scales, the weight on the platform is transmitted through a series of levers, to place a bending stress on a piezoelectric crystal. This is a special kind of crystal, which becomes electrically polarized when subjected to a mechanical stress, and will be physically deformed if electrically polarized. The voltage produced under mechanical stress is proportional to the magnitude of that stress. Properly amplified, calibrated and displayed, the signal from the crystal module of the scale provides a measure of the weight on the platform. The weight was shown on the digital display of an attached electronics module that contained a microprocessor. A special feature of this scale was the averaging button. If you pressed it, the microprocessor's
hard-wired program would take ten weights over a three second period, store them in memory, and display their mean. This was intended to compensate for the fluctuations in the displayed weights of the deer that would result from the deer moving around on the scale. It was one of the scale's features that turned out not to be useful under working conditions. With the scale came a 5.000 kilogram test mass, which we could use any time we wished to test its accuracy. The crystal module of the scale could handle masses of up to one hundred kilograms without overloading, and could measure their weight to within two grams. This is accurate enough to detect the weight a fawn loses in half an hour as the carbon dioxide and water produced by cellular respiration escape from the lungs in expired air.

**Trying to make life regular**

June 6 saw us cautiously pessimistic about the scale. On the previous day it had varied by up to one hundred grams in successive weighings of the test mass. Since we expected the fawns to drink roughly this much each time they suckled, this was bad news, and tempers were visibly frayed. The scale's performance was quite erratic. It showed no consistent bias, such as always weighing the test mass two hundred and nineteen grams too light. If this had been so, we could simply have reset the zero point on the display, and because we were going to be dealing with differences in fawns weights, this kind of
inaccuracy would not have put an end to the milk consumption work.

A great deal rested upon the scale's performance. This research was months in the making. Essentially, a year's research funding, and a year of Don's and Dr. Williamson's time, were invested in this. Half a dozen people were committed to spending the entire summer on the research. Trouble-shooting began. We wondered if the movement of the deer on the platform caused the problem. Not this problem. The test mass did not move, and it too was inconsistently weighed. Did the readout vary when people were moving around in the room? Sometimes. And it also varied when they did not move around in the room. Could the wind, blowing through this drafty deer-barn, affect the reading, through some strange Bernoulli effect on the platform? I pondered wind-proofing the room, not relishing the task. Later in the month, at a friend's suggestion, I windproofed the scale enclosure I had built. This changed nothing.

Other preparations continued that day, on the assumption that the scale could be made to work. We put plexiglas doors on the five contiguous pens that were to be used for suckling observations. The plan was to sit in the central room of the deer barn and look out into the these pens. Since they radiated from the centre like slices of a pie, it was easy for one person to watch two pens at once. I was waiting for some of the results of my work. One of my responsibilities was to put the does on a fully standardized diet of commercial cattle feed, at the right
times for the food consumption measurements. We were to use calf starter and pelletized alfalfa hay, purchased from the local outlet of Buckerfields, a major agricultural supply firm in the area. The deer ate calf starter every day, but in combination with loose alfalfa hay, and browse that Elaine cut in the nearby bush. Karl and Dr. Williamson explained to me that this diet is unsuitable if you want to measure daily food intake, because deer are inveterate nibblers. They eat the ends of branches and only the tops of hay. Having deplorable table manners, they strew the remains all over the ground, and picking up all the twigs and bits of hay to weigh them was impossible. On the other hand, they very politely ate pelleted rations whole, spilling very little. There were no discriminations for them to make among or within the pellets.

The alfalfa was in the diet to give the deer roughage, which is in short supply in calf starter, and the catch was that no one was sure that the deer would eat enough alfalfa pellets, which they thought the deer might find unappetizing, to get the roughage they need. No one knew, because the experimental diets used earlier had contained alfalfa, so that administering pellets containing only alfalfa had not been necessary. They thought it might be necessary to give the animals loose hay for the part of the week when their intake was not being measured, but this added complications, because animals often are reluctant to switch from one food to another, as any cat owner knows. As a consequence, I had to find out whether or not the
deer got enough roughage on a diet of calf starter and alfalfa pellets, and to do this, I, with Elaine's cooperation, had to keep track of the firmness and other pertinent qualities of the does' bowel movements. From such humble beginnings grew a two year concern for the quality and quantity of deer shit produced in this research.

As well, I was contemplating how to get a doe to eat out of one pair of boxes and her fawns out of another, even though they lived in the same pen. This had to be worked out because we needed to know how much energy and protein the does needed to lactate, and how much of these the fawns were getting from solid food, in addition to milk. It was a simple matter to put the does' boxes high out of reach of the fawns, but the fawns' boxes had to be fitted with some kind of variable sized head hole that would exclude their mothers, and allow for the growth of the fawns. Conceptually, this problem is the last word in simplicity, and typical of the controls we were trying to impose. Without money, however, it is extremely difficult to solve effectively. In more lavishly funded agricultural research, David said, much more sophisticated methods are within reach. David said that when agricultural researchers want to put cattle on individually controlled diets, but not confine them to individual pens, they use feed boxes activated by radios. An ultra short-range radio transmitter is fitted around each animal's neck, and each food box has an electrically operated lid that is triggered by a radio receiver attached to it. Each
box will receive signals of only one frequency, that of the
transmitter around the neck of the animal whose ration it
contains. The box opens when the animal reaches it, and closes
when it leaves. Each animal's diet can be closely specified, and
intake measured. None of this was available to us, of course.

Much of our short term planning went into developing
habitual behavioral routines and schedules, and arranging the
physical site. Once established these faded into the background
of our deliberations, but they were not just props or furniture,
they were part of the action, stable patterns in our behavior,
that we just did not pay much attention to until there was
trouble with them. They had to be maintained, of course, and on
June 6 we tidied up a number of these established routine
matters. David had assumed his role as herd doctor, and went
about medicating the deer. We caught and ear-tagged several
adult deer, an athletic diversion, to say the least. As ever,
Elaine came and went quietly, changing the deer's water,
cleaning out their pens, and replenishing their feed. Most of
our conscious attention went to whatever was problematic at the
time, but this always happened against a background of habitual
behavior that, I suspect, provided much of the momentum that
carried us forward. All of us had to learn most of these
customary practices in order to participate.

At this time in the summer the weighing routine had not
been established, and on June 8 and 10 we spent considerable
time discussing it. One problem was that the fawns moved around
nervously on the scale platform, which caused the displayed weight to vary widely and rapidly. We tried blindfolding the fawns, a procedure used to calm horses. It does not work on fawns. Kirstin, the smallest of us, took a fawn in her arms and sat on the scale, but this did not work because the fawns struggled. Sheep can be tied up for this kind of procedure, but we found it did not work on fawns. The only conclusion we could reach at that time was to leave the fawns free to move on the scale, and chose an auspicious moment to press the sample button. We had already discovered, however, that the sample period was too long. Two and one half seconds gave the fawns ample time to shift their weight several times. This meant we had to press the button repeatedly, and hope to cover a period when the animal did not move.

On June 14 we practiced observing suckling, using the first fawns born that summer. All of us had to learn to see the events that Don wished to record. For instance, it was important to discriminate successful suckles from unsuccessful attempts, and strong attempts from passing grabs at the nipple. We had to record the initiator: did the mother come and rouse the fawns, or did they approach her? In addition, we were to note when the fawns began rumenating, when they ate solid food, or their mothers' faeces.

That day, we also considered the timing of suckling watches. One day a week, for 24 hours, we were going to observe and record suckling. We would work in pairs, and spell each
other off at regular intervals. The question was one of how long
the shifts should be. We did not have enough people for very
short shifts, such as two hours, but feared that the observers
would doze off during very long ones, say, six hours. Four hours
seemed the best compromise at the time, but the temporal pattern
was not set until late in June, when Don drew up a schedule.
Most such arrangements had a very tentative feel at this time
because most of the fawns were yet unborn and the problems of
sex ratio, and assignment of specific animals to specific people
overshadowed other concerns. There could be no planning of how
the fawns should be placed within the barn, for instance.

Longer term planning was also common, and often it was
mixed in with discussions of more immediate import, so that we
skipped around in time fairly cavalierly. Early in the afternoon
of June 14 I was sitting in the sun beside the hayshed,
dismantling and rebuilding the food boxes I had made earlier.
David and Dr. Williamson were sitting near me, taking a break
from learning to watch deer drink milk. They made some helpful
remarks about the feed boxes, and we passed the time with a
discussion of sexual dimorphism in human beings. Then Dr.
Williamson said suddenly, "You know, David, I've been thinking",
going on to state that David must be very sure to document the
solid food consumption of his fawns before he weans them and
puts them on their experimental diets. That way, he will be able
to tell if they have some kind of reaction to the change in
their diet, if they continue to eat normally or not. David
replied that he had been thinking of that himself, just recently. Around that time changes of diet were an issue, as I have mentioned above, because of the problem of putting the does on alfalfa pellets. Although one can never be sure what it means when a subordinate claims to have been considering the same questions as his supervisor, it did appear to me at the time that this specific reference to dietary change in David's work in the fall was connected to the switch being contemplated now, for we were preparing to put the does on a fully standardized diet.

Later that afternoon, Don and Kirstin had a very frustrating time trying to work out another of our intended routines, diapering the fawns. The problem we faced is that baby deer defecate and urinate only when suckling, and do so in response to the mothers' licking their anal and genital regions. And, as is common among mammals, the mother consumes the excrement of the young. Presumably, this behavior came about because animals which consume their young's excrement leave fewer clues to predators of the existence of weak helpless prey. If this is so, the behavior has a sound practical role in the lives of these animals, but it was very much at odds with our purposes. We intended to weigh fawns before and after suckling to find out how much milk they had drunk. Excrement loss would confound the calculations completely. Dr. Williamson and the others had known of this problem beforehand, and the diapering
technique had been reported in the literature. It was just one of those things they would have to sort out when the time came.

It turned out to be quite a problem, and they soon began to discuss alternatives. On June 10 they had tried simulating the mothers' tongues using warm wet cloths, but this had not worked. They wondered if perhaps the cloth had not been rough enough to be like a mother's tongue, but abandoned this approach in favour of making the diapers work. We discovered the next year that a cloth will work, if the fawn is being bottle fed at the same time, but that would have been of no assistance anyway. While this was being discussed, Dr. Williamson mentioned a paper he had seen reporting a study of moose, in which the milk consumption of calves had been determined to within 7 grams per suckle, but there was little indication in this paper of how the result had been achieved. Williamson turned to me in mock annoyance and said, "They never tell you how they did it!" Not only did they not learn anything about how to do it themselves, they didn't know whether or not to believe the result.

On June 14, as I said, Don and Kirstin were having trouble. The diapers would not stay on, or the fawns would fall down, or they would resist being diapered. One memory that sticks with me clearly is that of Don, a naked fawn in his arms, right leg kicking high, and a neatly pinned diaper containing an invisible

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deer sailing through the air. There were numerous times that
summer when I had occasion to reflect upon the tortuous path
followed by the Seeker of Truth, while putting diapers on baby
deer.

Sometimes it takes guts

In mid-June I learned that David and Kirstin were planning
to cooperate on a study of the post-natal changes of fawn's
digestive tracts. She did a morphometric examination of the gut,
studying the length, volume and shape of tracts removed from
fawns of various ages. Using the same animals he followed the
development of digestive function with a technique called in
vitro digestion. This is simulated, test tube digestion. In this
technique a food sample is incubated with rumen flora taken from
a deer, and after the bacteria and protozoa have partially
digested it, it is run through a simulation of the rest of the
digestive process, using enzymes, and other chemicals from
scientific supply firms. Samples of the food, and of the solid
waste from the simulated digestion are combusted in a bomb
calorimeter and broken down in a Hydrogen-Carbon-Nitrogen
analyser. These give the caloric value and protein content,
respectively, before and after simulated digestion, from which
the efficiency of digestion can be calculated. The in vitro
study of the development of the digestive tract became a
significant part of David's work that summer, and of his study
In the fall, after following the development of the digestive function of the fawns, he used the technique to assess the nutritive value of the wild foods available to deer in the fall and similarly, to test the experimental feeds prepared for him by Buckerfields. It was also incorporated into the data from the summer's work, in that the solid food consumption was converted into digestible calories and protein consumption before the data were analysed. Kirstin's morphological development study became a senior undergraduate research project, for which she received course credit, and which she published.

This amounted to a basic reformulation of the experimental approach to diet. Until David thought of incorporating in vitro digestion into his research, Dr. Williamson worked in terms of input/output analyses, as we saw in Chapter Four. He did not actually delve into digestive function in his research, although it was a subject of discussion. It appears that Kirstin's looking at the morphological development of the gut was the stimulus to David to incorporate this well-known technique into his research plans. Her work grew out of Dr. Williamson's suggestion during the meeting on May 26. After the meeting she and Don consulted Moen's book, and from there, she says, she followed citations to find the methods used to measure the digestive tracts of animals. At some point she teamed up with David, who decided to study the functional development of the gut. He says that he had known of in vitro digestion long before he came to work with Dr. Williamson, but he did not raise the
issue during the discussion of rumen development at the meeting, which at no point addressed the possibility of studying digestive function. Dr. Williamson said in July that David had come to him a week or two after the meeting, very excited, and had proposed the use of the technique. This had impressed Williamson a great deal, because he had been thinking only of the morphological development, not the functional development, when he had made the suggestion to Kirstin.

The reason for this is not hard to find. David had to kill fawns at regular intervals to get the samples of rumen flora. This is a different matter from using animals which had died for other reasons, and Dr. Williamson, I suspect, was accustomed to shying away from such procedures because of Elaine's and Karl's opposition to them. This constraint had removed such topics as digestive function from Dr. Williamson's research agenda. But with Karl no longer involved with the research, Dr. Williamson was for once willing to brook Elaine's opposition to get these data that he very much wanted to have, once the suggestion was made to him.

The digestion work also involved me, for my nutrition project meant that several does and their fawns were "mine", and if David wanted to use them my consent was at least ceremonially required, for diplomatic reasons. In fact, we were on quite friendly terms and he would have asked me as a matter of course. I had no real power over the fawns' fates, Dr. Williamson would have asked me if I had refused David, and I could not have
refused him. The subject of killing these animals was an uneasy one for a good liberal such as myself, but as I sat there, eating my ham sandwich, I hardly felt justified in opposing the deaths of animals for human purposes.

Formalizing the routines

On June 22, Dr. Williamson and Don reviewed our situation. Dr. Williamson dominated this conversation, and David and I passed in and out of it as we went about our tasks. The situation they summed up was as follows. Soon there would be enough fawns to make it possible for Dr. Williamson and Don and David to work out the final assignments of specific animals to specific projects. This would probably happen early next week, and be followed immediately by the first round of suckling observations and milk consumption measurements. They still had not decided whether or not Don would have both males and females in his study. The same arguments detailed in the last chapter were brought forth again. Don argued that males are known to have a faster rate of development than females, and accordingly, he wanted to have males and females represented equally. Dr. Williamson was inclined to extrapolate from males to females, getting results for females of a given age by looking at the results for somewhat younger males. David felt that the restrictions placed on the fawns' access to their mothers, the possible stresses resulting from the manipulations of diapering and weighing, might have some unspecifiable effect on the fawns'
growth rate and development. If Don had females in his sample, then some of David's experimental animals would have to be included because there was a limited number of available deer. He had enough potential unknowable sources of error without adding any more, and so preferred that his deer not be used in Don's study. Dr. Williamson found himself mediating between the two of them, and not for the first or the last time. The two projects crossed paths repeatedly, and as final authority in almost all matters, he made the decisions that kept the two projects from interfering with one another.

A shift schedule had been organized for the observations of suckling behavior, but this had not been done for the milk consumption work. The immediate practical question was how long can the fawns comfortably go without suckling when they are very young? In the wild they have to spend fairly long periods of time separated from their mothers, when she is feeding or running away from predators. The agricultural literature indicates that the young of domestic sheep show the same growth rates whether they suckle once an hour or once every four. This was taken to mean that we probably have considerable leeway. Dr. Williamson asked Don if his pilot study of last summer had shown any time of day when suckling peaked, and Don said there was no

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consistent diurnal pattern. This meant that the shifts could start at any convenient time of day. No decision was reached at this time on how long to make the shifts, however. Other topics were raised. It came out, for instance, that I would be recording the food consumption of Don's five animals and their offspring, plus two controls, and possibly some more. Dr. Williamson's deprivation work was to be done on male fawns only, using one fawn of a male-male pair. The other fawn of the pair would be left to suckle to keep the milk flowing.

Most of these issues had been discussed before, but they now had more substance because of the intervening events. This recapitulation appeared to pull recent developments together in Don's and Dr. Williamson's minds, to rationalize their present position with respect to their previous plans for the future. It was made more formal on June 27, Monday of the following week, at a special meeting in the laboratory on campus, in which the deer were assigned. Most of the does had by now given birth, and the details of the discussion were largely a matter of which animals were to be placed under which specific constraints. The familiar issues arose. Don wanted half males and half females, and he wanted to allow them to suckle until they were weaned by their mothers. Two papers, he mentioned, show clearly that does shot by hunters in October, November, and December, were still lactating.*

*Scanlon, Patrick F. and David F. Urbston (1974) "Persistence of lactation in white-tailed deer". Journal of Wildlife Management, v. 42, # 1, pp. 196-197. See also, Mohler, L. L., J. H. Wample, and E. Pichter (1951) "Mule deer in Nebraska National Forest".
FIGURE IV: Variation of quality of does' milk during lactation.
David, as we know, preferred that his females not be used, and he insisted that they be weaned in mid-September, so that they could go on his special diets. Dr. Williamson pulled out the graph of milk quality from the paper he and Karl had just published, and noted that the milk underwent a marked drop in quality after the twelfth week, with protein and fat concentration falling off rapidly from that time on. This graph is reproduced on the facing page.7 This meant that mid-September was a reasonable time to wean fawns born in mid-June. Dr. Williamson stated that all female fawns would be weaned then, and the males in Don's study could continue to suckle as they pleased.

The situation they were dealing with was not at all what they had expected. There were far more male fawns than females. The original working hypothesis was that there would be a 50-50 ratio of males to females, an assumption they had made in order to proceed with their plans. It had been recognized that there might be a shortage of females, hence Don's agitating for them in his own work. The upshot was that he got three male-male pairs, one female-female pair, and whatever was produced by a doe due to give birth soon. Five does with one or two females went to David. Some of the does were still carrying their young, and any more females that were born would go to him as well. I

was to work with Don's animals, two controls that did not have fawns, and two or three other does with male fawns.

During the meeting I worked out a tentative scheme for placing the fawns, so that each project's fawns would be grouped in neighbouring pens, in three contiguous arrays. I showed it to David, who followed it with some modifications that night when he went to the Forest to check the health of a doe whom he suspected was having birthing problems. For the most part, we were ready to roll. Our use of time and space was more or less formalized at this point, and several days later Don distributed to each of us a complete specification of our weekly routine, which was an enlarged and more detailed version of the earlier handout, and assigned people to their shifts. A xerox copy of this handout is found in Appendix II. A few details needed to be cleaned up, but we were ready to put our newly formed routines to work.
II. Formalizing action and talk, 2: reconstruction and explanation

July 6: Murphy strikes

Our routines were not as reliable as we had hoped they would be. On July 6, we knew from the analysis of the first week's data that the milk consumption results were uneven. The reasons for this were not clear. True, the scale was occasionally indisposed, but we knew when this was so, by using the test mass. Diapering was a problem, but we had cut the diapers in half, and soon they became quite saturated with the smell and presence of the fawns so that they were not so alien to the mothers. Most of the mothers seemed now to accept the diapered fawns with relatively good grace, and allowed them to suckle. One would not accept them, and was removed from the study. Even with all of the apparent difficulties corrected or taken account of, there were disconcerting times when, as David put it, the mothers were being injected with milk by their fawns. Very small losses or gains of weight of fawns were recorded sometimes, when everything had apparently gone well. The fawns suckled, the diapers seemed to deter the mothers, the scale worked, (or so we thought), and nothing happened. Since the scale did go wrong from time to time, and the diapers did
not stop the mothers from time to time, there was considerable confusion anyway, and it was difficult to have confidence in our understanding of what was happening in any given case. This produced a pervasive atmosphere of tension and confusion, affecting Don most strongly because this was his project, but bothering the rest of us as well.

We felt we did not know how much milk the fawns were getting on a number of occasions, and we did not know which of the many possible causes was behind it, which made us wonder about the dependability of the results that did look good. The question at stake was, "How much milk do the fawns really drink?". The response to the presence of this question, the "answer", if you will, was a series of plans for possible future work. How could we modify our procedures this summer, or what could we do next summer, or what additional things could we do now? Don and Dr. Williamson, with help from David, would ransack their memories of what they had read, would go back to the library to look for supporting information. This is something I cannot deal with adequately here, because I did not use a tape recorder, and the conversations were much too complex and fast-paced to remember or write down while I was working.

This much was clear, that the answer to the question, "What is going on here", or, "What is this really?", was "What should we/can we do?" Act and fact were hard to separate, and it seemed that the information they drew upon was very much subject to interpretation in the light of the practical problem at hand.
Sometimes the plans were quite immediate. For instance, David thought the answer might be to restrict the fawns in some way that would prevent them from suckling, but let them be with the mother, so she could groom them and get the elimination of waste over with. Then they could be allowed to suckle. His first idea was to use muzzles, but the fawns did not accept them at all. He was not interested in constructing those brassieres for the does that he had been contemplating earlier in the year, and his attention soon shifted to a piece of agricultural equipment that he had been hoping for a chance to use because he felt its name belonged in the scientific literature: Kant Suk Weaners. Ever since discovering these paragons of agricultural technology in a catalogue, he had felt sure there should be place for them in his work. They are a simple device that fits on the muzzle of a calf, pricking the mother's udder when the calf tries to suckle, and causing her to shy away. They are used to wean the calves of dairy cattle. Fawns do not have sufficiently robust noses to hold these devices, however, and Kant Suk Weaners went the way of most ideas.

The future possibilities discussed on July 6 included raising some deer from birth and measuring both their milk and solid food intake and their excrement production. There simply was not time to do this in 1977, it would have to wait for next year, a feature of this work which Dr. Williamson found frustrating. In fact we did do this the next year. The idea was to get some information on deer raised under completely
controlled conditions to compare with the results from the work we were doing now. If the consumption figures and growth rates were similar we could have more confidence in the methods we were using. Another possibility was to make up a solid ration for the fawns that would include a known proportion of some indigestible material, such as straw, which would pass through the digestive tract unchanged. The amount of it in the faeces could be used to determine the proportion of the fawns' diet that came from solid food. Since the efficiency of digestion was being worked out by David in his in vitro work, and the absolute amount of solid food was being recorded, the amount of milk consumed could be calculated. This could then be used as a check on the present work. In fact, it turned out that none of the immediate plans would work for us, the feasible plans were all for next year, so we remained in a state of unresolved tension for some weeks.

This was a very problem-oriented reconstruction of the group's future, aimed at resolving an extremely pressing immediate question. We really did not know with any confidence how much milk the fawns were drinking. The response to this was to do something and to review the available information in the light of what had to be done. Their understanding of the group's future changed, and the specific material and behavioral characteristics of the immediate situation were open to question. One interesting point is that the specific substantive projections that Dr. Williamson made were closely parallel to
the general plans for the future of his research career that he had expressed earlier. His "answer" to this question was also his career plan. On June 8 when he and the others were trying to figure out how to weigh live deer accurately on this scale, Williamson extolled the virtues of the machine. It would permit them to do all kinds of research on nutrition, efficiency of digestion and assimilation, and growth rates, that had not been possible before simply because you could not weigh large live animals accurately enough. All of his projections were ways of backing this up in the specific case of milk consumption, ways of corroborating this approach. The substantive proposals he made in July were technical versions of his avowed professional ends in view.

Whether or not the group members could feel they knew how much milk had been drunk was connected to whether or not the peer public would accept their results. This was expressed on July 13 when Victor, the PhD candidate, paid a rare visit to the deer enclosures. At noon that day the scale had had one of its spells just as the measurements of milk consumption were getting underway, but this had not persisted. Later, Victor and Don stood outside the main enclosure, discussing the research in a subdued atmosphere. Don was glum; our difficulties with technique alarmed and depressed him. He could not understand Dr. Williamson's enthusiasm. Williamson felt that this was a new technique to them, and they had to expect a lot of trouble when they were developing it. Responding to Don's reports on the
data, Victor was dubious of the results, suggesting that Don could spoil his reputation at the outset by publishing such material. Don was not sure what to think, saying that the technique had been reported, but that we didn't know how well it had actually worked for other people. He was getting his first glimpse of how long it takes to make experiments work.

His feelings of concern were often shared by others, who wondered what could be made of these puzzling results. It was demoralizing not be able to tell what was causing the trouble on any given occasion. The mood of the day varied a great deal, though. Even though there was considerable anxiety about the research, there still were days when the scale worked well, the milk consumption figures looked real, and the project seemed to be coming together. Ironically, July 13 turned out to be such a day for most of us. There was much discussion of future research. For the most part these were about analyses of digestion, and they were much influenced by David's in vitro simulations, which were going quite well. David's plans for the fall were well in hand, and Dr. Williamson was busy thinking ahead to when he should begin his deprivation experiments. The mood seemed to have rebounded from the previous anxiety, largely, it seems, because of people's contemplation of the future and some sense of coherent purpose.

These acts of contemplation of the future were part of the daily round of events, and they were partitioned in several

\[1\] Arman et al, 1974.
ways, and on several time scales. As events they often were just jumbled together. Don and Dr. Williamson might be discussing projects for the next summer, while David and Kirstin played backgammon by the trailer; I weighed food boxes, and Elaine cleaned pens. Elaine's work was part of a regular daily routine that went on, with minor modifications, all year. This was her long-standing institutionalized role of animal caretaker, which had developed over the years. Weighing feed boxes was part of a complex weekly cycle, worked out over several months, and formalized in a five-page schedule handed out by Don late in June. Backgammon was just a way to pass some time. Don's and Dr. Williamson's plans were an attempt to locate our present situation on a yearly cycle of reproduction and experiments, in order to establish their meaning for us. In sum, they were saying, "So, there are problems with what we are doing, let's get what we can from it this summer, and next year we can do some work that fills some of the gaps."

Crisis: the night of the raw chicken

These various kinds of activities and levels of organization had their focal point in the coherence of the daily scene. Every now and then this would fall apart into confusion and discord and had to be put back together again afterwards, eventually being re-assembled in a different form. David named one such occasion "The Night of the Raw Chicken". This came
about when a barbecue we had planned to have at the Forest was
cancelled, partly because the research was disrupted by an
unfortunately timed breakdown of the scale. We had various
diversions to fill our spare time at the Forest, which we needed
because we spent more than forty eight continuous hours there,
doing first the suckling observations and then the milk
measurements. We slept overnight in a small travel trailer that
was parked outside the deer pens. It was a leftover from
Lucille’s field study on ground squirrels in Alberta. During an
impromptu midnight weiner roast over the propane space heater,
we had planned to have a more ambitious party, thinking at first
of roasting a whole young goat, and then settling for chicken
instead. The date was set for Wednesday, July the 20th, the
night of the milk measurements, and we assembled the requisite
chicken, corn, wine and other goodies for the occasion.

That afternoon, the scale began to work very erratically,
as it had done many times in the past, causing us to miss one
round of weighings. Since we never knew how long it would take
for the thing to start to "fly right" again, we simply waited,
in the hope that it would spontaneously correct itself. We
checked it from time to time with the test mass. Tension built
through the afternoon as the scale did not self-correct, for we
were in danger of losing the whole day’s data, and hence the
week’s work. At some point in the day, it would become useless
to continue, and 8:00 pm was agreed upon as the cutoff time. We
began to discuss the re-scheduling of the milk consumption
measurements. This was not easy, most of the time not specifically set aside for these observations and measurements was otherwise spoken for. Dr. Williamson was going out of town, so later in this week was out of the question, but he felt we could do it at the same time next week. We were about to switch from once a week measurements, to bi-weekly measurements, so there was an unaccustomed slot the following Wednesday. We could do it, he said, without the preceding twenty four hours of suckling observations.

Don objected strenuously to this. He felt that the behavioral observations were linked to the milk measurements, and therefore they should be duplicated as well. Williamson replied that these two kinds of data ran parallel but were separate. The milk consumption results for a given week were just another point on the graph. Presumably he meant that if one point on the milk consumption versus time graph were at a different week from that originally planned, this would make no difference to the shape of the curve, which he would be representing to himself and the world as a regression equation anyway. Don did not see things this way, possibly because his behavioral data were not so easily quantified, and extrapolating between weeks would not be so easy. He wanted to keep the two together. Dr. Williamson was firm. The disruption of time and effort involved in getting us all there to do both days work was great, and Don's point was dubious.
Eight o'clock came and went without improvement in the scale's temper. On top of that we were in the midst of a prolonged spell of dry weather. Fire regulations were extremely strict, because of the tremendous damage a forest fire could do to the forestry experiments, some of which last thirty years or more. We could not have a fire at the research site. Dr. Williamson made quite strenuous efforts to find us a place on one of the Research Forest's official outdoor cooking sites, complete with masonry barbecue stand, picnic table and the like, but could not find any of the officials who could give us permission to use the place, since it was now well after office hours. In haste, we divided the spoils and went our separate ways, feeling more confusion than a fairly straightforward equipment breakdown should have called for. Perhaps the incident was a focal point for a growing sense of discord, stemming from the problems with the research.

The next week when we tried again, the scale worked perfectly and the milk consumption data were the most reliable-looking we had yet gotten. The improvement in results was a happy trend, but it did not lack for counterpoints. The same day we completed the rerun, 3 deer died. One had been in poor health for a long time with an infected foot that got progressively worse despite repeated injections of antibiotics. In a final attempt to cure her, David released her from the small asphalt pen we had kept her in for treatment into the large grassy enclosure. He hoped that the softer surface and
freer circumstances would make it easier for natural healing processes to take their course. He reckoned without the harassment she received from the other does. For, as a newly introduced animal to the herd she fit in at the bottom of the female dominance hierarchy, and the other does chased her constantly. Her foot got worse and worse, and finally fell off. When David saw this, he brought his shotgun to work and killed her, commenting angrily at the time that he had had the compassion to do the thing which the "nice" people had shrunk away from. Then an apparently extremely healthy buck simply keeled over and died. A vet was summoned immediately to do a post-mortem, and we all crowded around to watch, gagging on the smell of the opened abdominal cavity. He found one badly degenerated kidney, and a softened and discoloured liver, from which he took tissue samples for histological examination. Nothing conclusive turned up, though. Then one of the control does, one of "my" animals, broke her leg in the weighing enclosure. There was a long debate about the feasibility and expense of taking her to the vet to have her leg put in a cast, and it was decided to kill her because she was in rather poor condition and unlikely to survive this risky and expensive procedure. Dr. Williamson and Don dissected her reproductive system, and found that her uterus was full of pus and bone fragments. Her fetus had died without being expelled or properly resorbed. This was some small consolation, for it made her death seem less repugnant, but as you might expect these deaths cast a
pall over the proceedings.

David's domain

During this time, David kept doggedly doing and planning his own research, when he was not working on Don's and Dr. Williamson's. In the midst of the turmoil, his in vitro digestion study was going very well. Using rumen flora from three week and eight week old fawns, he did digestion on alfalfa hay, calf starter pellets, and the terminal shoots of blackberry bushes, the primary wild food of the deer on the coast. At this time he was following the development of the rumen, which had little to do with his ultimate aim of comparing the deers' autumn wild diet with the experimental diets he was planning to use. This developmental work followed from the discussion in the May meeting, and fit in with Williamson's and Don's work on lactation. It was also dependable, presentable data that David could use in his thesis if his more risky experimental work did not pan out. His replications were well clustered, and although he had a somewhat higher figure for digestibility of alfalfa than was reported in the literature,\(^2\) he could easily attribute this to a difference in method. He assumed that the author of the article had used the whole alfalfa plant (minus roots of course) for his study, because he had not said otherwise. Deer

eat only the leaves and terminal shoots of the plant, though, leaving the tough main stem behind, a piece of the local lore that was mentioned earlier. Accordingly, David had used only the parts that the deer actually eat, and had obtained slightly higher digestibility figures (75% as opposed to 68%). He was quite pleased with the place he had given himself in relation to the published work. It was close enough to corroborate his own, but clearly not as good.

On August the 3rd the scale caused serious problems again, and again did not self-correct in its mysterious way. This precipitated a minor turf war between Don and David because Dr. Williamson was away, paying a visit to the field site of a senior undergraduate working on grizzly bears. Don thought the special scale was too touchy, and therefore its use ought to be restricted to the milk consumption study, which cut out David's and my use of it. David and I should use the more primitive machines from past years. We had just spent a morning trying with the men from the scale company to figure it out, and David was having none of this exclusion. The question was left to Dr. Williamson to resolve, which he must have done by quashing Don's suggestion, for I heard nothing further about it. The next week the scale worked well again.

In early August David began to think more about how he would determine the moment his animals ovulated. He wanted to know whether or not they reached puberty, and while parturition was the final criterion, it was also interesting to know whether
Since estrus is a very short event, pinpointing its onset is the same thing as finding out whether or not it has occurred at all. His potential handle on the phenomenon was that females of many mammal species become quite restless at the moment of ovulation. A reliable way of measuring activity could be used to pinpoint the moment of estrus. Here we ran into the problem of money again. David spoke to me of two papers on estrus activity, one of which showed a fourfold increase in activity for mule deer3 (conspecifics of black-tails), and a twenty-eight fold increase for white-tails.4 This posed a typical conundrum, for the two authors had used different methods and there was no reason to believe that these methods would be equally sensitive to sharp peaks in activity. In addition, the size of the activity peak might well be a species-specific trait. David could not afford the cost of the methods used in either paper, and would have to use a third, which he had not yet devised. David and Don and Dr. Williamson were going over all of this one day in early August, considering light beams, traffic counters, telemetric activity sensors, and other things that they could not possibly afford. After some time spent on this, David reported to me, he had a flash—pedometers! Pedometers? Yes, and

3West, Nels (1968) *Length of the Oestrus Cycle in the Columbian Black-tailed Deer or Coast Deer*. BSc thesis, Department of Zoology, University of British Columbia.

he was quite pleased at the possibility that they might work. Not only that, but they cost only eleven dollars down at the outdoor equipment co-op.

Pedometers are devices hikers strap to their ankles, which record the number of paces (and other movements) the hikers feet make, on a watch type of dial. Using them in research was not straightforward. A baseline of activity had to be established for each animal. Then there had to be allowances made for activity spikes that happen when the deer are frightened by cougars or domestic dogs hanging around the pens. In addition, David did not know what sort of activity spike to expect, because the papers he read were so divergent. He also did not know how accurately pedometers would register very rapid jerky movements, and so did not know which sensitivity setting to use. If the black-tails could only be expected to show a four-fold increase in activity, then distinguishing estrus from fright would be difficult. If the white-tail paper were more representative of what goes on, clearly life would be simpler for David, but there was a cross-species comparison at stake here as well as a cross-method comparison. Two species can be very similar in some respects and very different in others.

Another difficulty David faced was the shortage of female fawns. There had been rumors that a nearby game farm had a female black-tail fawn, so on July 6 we juggled schedules very athletically indeed to send an expedition to capture her. It was almost too much a re-run of Murphy's Laws to be true. There were
four fawns, three known to be males, and one the manager was sure was a female. Assisted by a friendly elk, my friends spent the entire afternoon chasing the fawns, catching the presumed female last, of course. He was particularly elusive.

David had another lead. The Fish and Wildlife Branch regularly hears from people who have found "abandoned" fawns, and raised them. These usually are fawns whose mothers were scared off by the approach of the people who discovered the fawns. The fawns freeze, the mother flees and returns later to find her young ones gone. David had heard through a regional office of the Branch that an elderly couple had a pair of female fawns under their care, and was advised to visit them and ask if he could use the fawns. He went to see them and the heat in his voice when he returned suggested that he had not been prepared for the hostility they showed him. They bad-mouthed his research and biologists and universities in general, saying that they personally had done more for wildlife than all the biologists in the province put together. David didn't take kindly to being called a despoiler of wildlife. In his own mind, his commitment was clear. He had wanted for many years to be a wildlife biologist. His aim was to become an accredited biologist, rise to a position of influence, and have some say in policy decisions, as the scientists advising the Berger Commission were doing at that time. In this case the law was on his side, for

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all wildlife is the property of the provincial Crown, and people have it in their possession at the sufferance of the Fish and Wildlife Branch. In the end, David got his farms.

Recomposing the history and reorganizing the scene

At the same time as David was going through all of this the research was being re-organized, politically, conceptually and materially. Williamson told me on August 9, while we were on shift observing and recording suckling, that he had decided to drop the deprivation study. The prevailing opinion among the others was that between the technical difficulties with the milk consumption project and his administrative duties, he was not prepared to do the deprivation study. His involvement with the milk consumption work continued to grow, however, as we found out on August 16. That day he surprised David and me by calling us over to look at the graph he had just made of the milk consumption data. He was much happier with it, he said, than he had expected to be. He had plotted it up on semi-logarithmic paper and the curve showed a steady decline of grams of milk consumed per unit metabolic body weight. Some points were far below the line, but many were quite close to it. Metabolic body weight is the body weight taken to the power of .75, a figure that is about one half of the body weight. Numerous studies of basal metabolism of animals of many species have indicated that the basal metabolic needs of almost all animals vary according
to the .75 power of body weight. This makes a more meaningful point of comparison for food consumption than does whole body weight, and makes extra energy loads stand out more clearly.

Dr. Williamson had seen his data the way that someone reading the paper he might write would see it, plotted, and reduced to a curve. He had used the standard methods of public representation, which had been drilled into him during years of training in composing and reading graphs. Reading graphs takes experience, requires its own form of literacy. Looking at our transformed experience in this way, standing back from it, so to speak, he saw a far more stable and consistent pattern than we had anticipated. The same day, Dr. Williamson asked me if I had graphed up the food consumption data that had been building up in my notebooks. I had been waiting for the results from David's analyses of the digestibility of the feeds we had been using. Once I had these I would transform the raw consumption figures into values of digestible energy and digestible protein consumed per day, per kilogram metabolic body weight. Dr. Williamson was quite satisfied to wait for these results, because they were the sorts of transformed data he would want to use anyway. These would be the representations of the deer and the research situation he would use in his contemplations of the questions he had in mind. He was already moving into a formalized world of numerical data, abstract mathematical functions and idealized processes, jumping back and forth between it and the deer. I eventually gave him the figures in early September, but was more
occupied on the 16th of August, with enlarging the head holes of the fawns feed boxes, and wondering how to keep the does from wriggling their noses into them and snitching food.

Later on that day David and I discussed this new development in data analysis. At first it surprised us to see Dr. Williamson plotting up Don's data, but we soon concluded that it fit into the sort of managerial career pattern that Don was planning. He wanted his MSc so that he could be a field supervisor for an environmental consulting firm. Broadly speaking, David said, technical staff have bachelor's degrees or diplomas from technical institutes. This was the sort of work Don had been doing before returning to school, and wanted to move beyond. The next layer of people have masters degrees, and the data are given final form by more highly placed people yet, who often have doctorates. So far that summer we technicians, Don and Dr. Williamson were fitting into that pattern. David felt his approach was different and more autonomous, and I was inclined to agree. He was headed for an academic or civil service career, following his doctoral work. He fit into the pattern of apprentice researcher, aiming to have a lab and students of his own.

There were various material and procedural adjustments to be made, along with the intellectual and political reorganization. Dr. Williamson decided to lengthen the time between milk consumption measurements to six hours. The fawns were less and less dependent on milk, and cutting back would
save work. Don offered no resistance to this move, made while we were doing observations. He seemed to be left with the behavioral study now. More importantly, Dr. Williamson changed the weighing procedures. The sample button did not give the best value, he declared. Given the instantaneous, highly visible readout from the scale, it should be possible to watch for the fawn to stop moving and take the weight at that instant. He also felt that the same person should do all of the weighings, to ensure consistency, and he took responsibility for that himself.

Karl leaves. Michael arrives

While he was reorganizing the minutiae of his research activities out at the Forest, Dr. Williamson was also finishing off old business and starting up new at the University. Karl successfully defended his doctoral dissertation at this time. Dr. Williamson and David and Michael attended, of course, but the event had little impact out at the Forest; the conclusions of the thesis were already well known. I learned little of the event except that Karl had taken a rather challenging attitude toward the audience, and Dr. Williamson thought that the other people at the examination had not read the dissertation carefully enough. Karl was soon off to Iran, where he had a senior managerial position at one of the Shah's game parks. He stayed there until the recent unpleasantness, and now is a zoo director in Australia.
I heard at the time of the defense that Michael had been accepted into the graduate program as an MSC candidate. A serious outdoors enthusiast, he wanted to study the same herd of bighorn sheep Karl had started with. He had become interested in these animals while hiking and camping for a summer in the park where they live. He specifically wanted to do field research, being no aficionado of laboratories, test tubes, or flourescent lighting. Nevertheless, his field work was to be done against a standard established through work with captive bighorns, which are expensive and awkward to keep. Dr. Williamson commented at the time that this was where he really had to do his job, because now he had to find the money for Michael's research. There was some chance of using bighorns in captivity elsewhere, although one of these herds was about 250 miles away which meant much inconvenience and travel expense. There were some more animals at the University of British Columbia. Dr. Williamson was involved in a semi-formal network of bighorn researchers operating in the province, and would use these contacts to get what he needed if the Fish and Wildlife Branch did not give them the money he and Michael had applied for.

As usual, they did not. Michael was granted only four thousand of the eleven thousand dollars he had applied for. This covered only his field expenses: the balance was to have been for maintaining several bighorn females in captivity. He wanted to monitor the serum concentration of progesterone at different times of the year. This hormone is closely connected with
pregnancy, and his tests would tell him what concentrations are typical of pregnant bighorns. His field work would be to devise a way of sampling the blood of the free-living animals, so that he could tell how many of them were pregnant. Once this was known field observations at lambing time would establish how many of the ewes had carried their young to term. David reasoned that the Fish and Wildlife people knew of all the captive animals in the province, and did not want to pay the bill for yet more of them when Michael and Dr. Williamson could work out a deal with someone else to get the blood samples they needed.

David had been thinking again about the pedometers and the papers on estrus activity. The pedometers have a dial like a wrist watch, but with only one hand. There is no way of telling how often the hand has gone around, and so you really can't know how much activity there has been. So David had to find a sensitivity setting that would lead to a whole day's activity being represented by less than one circuit of the dial. Then perhaps, the spike would appear as a complete circuit. He had re-read the estrus activity papers, and felt more impressed than before with the paper on white-tails, which showed a twenty-eight-fold increase in activity at estrus. He was more inclined to believe that there might be a very large increase of activity at the time of ovulation.
On September 15 David and I walked out of the deer barn to find Dr. Williamson in a state of considerable excitement. He called us over to look at his latest work on the milk consumption data. He was really quite pleased, for he felt that he had hit upon a way to understand the sporadic null consumption results we had had earlier. These were the occasions on which the scale apparently worked well, the fawns apparently suckled, the mothers apparently were deterred by the diapers, and the fawns did not change significantly in weight. During the previous night's work, he had noticed that 17-8, one of the does in the study, was showing this pattern again. The scale worked perfectly, there were no diaper problems, for the fawns now eliminated waste completely independently of their mothers, and there were these very small positive and negative weight changes. He puzzled over this and then it clicked—she's dry, she's not producing any more milk! This fit with something else that had just happened. The fawns had been separated from their mother over the weekend because of a misunderstanding between Don and David. David had just weaned his fawns in preparation for the dietary experiment he had set up, by putting them in separate cages from their dams. Because of the cramped situation in the deer barn, he and Don had had to shuttle deer back and forth from pen to pen in order to get the arrangement they wanted. During their attempts to sort out which animals should
go into which pens, 17-B and her fawns, which were not to be removed from the milk consumption study, had mistakenly been placed in separate pens and left in them over the weekend. The fawns had subsisted perfectly happily on solid food, and the mother had dried up.

As soon as he saw this Dr. Williamson went back through the suckling behavior records and compared them with the milk consumption results. He found that often the null consumption results were accompanied by records of suckling which showed many short tries of one or two seconds, up to ten to fifteen seconds. The same held that day for 17-B, who clearly was dry. It might be then, that some of the null results came when the mothers were dry or were not letting their milk down. Apparently, the technique was not as problematic as we had feared, and much of the anxiety we had felt had been unnecessary.

Dr. Williamson had reached this conclusion by the time I came to do my shift with him. Having reconstructed and rationalized the past, he proceeded to try to see the present differently. As the fawns suckled that shift he looked for indications that the mothers might be dry. When one pair made loud slurping noises he deduced that they were drawing air because the mother was not providing milk. An immediate check on this was available when we weighed the animals, but they had gained a respectable amount of weight. Another pair pulled long and hard, and he judged them to be getting a good drink. He was
right, in fact all the animals remaining in the study (17-B had been dropped) produced milk that day.

Conclusions

We have here a modest example of the complex relationships among the formal order of schedules and routines, the daily behavior of people and deer, and the formal discourse of data tables and graphs. We have the formal order of physical location, the placement of fawns and scheduling of research, and the order that was beginning to characterize our conversations as they became more informed by numerical and mathematical records. We also have a misunderstanding that developed between David and Don when they were moving deer from pen to pen, which resulted in one doe drying up and behaving unexpectedly. A mistake in the reorganization of the physical order ultimately lead Dr. Williamson to re-assess the data, to compare suckling records with milk consumption records. These were collections of numbers, already one step removed from highly routinized experience that was in turn removed from the intensive efforts to create the routines. This reassessment of data lead to a re-interpretation of a graph that was another two steps removed from the raw data, for it was a graph of consumption/day/metabolic body weight. Two kinds of data had been combined in a calculation to produce these numbers which were then graphed. From here Williamson went on to have a
different look at the deer and the experimental routines of the milk consumption study with an eye to manipulating them if it seemed appropriate to do so. Such changes would remove the present affairs of the project from their previously formalized physical and social order, so that both the situation itself and our understanding of it would be different from what we had been doing only the day before. In this way, a bit at a time, the formal knowledge comes to be quite set apart from the work and the conditions out of which the knowledge is produced. The separation of knowledge from everyday activity is very real, a normal feature of the way we make sense of events. The events of the moment pass. They are ephemeral, in flux. The scientists try to make their activities routine, but it is in creating formal statements that they are most successful in achieving stability.

Dr. Williamson was working in a complex multi-faceted situation, surrounded by many different kinds of events and things, about which many different kinds of stories could be told, concerning funding, personality clashes, or the medical care of the deer, to name only three possible topics. From the great array of information available to him, he selected and put together the numerical traces I mentioned, to show himself, and us, what had really been going on this summer, so far as his professional audience is concerned, to find out what the deer had really been doing. Most of the other topics were not considered relevant. This was by far the most important history he could have composed, the one that he hoped his peers would
want to hear about, the one that was produced for public
consumption. He was already composing it in the form suitable
for publication, representing our work to himself and to us the
way he would in print. In fact, the graph he showed to David and
I on August 15 appeared in a paper he published in 1980,
augmented with another year's data and changed slightly in form.

In 1977, though, he was not entirely confident that he had
found out what was going on. There were still these annoying
null consumption events and while he had an explanation for
them, he was not entirely sure of it and wanted some more
evidence. With this, he might have more confidence that the
curve he had drawn was truly representative of what the deer are
like. He wanted results that could stand on their own and not be
dragged down to earth by carping questions about questionable
methods and imaginative analysis. In the next chapter we shall
see how this work was concluded and then discuss one of the
papers Williamson wrote about it.
I. The outcome of the deer project

The consequences of undertaking a research project are as varied as the processes that constitute the research itself. The most visible and in many ways most important product is the formal written account, but there are other outcomes. Students are launched on their careers, research facilities are built up, political connections and friendships are established and reputations are forged—which brings us back to the published paper, of course. Since I have gone to great lengths to introduce this arcane world and its inhabitants, it seems only fitting to indicate how the research ended and where Dr. Williamson and the others moved on to. Once I have done this, I shall examine one of the papers about the milk consumption study that Dr. Williamson published.

We have already seen that in writing papers scientists create Science, that is, their research is transformed. Therefore, I shall not consider this issue again, but show how the paper might be received by a member of Dr. Williamson's intended audience, how it might be read by a professional biologist, and incorporated by that person into his own research practices. In this way I shall be completing a cycle of sorts, having moved from Williamson's use of published work to the use by other people of his product. My intention here is to focus on the network of influence into which Williamson hoped to
introduce his work, and the kind of significance it might acquire there, rather than treating it as a document unto itself.

**Finishing Dr. Williamson's Research**

On November 17, 1977 I met with Dr. Williamson in his office to discuss the work we had done during the summer. He was working on the data and his desk was covered with tables and graphs and typewritten manuscript pages. He showed me the graphs he had made of consumption of calf starter pellets, portrayed in the terms we had been using earlier: Kcal digestible energy and grams digestible protein/Kg metabolic body weight/day. The information on the alfalfa pellets would be plotted up soon, he said. He was preparing to examine the changing composition of the fawns' diets during the summer, to chart the declining importance of milk relative to solid food. We had now moved away from the deer completely and were dealing only in a world of facts and figures. Contemplating publication Dr. Williamson said that he was thinking about the order in which the results should be published. Probably milk consumption should come first, and then fawns weights and ages. These papers would combine data from his, Don's and my work, none of which would stand on its own.

Then we thought about how we might approach the deer again, reconsidering the nutritional independence study that Dr.
Williamson had had to give up. The data now in hand indicated
that mother-raised fawns become nutritionally independent
somewhere between 48 and 85 days of age. The picture of the
changing composition of the diet along with the information from
Kirstin and David now made it possible to devise an experiment
in which an hypothesis would be at stake, that would test for
nutritional independence at ages specified in advance, rather
than simply trying it out from time to time during lactation.
Williamson was of course aware that long periods of documentary
work must be done before one knows the species one is working
with well enough and has a good enough grasp of the emerging
questions, that one can formulate testable hypotheses, but he
strongly preferred to cast his work in that mould, as we have
already seen.

In this case, I do not know what real purpose such an
approach could achieve so far as ensuring the technical validity
of the work is concerned. For the sake of argument, let us
consider an experiment on nutritional independence formulated as
the test of an hypothesis. Suppose we are working with four
groups of fawns. Three of the groups are separated from their
mothers at 45, 65, and 85 days respectively, and a fourth remain
with their mothers throughout this period as a control. All of
these animals have unlimited access to solid feed and are
weighed three times per day. The experiment is designed to
refute the null hypothesis that fawns do not achieve nutritional
independence between the ages of 45 and 85 days. This is in
FIGURE V: Growth of fawns separated from their mothers at various ages. (2)

Group One:

- Fawns separated from mothers
- Control
- Experimental: discontinued

Group Two:

- Fawns separated from mothers
- Control
- Experimental: discontinued

Group Three:

- Control + experimental
- Fawns separated from mothers

Age (days) 258a
accordance with Karl Popper's dictum that it is the formulation of refutable hypotheses that sets science apart from other pursuits. Based on the work of 1977 the weight curves for each group would look roughly like the ones in Figure V, on the facing page.

The youngest fawns, Group I, lose weight without milk, the second group take a few days to adapt and then grow slightly more slowly than the controls, and the third experimental group are virtually unaffected by the removal of their mothers so far as their rate of weight gain is concerned. This does not mean that the mothers would not contribute to the survival of their young in other ways, through greater experience with evading predators, for instance. But it is clear that the members of the oldest group would be far more likely to survive on their own than would those of the youngest group.

It strikes me that it makes no real difference to conduct this experiment as an attempt to refute a null hypothesis, instead of treating it as a straightforward documentary exercise guided by previous experience. The two approaches yield exactly the same information: black-tailed fawns achieve nutritional independence from their mothers between 45 and 95 days of age. Clearly it is necessary to set the work up so that one can specify this range of ages as narrowly as possible, and that takes experience and planning. The experiment is not just an undirected gathering of data. But the outcome and its validity are the same, whether or not it is dressed up in Popperian
logic. One could argue that there is an hypothesis implicit in
the documentary approach, and that the importance of organizing
work in this way should be recognized. This is true, but I do
not think that it is the only or even the main reason many
scientists have taken up Popperian methodology. In fact, it has
been established by historians and conceded by some philosophers
that there is no one specification of how scientific inquiry
should be done. Any attempt to provide one can be countered with
examples from history, and none of the schemes that have been
proposed are logically binding. I think that the interest in
Popper's philosophy has more to do with the desire of people
working in embattled fields such as ecology, to achieve
respectability, and to define themselves as scientists.

As it happens, this project was not followed up. When I
worked for Dr. Williamson again the next summer we repeated the
measurements of milk consumption. He said that there had been
too much scatter in the previous year's data and he hoped to
gain a better understanding of the null consumption events. In
most respects, this summer's work was much easier than that of
the year before. Only one project was involved, and it was
conducted on campus at the newly-completed Animal Care Facility,
which eliminated problems of organization and travelling.
Diapering the fawns caused more disruption than the year before,
though, so Dr. Williamson changed the methods we used. The fawns

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1MacIntyre, Alisdair (1977) "Epistemological crises, dramatic
narrative and the philosophy of science". The Monist, v. 60, #4,
pp. 453-472.
were not diapered. They were weighed before and after suckling
and the net weight gain (milk consumed minus urine and faeces
lost) was multiplied by a correction factor. To get this, we
followed up on an idea that had been suggested the previous
summer. An orphaned fawn was hand-raised, and its milk, water
and solid food consumption, and urine and faeces production were
recorded. Williamson worked out a formula that would give the
correction factor he needed and used the data we obtained from
the orphan to calculate it. When corrected the milk consumption
figures from 1978 matched the figures from 1977 closely enough
that they could be analysed together. The editors and reviewers
of the journal to which he submitted his paper on milk yield
demanded that he explain the two methods and the correction
procedure at length, but the paper was accepted for publication
without undue delay.

During this summer Dr. Williamson began writing papers on
the deer research. He had published very little on it and
Michael told me one evening while we were working with the deer
that the people he knew through his sheep work were asking him,
"When is Williamson going to publish that stuff of his, anyway?"
Consequently, a large part of my work at this time consisted of
data analysis. I was given sheafs of data from previous years:
tables of weights of mother-raised fawns, the weights and milk
consumption of hand-raised fawns. I converted these last figures
to the forms we were using, digestible Kcal and grams of
protein/ Kg.MBW/day, to compare these figures with our
experimental ones. I plotted the weights of mother-raised twin fawns against age, and did the same for singletons. This graph appeared in a paper which was submitted the next year and published in 1980. While I was working in this world of numbers, rooting around in the stores of old data and plotting up the new, Dr. Williamson was reading papers, doing computer analyses and writing. He assumed a very abstracted air. Occasionally, I would see him walking in the halls of the Animal Care Facility with a paper in his hand, deeply absorbed in reading, punctuating the silence with suppressed cries of "fascinating!"

In July of 1978, the Research Director of Fish and Wildlife Branch visited the lab, for the Branch was partially funding the research. During the visit Dr. Williamson suggested that this kind of milk consumption work could be done on bighorn sheep, and would tie in with the field studies on that species better than work on deer could. The Director, however, was more interested in mountain goats and steered Dr. Williamson in that direction. In 1979 he did a little more work with the deer, but after that he turned the deer herd and the pens over to the administration of the Research Forest. Elaine continued to take care of the animals and other professors began doing research on them. In 1980 Williamson lead a short expedition into the Olympic National park in Washington state, USA, in collaboration with the Seattle Zoo. I went along as an assistant.

A number of graduate students from the University of Washington were doing field research there on the goats and
their habitat. Their field coordinator was a senior doctoral candidate who informed me of the political background of this work. The goats were being intensively studied and we were allowed to capture some of them, because the park administrators wanted to remove all of the goats from the park eventually. Although this area is in some respects prime mountain goat habitat, they are not native to it, but were introduced by hunters during the 1930's. The Olympic peninsula is in effect an island so far as goats are concerned. Cut off on three sides by water and to the south by a broad lowland valley, it was not colonized by goats after the last major glacial retreat. After the park was created the goats spread rapidly, having no competitors, few predators, and no hunting pressure. The plants of the alpine meadows are not adapted to their grazing, and serious erosion problems have resulted. The park administrators wish to return the park to its original condition, and would like to reintroduce wolves and get rid of the goats. Such moves are politically touchy, and they are having the goats and their impact studied in order to help them make their case. The graduate students and their supervisors are, of course, very pleased to have this opportunity.

Capturing the goats was quite simple, and illustrates why some wildlife biologists take a kind of inverse pride in calling their field "cowboy biology". The first step of this expedition

Fred Bunnell, Faculty of Forestry, University of British Columbia. Personal communication.
was to hike up to the tree line and establish our camp. Being in
the bush is an indispensable part of the experience for many
wildlifers—David and Michael are very much like this. When we
arrived we were all instructed to urinate on the same small
patch of ground. The goats crave salts and gather around urine
soaked dirt, to eat it. A noose was made in a 30 foot manila
rope and placed next to the urine soaked spot. Someone held the
other end, waiting for a goat to step in the noose. When one did
the holder yanked on the rope and then was dragged across the
ground until 3 or 4 other people jumped on the goat and tied it
up and blindfolded it. Once blindfolded it became very quiet.
There were several spectacular struggles with assorted
biologists rolling around in the dirt hanging onto legs, horns
or patches of wool. While doing my bit I landed in a remarkably
cold creek with an armful of displeased goat.

Unfortunately, the goats did not survive in captivity. The
animals in this area are very susceptible to capture stress
because the soils, and hence the vegetation contain very little
selenium, an important trace nutrient. The goats were given
selenium injections to counter this, but they died shortly after
being taken to the Seattle Zoo to recover. Dr. Williamson also
considered working on snowshoe hares but, again, had difficulty
keeping them alive in captivity.

Such reverses are not unusual, but Williamson was losing
interest in being a university professor. He was in his
mid-forties, had been teaching and doing research for some 20
years, and wanted to move into administration. There was some
limited opportunity for his ambitions at Western. For instance,
he had led the campaign to have the Animal Care Facility built,
and was its first director. But this was a relatively minor
responsibility, and his bid to become department chairman had
failed. During 1980 he searched very actively for a senior
administrative position elsewhere and was working hard to get
out some more publications to help with this. In early 1981 he
was offered the position of director of ecological research for
the federal government of another country, which he accepted.
His position at Western was held for him for one year in case he
wished to return, but by all accounts he is quite happy with his
new position, and has resigned his professorship.

Reading the research report

In 1978 and 1979 Dr. Williamson successfully submitted four
papers on deer to refereed journals. Two of them drew heavily on
work that had been done before 1977, and carried Karl's name as
co-author. The others were in Dr. Williamson's name only, they
were his property, and dealt exclusively with the research on
milk and solid food consumption that I have just recounted. The
longer and more comprehensive of these is called "Energy and
protein consumption in relation to growth of suckling
black-tailed fawns. It was published in a prominent Canadian biological journal. In this section I shall try to indicate what often becomes of a scientist's work once it is made public, how it is received and used, by presenting this paper through the eyes of a hypothetical professional biologist, a member of Dr. Williamson's intended audience. This analysis of how the paper might be read is based on the reading habits of Dr. Williamson and his students, which I mentioned in Chapter One. Briefly, the scientists I worked with do not first read a paper from front to back. If they do that at all, it is usually after they have decided to take the paper seriously. In most cases this means that first they survey the results section and the discussion and perhaps the materials and methods. Only if they have been persuaded by the data and analysis that the paper is worth their time do they follow the form in which it is written. Therefore, I shall not be analysing the paper as it is written, but as it is read.

First allow me to introduce our reader, an ecologist who teaches and does research in a university biology department, so that we will know the context in which this paper is read. His name is Dr. Curtis. His reading habits, interests, and working situation, which I shall describe briefly, are a composite based

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Williamson, Roger (1980) "Energy and protein intake in relation to growth of suckling black-tailed deer fawns". *Canadian Journal of Zoology*. (Author's name altered, and details of publication deleted for reasons of confidentiality.) Graphs from this paper are reproduced below. I gratefully acknowledge the permission of Roger Williamson to use this material.
on biologists I have worked with, both inside and outside of Dr. Williamson's group. I have also taken into consideration the written comments of the referees who reviewed Williamson's paper for the journal in which it was published. As I said, Dr. Curtis is an ecologist. To narrow this down, he specializes in terrestrial ecosystems, rather than marine or freshwater environments. Further, he deals mainly with mammals, rather than birds, or reptiles or insects, and concentrates heavily on ungulates, rather than rodents, carnivores, bats or other orders of mammals. Such is the nature of specialization in science. He is interested in such topics as nutrition, reproductive strategies, foraging strategies, reproductive physiology and socio-biology. Curtis is of course actively engaged in his own research.

He supervises several graduate students, most of whom work on free-living game animals such as moose, sheep and bear, and co-supervises several others who work with captive animals that are maintained by another professor. One of these students is working on the efficiency of forage utilization by captive moose. Consequently, Curtis is interested in both field and captive studies and what they mean for each other. His own current project is a field study of the foraging strategies of caribou (Rangifer tarandus), their nutritional requirements and how they meet them. Williamson's paper is of possible interest to him or his student, but its relevance will not be immediately obvious. That will depend on exactly what Williamson has to say about
energy and protein intake with respect to growth of suckling deer. The most likely point of contact between Williamson's and Curtis' interests has to do with conditions that affect the survival of very young ungulates, what they need in order to overcome the very high mortality rates of early life. In general, Curtis will be looking for information that will help him with his own work: supporting or contrasting data, techniques, references to other studies he had not heard of, or a whole new research option he had not known existed.

We find him sitting in the serials section of a university library going through the latest issues of his favourite journals and scanning a 'current contents' journal as well. He has a lecture to give later in the day and another to prepare, a meeting with some departmental committee, a paper and a grant proposal to write, and he has to pick up the kids at school today because his wife had to go out of town on business. That is to say, he has a six inch stack of periodicals and about 60 minutes at his disposal, because today he has more than the usual amount of unscheduled time.

As he scans the index of an ecology journal, his attention is drawn to Dr. Williamson's name, or perhaps to a word in the title of the paper, such as "energy", "growth", or "black-tailed deer". This bears on his interests so he decides to investigate the article. We must keep in mind that there are many places along the way at which Dr. Curtis might stop reading this paper and move on to another. He might pass the reference on to his
FIGURE VI: Tables and figures from the results section of Williamson's paper.

a:

Milk consumption of fawns at different ages. Vertical lines indicate ±1 SD from mean (solid circle) and sample sizes (number of determinations in each age range) are given beside each line.

b:

Change in proportion of energy consumed in milk compared with total energy consumption, as fawns age. Each line represents a fawn (N = 7). 1978 data.
student, whose work is closer to Williamson's than his own is, or keep a reference himself and come back to it later. He could make such decisions after reading the abstract, or the results, or more. He might also dismiss the paper and move on. It is one of many he will see, and, while of interest to some people, its implications are limited. Turning to the front page, he notes the university at which Dr. Williamson works, and tries to recall anything he might have heard about its reputation in mammal ecology. But unless he knows of Williamson's work he will have heard of very little. He glances at the abstract, reading the first line: "Milk and solid food consumption of energy and protein were measured in suckling Odocoileus hemionus columbianus fawns to three months of age." Then he moves directly to the results section looking for the tables and graphs that will indicate very quickly what this paper is about and enable him to decide whether or not it is worth a closer examination. I shall present these in the order of their appearance, along with a small note to indicate how they might be read. The numbers presented here correspond to the numbers of the figures and tables on the facing pages.

a. This graph indicates the amount of milk the fawns drank at various ages between birth and three months. The milk consumption is expressed in gm/Kg.MBW/day. It shows how, as the fawns grow, the amount of milk they drink in relation to their body size declines. This decline begins at birth and is fairly rapid to about 55-60 days, with consumption
FIGURE VI: Cont.

Nutritive values of dietary items

<table>
<thead>
<tr>
<th></th>
<th>Energy, kcal DE/g(^a)</th>
<th>Protein, g digestible/g</th>
</tr>
</thead>
<tbody>
<tr>
<td>Doe's milk(^b)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0–35 days</td>
<td>1.66</td>
<td>0.069</td>
</tr>
<tr>
<td>36–70 days</td>
<td>1.86</td>
<td>0.080</td>
</tr>
<tr>
<td>71–120 days</td>
<td>1.95</td>
<td>0.091</td>
</tr>
<tr>
<td>Calf starter pellets(^c)</td>
<td>3.56</td>
<td>0.156</td>
</tr>
<tr>
<td>Alfalfa pellets(^d)</td>
<td>2.37</td>
<td>0.109</td>
</tr>
</tbody>
</table>

\(^a\) DE, digestive energy.
\(^b\) Estimated on a wet weight basis (see text).
\(^c\) Dry weight basis.

Mean daily food (milk plus solid diet) consumption, relative to body weight, of digestible energy and protein by suckling fawns (1978 data)

<table>
<thead>
<tr>
<th>Fawn(^e)</th>
<th>No. determinations</th>
<th>Range in ages, days</th>
<th>Energy consumption kcal kg(^{0.74}) BW (^{-1}) (\text{day}^{-1})</th>
<th>Protein consumption g kg(^{0.74}) BW (^{-1}) (\text{day}^{-1})</th>
</tr>
</thead>
<tbody>
<tr>
<td>0DC</td>
<td>9</td>
<td>13–104</td>
<td>251 (\pm 12)</td>
<td>11.0 (\pm 0.6)</td>
</tr>
<tr>
<td>0DD</td>
<td>8</td>
<td>13–104</td>
<td>262 (\pm 13)</td>
<td>11.4 (\pm 0.5)</td>
</tr>
<tr>
<td>17BC</td>
<td>11</td>
<td>7–105</td>
<td>232 (\pm 14)</td>
<td>9.7 (\pm 0.6)</td>
</tr>
<tr>
<td>17BD</td>
<td>10</td>
<td>7–105</td>
<td>238 (\pm 18)</td>
<td>10.4 (\pm 0.7)</td>
</tr>
<tr>
<td>30G</td>
<td>10</td>
<td>9–107</td>
<td>229 (\pm 13)</td>
<td>10.2 (\pm 0.4)</td>
</tr>
<tr>
<td>30H</td>
<td>10</td>
<td>9–107</td>
<td>216 (\pm 13)</td>
<td>10.1 (\pm 0.6)</td>
</tr>
<tr>
<td>24H</td>
<td>8</td>
<td>15–92</td>
<td>285 (\pm 8)</td>
<td>12.5 (\pm 0.4)</td>
</tr>
<tr>
<td></td>
<td>66</td>
<td></td>
<td>247 (\pm 5)</td>
<td>10.7 (\pm 0.2)</td>
</tr>
</tbody>
</table>

\(^e\) 0DC and 17BD were female, 24H was singleton.
\(^\text{SE}\) \(\pm \text{SE}\).
tapering off slowly from then on. This is essentially the same graph Dr. Williamson showed to David and me in August of 1977, but with the 1978 data added to it.

b. This graph shows the changing proportion of the fawn's diet that was made up of milk. In the upper left hand corner, milk forms 100% and in the lower right hand corner, 0%. The half way point, after which solid food dominates, is at about 55 days. Inspection of the curves shows that the fawns eat relatively little solid food until about 40 days, and then the proportion of the diet made up of solid food rises rapidly until about 65 days, increasing slowly after that.

c. This table shows how much energy and protein were available from the solid feeds and from the does' milk at various stages of lactation (the composition of milk changes during lactation.). The latter figures are based on the milk consumption study that was published in 1977. The digestibility of the solid feeds was taken from David's in vitro study and digestibility of milk was taken from the literature.

d. This table lists for each fawn analysed in the paper, its average daily food consumption, in relation to its size, for the first three months of life. That is to say, its total food consumption for the first three months of life was calculated, along with its average weight during that period which was converted to average metabolic weight. Then the total food consumption was divided by the number of days in
Birth weights, growth rates, and total consumption of digestible energy and protein by fawns to different ages

<table>
<thead>
<tr>
<th>Fawn</th>
<th>Birth weight, kg</th>
<th>Growth rate, g x 10^-3 per kg^-1</th>
<th>Energy, kcal x 10^-3 per kg^-0.75</th>
<th>Protein, g x 10^-3 per kg^-0.75</th>
<th>Growth rate, g per kg^-0.75</th>
<th>Energy, kcal per kg^-0.75</th>
<th>Protein, g per kg^-0.75</th>
<th>Λ,%</th>
</tr>
</thead>
<tbody>
<tr>
<td>15F</td>
<td>3.2</td>
<td>39 (24)</td>
<td>8.13</td>
<td>0.33</td>
<td>25 (58)</td>
<td>0.06</td>
<td>0.33</td>
<td>0.6</td>
</tr>
<tr>
<td>15G</td>
<td>3.2</td>
<td>35 (24)</td>
<td>7.24</td>
<td>0.30</td>
<td>22 (58)</td>
<td>0.06</td>
<td>0.30</td>
<td>0.6</td>
</tr>
<tr>
<td>17BA</td>
<td>2.2</td>
<td>46 (21)</td>
<td>6.45</td>
<td>0.25</td>
<td>31 (55)</td>
<td>0.06</td>
<td>0.25</td>
<td>0.6</td>
</tr>
<tr>
<td>17BB</td>
<td>2.5</td>
<td>42 (21)</td>
<td>5.80</td>
<td>0.25</td>
<td>29 (55)</td>
<td>0.06</td>
<td>0.25</td>
<td>0.6</td>
</tr>
<tr>
<td>21AC</td>
<td>2.6</td>
<td>41 (24)</td>
<td>6.52</td>
<td>0.27</td>
<td>27 (58)</td>
<td>0.06</td>
<td>0.27</td>
<td>0.6</td>
</tr>
<tr>
<td>21AD</td>
<td>2.4</td>
<td>39 (24)</td>
<td>7.30</td>
<td>0.34</td>
<td>28 (58)</td>
<td>0.06</td>
<td>0.34</td>
<td>0.6</td>
</tr>
<tr>
<td>0DC</td>
<td>3.0</td>
<td>31 (27)</td>
<td>6.68</td>
<td>0.28</td>
<td>26 (55)</td>
<td>0.06</td>
<td>0.28</td>
<td>0.6</td>
</tr>
<tr>
<td>0DD</td>
<td>3.2</td>
<td>33 (27)</td>
<td>8.41</td>
<td>0.34</td>
<td>28 (55)</td>
<td>0.06</td>
<td>0.34</td>
<td>0.6</td>
</tr>
<tr>
<td>17BC</td>
<td>2.2</td>
<td>44 (21)</td>
<td>5.41</td>
<td>0.23</td>
<td>32 (56)</td>
<td>0.06</td>
<td>0.23</td>
<td>0.6</td>
</tr>
<tr>
<td>17BD</td>
<td>1.9</td>
<td>44 (21)</td>
<td>5.75</td>
<td>0.25</td>
<td>31 (56)</td>
<td>0.06</td>
<td>0.25</td>
<td>0.6</td>
</tr>
<tr>
<td>30G</td>
<td>3.4</td>
<td>28 (23)</td>
<td>4.78</td>
<td>0.20</td>
<td>24 (58)</td>
<td>0.06</td>
<td>0.20</td>
<td>0.6</td>
</tr>
<tr>
<td>30H</td>
<td>3.8</td>
<td>33 (23)</td>
<td>5.93</td>
<td>0.24</td>
<td>22 (58)</td>
<td>0.06</td>
<td>0.24</td>
<td>0.6</td>
</tr>
<tr>
<td>24H</td>
<td>3.4</td>
<td>34 (22)</td>
<td>5.64</td>
<td>0.24</td>
<td>23 (50)</td>
<td>0.06</td>
<td>0.24</td>
<td>0.6</td>
</tr>
<tr>
<td>x</td>
<td>2.85</td>
<td>37.5</td>
<td>6.484</td>
<td>0.274</td>
<td>26.7</td>
<td>0.36</td>
<td>0.018</td>
<td>0.6</td>
</tr>
<tr>
<td>SE</td>
<td>0.16</td>
<td>1.6</td>
<td>0.296</td>
<td>0.012</td>
<td>0.9</td>
<td>0.36</td>
<td>0.018</td>
<td>0.6</td>
</tr>
</tbody>
</table>

*Proportion of total calories that were in milk consumed over the whole period

Values in parentheses are actual ages in days.

Regression of growth rate of fawns to 60 days on their dam's growth rate to 60 days. Each symbol indicates fawns from a single dam.
the period (the exact number of days varied from one animal to the next) and this average consumption per day was divided by the average metabolic weight. These figures can be used to compare different fawns' individual food consumption rates, which can then be compared to their birth rates.

e. Inspection of this table shows that some of the fawns that were quite small at birth have the highest growth rates; that is, they appear to be compensating for their smaller birth rates. This can be seen in relation to the total amount of food consumed in that period, and compared with the rate of intake, in the previous table. It is also evident from this table that Dr. Williamson has decided to divide the three month period into three sub-periods, 25, 26 - 60, and 61 - 90 days, apparently because the growth curves had three different phases, in his estimation.

f. Here we have a comparison of the growth rates of fawns in the first two months of life with the growth rates of their mothers at the same age. The correlation coefficient here is 0.922. This figure is a measure of how well the mother's growth rates can be used to predict the growth rates of their young. R=0 means that there is no predictive power, and r=1 means that the mothers' growth rates are identical to those of their fawns. R=0.922 means that the correlation is very high. The value p=0.001 is a measure of statistical significance. It means that there is only one chance in a
FIGURE VI: Cont.

Regression of proportion of calories consumed to 60 days that came from milk against birth weight of fawns.

h:
- Analysis of variance, regression, and correlation of birth weight and mean daily energy or protein consumption on growth rate

<table>
<thead>
<tr>
<th>Birth to 60 days</th>
<th>Birth to 90 days</th>
</tr>
</thead>
<tbody>
<tr>
<td>Energy, kcal/kg^{0.75} per day</td>
<td>Protein, g/kg^{0.75} per day</td>
</tr>
<tr>
<td>Intercept</td>
<td>3.78 x 10^{-2}</td>
</tr>
<tr>
<td>B_{1}</td>
<td>-5.08 x 10^{-3}</td>
</tr>
<tr>
<td>B_{2}</td>
<td>5.146*</td>
</tr>
<tr>
<td>B_{3}</td>
<td>1.58 x 10^{-5}</td>
</tr>
<tr>
<td>t_{1}</td>
<td>0.553</td>
</tr>
<tr>
<td>F</td>
<td>13.38*</td>
</tr>
<tr>
<td>r</td>
<td>0.932**</td>
</tr>
</tbody>
</table>

Note: **, P < 0.01; *, P < 0.05.
*Birth weight (in kilograms) as variable; **Energy (in kilocalories per kilogram^{0.75} per day) or protein (in grams per kilogram^{0.75} per day) as variable.

i:

<table>
<thead>
<tr>
<th>Age, days</th>
<th>8</th>
<th>15</th>
<th>25</th>
<th>35</th>
<th>45</th>
<th>55</th>
<th>65</th>
<th>75</th>
<th>85</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean weight, kg</td>
<td>3.8</td>
<td>4.8</td>
<td>6.0</td>
<td>7.4</td>
<td>9.2</td>
<td>10.9</td>
<td>13.0</td>
<td>15.3</td>
<td>17.0</td>
</tr>
<tr>
<td>Mean milk intake, g/day</td>
<td>495</td>
<td>505</td>
<td>490</td>
<td>502</td>
<td>401</td>
<td>337</td>
<td>287</td>
<td>263</td>
<td>209</td>
</tr>
</tbody>
</table>
thousand that this high correlation could be a result of sampling error, of choosing a group of deer who are unrepresentative of the deer population. This means that the correlation is very likely to be representative of the population the animals are chosen from, insofar as a statistical test can determine without a direct comparison to a larger sample.

g. This graph ties in with the previous two tables, of growth rates and average food consumption. The graph shows each fawn's birth weight in relation to the proportion of its diet that was made up of mother's milk. It indicates very clearly (r=0.928, p=0.01) that the lightest fawns depended more heavily on milk and less heavily on solid food than did the heaviest ones, which were more independent of their mothers. This is perhaps the way that the lighter fawns were able to have higher growth rates since milk is a very high quality, easily digested food.

h. This table is quite complex and I shall not try to explain it in any detail. It summarizes attempts to use birth weight, mean daily energy of protein consumption and growth rate as predictors of one another. That is, it gives the values that can be plugged into standard equations (that are not shown here, the audience is expected to know them) to show the statistical relationships between these variables.

i. This final table is very straightforward. It indicates how the mean size of the fawns and the mean daily milk intake
change with age. It shows a high and fairly constant total daily milk consumption until about the sixth week of lactation, followed by a rapid decrease after that. The decrease comes after the age at which we know, from the second graph, heavy consumption of solid food begins. This table gives us total daily milk consumption, and not consumption in relation to body weight, which declines from birth.

Within about three minutes our reader has assessed this paper's results section, and he knows from the tables and graphs what topics it deals with, for the most part: milk and solid food intake in relation to body weight, the absolute amount of milk consumed by suckling fawns and the changing constitution of their diet as they age, and the genetic and dietary determinants of their growth rates. All of these results are for captive black-tailed deer on a standardized very high quality diet.

So?

Curtis has to ask himself if this information helps him understand the dietary needs of wild caribou in the crucial period right after parturition, when the cows must produce a great deal of milk? How much milk do the caribou cows produce? How much forage do young caribou need, and how much do the mothers need in order to produce enough milk? Will results about deer provide any hints for work on caribou? Are there any such sources on caribou? If there are, has Williamson cited them? Perhaps Curtis has not had the time to consider this kind of
breakdown of dietary requirements having been fully occupied

just trying to find his animals, follow them around their range

and see how and what they ate. He might decide that while this

is of passing interest he does not have time now to read it in

any depth, but that one of his graduate students who is working

with captive animals might find it useful. On the other hand,

the relationship between the mother and her young is quite

important. Let us suppose that he decides to look quickly at the
discussion to see what Williamson makes of this work.

Dr. Williamson used the discussion section to bring out

what he proposes to be the significance of his results, to make

his pitch. He does so partly by linking his work to what has

previously been done on this and other species, work that his

intended audience will be familiar with, or have easy access to,
or have done themselves. One of the striking features of the
discussion section is the large number of citations it contains,

compared to the results section. In the results section,

Williamson cites two authors whose forms of statistical analysis

he uses, and refers twice to other papers he and Karl had

published, as sources of data. By contrast, there are 23

citations of other people's work in the discussion, 6 of his and

Karl's other papers, and a reference to a personal communication

from Don. I shall go over a part of the discussion section to

show what Curtis will see.

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*Caribou are notoriously difficult and expensive to study in the

field.
Williamson notes that his animals continued to lactate long after the great majority of their nutrition was obtained from solid food. Scanlon and Urbston have shown that white-tailed deer lactate well into the fall in the wild. (This is a paper that Don quoted during the summer of 1977 to support his claim that he should be able to continue to observe suckling of both male and female fawns well into the fall). Robbins and Moen have suggested that extended lactation may serve only to maintain the mother-young bond, but a Dr. Smith and his co-workers contend that it may provide a significant pre-winter protein source. Weaning is obviously not just a matter of reaching nutritional independence, because fawns weaned as early as 84 days in Roger Williamson's study grew at the same rate as ones that continued to suckle. Arnold et. al. showed that for sheep the mother's milk yield determines the strength of the ewe-lamb bond. The long "tail" on the lactation curve may be the way the deer work out the parent-young conflict suggested by Trivers. Williamson does not elaborate on this point for he expects his audience to be familiar with it. For your information, however, Trivers is a well-known sociobiologist, whose work Williamson knew largely because of David's and Michael's enthusiasm for it. Trivers' argument can be interpreted to suggest that it is in the mother's interest to keep the young around long after they have become nutritionally independent, because her presence will improve their ability to forage and avoid predators. This in

5Scanlon and Urbston, 1974.
turn will increase their chances of surviving to produce her grandchildren. At the same time, it is not in her interests to provide too much milk, for that would be a burden for her at a time when conditions are getting difficult. Dr. Williamson concludes this paragraph by suggesting that there may be some selective advantage to the mother in producing enough milk in the fall to maintain the bond with her young.

The discussion goes on like this for another two pages, comparing these results for black-tails with studies of captive mother-raised black-tails, free-living mule deer, captive white-tails and domestic sheep. For the most part, Williamson discusses determinants of growth rates: milk consumption and solid food consumption by the fawns, genetic inheritance, and the mothers' nutritional state. He considers possible limitations on the fawns' growth: that the mothers may not be capable of producing as much milk as the fawns can consume, and the efficiency with which the fawns can utilize both milk and solid foods.

I suspect that our reader would find this discussion somewhat confused, and its conclusions suspect. A major issue here is that the fawns' ability to grow quickly early in life is crucial, for the largest fawns are the most likely to survive this extremely vulnerable period. Williamson's results indicate that despite the compensatory growth that the smallest fawns show, they do not catch up. Birth weight is the best predictor of size at age three months—by which time the animals are
largely on their own, nutritionally. As Williamson notes, "This indicates the importance of genotypic factors and (or) factors during pregnancy in influencing the subsequent early development of the fawn." Since these animals are on a standardized high-quality diet, and can eat as much as they wish, it stands to reason that conditions during pregnancy for these animals are relatively uniform, which would throw genetic differences into relief. Inexplicably, Williamson draws the opposite conclusion, "Under conditions of high-plane diets, variability in fawn growth is largely a result of pre-partum conditions." Williamson sums up by noting that under field conditions some does will be better fed than others, and have larger fawns with a greater chance of survival. This study suggests that the mother's nutrition prior to birth is probably as important to her fawns' survival as her nutrition during lactation.

The earth has not moved under Curtis' feet during the last five minutes, but he is piqued by the idea that very young ungulates may need more solid food than he had previously realized. Now, he wonders, are the milk consumption data reliable? This was the most pressing question we faced while doing the research, and it exercised the editors and reviewers of the journal to which the papers on milk yield were submitted. Curtis knows that very young mammals of some species defecate and urinate while suckling, and will want to know how Dr. Williamson dealt with this problem. There is also a possibility that the fawns might have drunk less milk during the.
experimental periods that the rest of the week, as a response to
being handled. Such behaviour would lead to an underestimate of
milk consumption. He wonders how reliable the figures for energy
and protein content of the does' milk are, for such information
cannot be common and probably is based on a samples from a few
animals. And, of course, he has misgivings because of the small
number of animals used in the study. The results might not be
representative of the species. Like everyone else working in
this area, he knows, however, that such studies never involve
large sample sizes. This is because of the expense involved in
keeping large animals, a limitation that is accepted because
there is no way around it. You just have to take what you can
from such work.

To pursue these questions Dr. Curtis turns to the methods
section which provides a typically sparse account of some of the
technical procedures that were followed. There is a very brief
description of the animals used, the conditions they lived
under, and the kinds of food the does ate while they were
pregnant and lactating. The method of determining milk
consumption is discussed in an equally short space, and the
reader is referred to the milk yield paper for details. As well,
Williamson tells how the figures for energy and protein content
of does' milk were obtained, digestibility of solid feed
determined, and weekly consumption estimated from two day
experimental periods. This section of the paper is quite short,
less than half a page long, and it is even printed in smaller
type than the rest of the text, as if to suggest that the
editors do not want it using up too much valuable paper. Victor,
the student who worked on moles, told me that he received
extremely harsh criticism from editors for making his methods
sections too long. He summed up his conclusion from these
episodes by saying, "Journals are not supposed to be serial
methods manuals." He was referring to the typical journals of
his field, ecology, which generally publish articles discussing
experimental results, or theoretical issues. There are also
articles devoted to describing new techniques in considerable
detail, but this is a different proposition from the one Victor
and I were discussing.

If my observations of David and Victor and the others are
any indication, our Dr. Curtis will use the materials and
methods section to try to put himself in Dr. Williamson's
position, drawing upon his own experience to fill in the blanks
as best he can. He will try briefly to reconstruct a situation in
which such work could be done so that he can assess the sort of
situation to which the results would apply. He is trying to
figure out the limits of this information. He will probably
conclude that more accurate milk consumption results would have
been obtained by using a radioactive tracer injected into the
mother that would come out in the milk and be measured in the
fawns.

You may be very sure that neither Dr. Curtis, nor anyone
else in this field will read the methods and materials section
of this or any other paper with an eye to actually replicating
the experiment in order to assess the validity of the results.
People in this field certainly pick up on the methods used by
others, if their citations of them are any indication, but they
use these methods in pursuing their own research problems. Dr.
Williamson dismissed replication as "a myth". Research projects
take years to complete, and consume all of a very limited amount
of money. Besides, doing an extended piece of work that had
already been done would be the kiss of death. Scientific
research projects are supposed to be original, and a manuscript
sent to a journal will be rejected if it reports work that has
already been published. In some fields a researcher might repeat
an experiment if he expected to get a result different from the
one reported, and if a controversial theoretical point hung in
the balance. The problem with ecology, in this regard, is that
it lacks the kind of mathematically rigorous theories that can
be focussed so narrowly as to be resoundingly confirmed or
refuted.

Let us suppose that after surveying the paper, Dr. Curtis
decides not to read it through, but rather to pass it on to his
graduate student who works with captive moose. This student
reads it carefully, and seeing that Williamson has published
another paper on milk yield, reads it as well. In its
introduction he sees a reference to a study of milk yield of
captive caribou—which he passes back to Curtis, and Curtis
reads that paper. Curtis also retains what Williamson had to say
about the change-over from milk to solid food consumption, and the very high proportion of the fawns' diet that comes from solid food while lactation is still underway. This information appears a few times in discussions between Curtis and his students about their work, such as I illustrated with Dr. Williamson and his students. One or two of Williamson's graphs are removed from the context of his paper and placed in the context of the users' research projects, treated as possible descriptions of what deer are like, and as hints for work on caribou or moose. They are now very far removed from their origin, but Curtis or one of his students might go back to the paper to look at it again and reassess its significance and reliability before deciding to trust or reject it. This could happen at any time. The paper could be forgotten for a while, and then retrieved and re-read because of something that happened one day that reminded him of it.

A published fact can also find its way into papers written by other authors. For instance a paper was published in 1981, called "Growth and nutrient consumption of elk calves compared to other ungulate species", which contains information drawn from Williamson's work, and more than thirty other studies. The authors of this paper used some of Williamson's data on birth weight and milk consumption. His results appear as two dots on a

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graph which summarizes the results of half a dozen studies, each on a different species, each by a different scientist. Another graph in the same article draws on some thirty publications, each project being reduced to a single point. The senior author of this comparative study has known Dr. Williamson for a number of years. In fact, he was to get some of the mountain goats we captured in the expedition I mentioned earlier. In addition, Dr. Williamson had helped to set up a deal whereby Michael would go to this man to do his doctoral research. This biologist knew of Williamson's work long before it was finished, and had the information he used in this paper before Williamson's paper appeared. It should be remembered, though, that this was not some kind of "informal" communication that sidestepped the process of public representation. These data were already highly formalized by the time Williamson would consider showing them to anyone outside the group. They already had a very public look about them. In this comparative study the results of other research projects are reduced to mere dots on graphs. Two of them are reproduced on the facing page as illustrations. The lower one owes two of its points to Williamson's work. This graph refers to the "tails" of the milk consumption curves, when the young animals are living mainly on solid feed. The graph indicates, if you accept it, that for the animals with smaller young at birth, like black-tailed deer, milk consumption declines more quickly in this period than is the case for the species with larger young at birth. In plain English, the tail
of the lactation curve is longer for large ungulates than for small ones.

Now, a very scrupulous reader of this paper might check the various studies the authors drew upon to decide whether or not he should accept the graphs, but I suspect he would do this only if he had other reasons to read these papers. A reader who decides to accept the graphs I have shown, is dealing with information that is a very long way from its origins indeed. The reader is in effect accepting the writers' assessments of the papers cited, and treating their outcomes as facts, at least for the time being. He may change his mind.

Publishing a research paper has consequences for the author's career that reach far beyond the research setting, and have virtually nothing to do with the specific technical assertions the author makes. The paper becomes another addition to his curriculum vitae, it becomes a pro forma measure of his productivity that is noted by people who will never read his work, and could not understand it if they did read it. Promotions, tenure, and job applications depend on keeping one's name in print; the simple fact that journal editors accepted a scientist's work carries considerable weight, particularly with people who are from outside his field and probably have not heard of him by reputation, or read his papers. I mentioned earlier that before coming to Western University Williamson published papers on artificial insemination, a subject that does not interest him a great deal, because there was little other
opportunity for research, and he had to keep on publishing if he were to get a university appointment. Similar considerations applied to his application for the job he now has. Only an ecologist could apply for that position, and to get it he had to prove he was one: enter the curriculum vitae. Of course it makes a difference if one's work is highly respected and there are scientists all around the world who have taken your ideas and procedures into their own work. It cannot be denied, however, that a lengthy c.v. has a cachet that is wholly independent of what the author has actually written.

The students

Since I have gone to considerable effort to introduce all of the people who played a part in this research, it seems only fitting to indicate how their research projects ended, and what they did next. So I shall briefly summarize the remaining work that David and Don did, and mention what became of Kirstin, Michael and Elaine, as well.

Dr. Williamson's re-evaluation of the milk consumption data came at the time when his and Don's work were winding down and David was taking over the foreground. The measurements of milk consumption and observations of suckling ended in mid-October. One month earlier, David had combined the female fawns from this study with his own, and had weaned them all. On June 26 he assigned them randomly to two groups. The members of one were to
be fed a high energy ration containing 3100 calories per gram of digestible energy, and the others a low energy ration of 2550 calories per gram. Each of these feeds was to contain the same concentration of digestible protein. The lower energy ration was meant to approximate the diet available to free-living fawns in the fall, when the quality and quantity of wild food declines.

David took the point of view that puberty attainment required that fawns reach some minimum weight and have a high calorie diet during the breeding season. He hypothesized that the diet in the wild does not enable the fawns to capitalize on the maturity they have achieved, and enter puberty, because they are short of calories at that time. He guessed that some of the fawns in his high energy group would reach puberty and that, like wild fawns, none of the low energy group would do so.

His results were ambiguous. One high calorie fawn conceived and carried the fetus to term, and none of the low calorie group were known to have become pregnant. But this was only one test of puberty. David had also sampled the animals' blood through the summer and fall, and assayed it for reproductive hormones. The assay showed that most fawns in both groups had had transient high concentrations of progesterone in their blood during the breeding season. Serum progesterone rises sharply at the time of ovulation, but such spikes can occur without ovulation and hence without indicating puberty. It was possible that some of the fawns in the low-energy group had reached puberty, but had not been impregnated, or had aborted or
The results greatly distressed David. Then he did an in vitro analysis of the experimental feeds and it turned out that the two diets were not as different in caloric value as they were supposed to be. They fell in the ratio of 3185/2906 instead of 3100/2550, so that the low calorie ration provided quite a high plane of nutrition. The rations had been developed by the nutritionist at a commercial feed company. He had combined grains, alfalfa and supplements in proportions that were based on standard digestibility tables derived from experiments on cattle. Evidently, the deer digested these materials more effectively than cattle normally do.

It made David considerably happier to be able to explain his results: he claimed to be content so long as he understood what had happened. He briefly considered doing some more work on the deer, bypassing the MSc degree and going directly to the PhD. But when the members of his supervisory committee said that he had enough data for an MSc thesis, he opted for that, defending it successfully in August 1979. He began his doctoral program the next month, a field ecological study of Stone sheep (Ovis dalli stonei) in far northern British Columbia.

Don did no further observations of suckling after October 1977. During the fall and spring he helped David with his research in return for the work David had done for him in the summer. Following protracted computer analysis of suckling observations and milk consumption figures he successfully
defended his MSc thesis in April 1981. One of his conclusions was that there was no significant correlation between the amount of time the fawns spent suckling and the amount of milk they drank. He is now working in environmental consulting. Kirstin completed her bachelor's degree and published a paper on her study of morphological development of the fawns gut. The last I heard, she was doing technical literature searches for the Ministry of Forests of the provincial government. Michael completed his MSc with Williamson in the summer of 1980, and immediately moved on to a field study of polar bears.
II. Summary and conclusions

The portrayal and analysis of scientific research is a complex undertaking, and the structure of this concluding chapter reflects that fact. The chapter has five parts. The first is a brief descriptive summary of what I consider to be the most important features of the research process. The conclusions, which follow, are divided into four parts. In the first of these I describe how I developed the narrative that enabled me to make sense of my field notes. This prepares the way for the next section, in which I lay out and discuss the major conclusions I have reached concerning the conduct of scientific research and how it should be portrayed and analysed. Following this I summarize the three central principles of this communications approach to inquiry. Finally, I consider the generality of the approach, asking whether or not the view of inquiry that I have developed is applicable to fields of science other than ecology, or to other professions as well. All sections of this chapter share the same themes. The themes appear as elements of research, as conclusions about how science should be discussed, as characteristics of bureaucratic institutions, and so on. In each section they are portrayed somewhat differently, seen from a somewhat different perspective. It is my hope that in doing this I have done justice to the multi-facted character of this problem.
Summary of the elements of research

Scientific research is a profession, and for the individual scientist, a job. Roger Williamson was a university professor. He worked as a researcher, teacher and administrator, and the latter two tasks, which he enjoyed in their own right, contributed significantly to the first. It was partly through the routine jockeying of university politics that he secured his deer herd and facilities, and some of his equipment, materials and money. His position and publications attracted the graduate students who, as is usually the case, did most of the research. Williamson also had to have an institutional niche if he was to be recognized by granting agencies, government ministries and the like, and taken seriously by his colleagues.

In regard to the affairs of the local world these people constructed I have stressed the pre-eminent importance of control and manipulation. Most of the work we did went into developing and maintaining various interlocking kinds of order. Some of this order came more or less ready-made. For instance, there were institutionally defined roles, such as "professor", "graduate student" and "technician" that we were accustomed to acting out, thereby determining the rough outlines of our relationships to one another. There were other and more important ways of ordering our experience. As I showed in Chapter Eight we placed the deer in a highly regulated...
artificial environment. Their physical circumstances, relations with one another and diet were closely controlled and standardized so that they could be observed systematically or reduced to numerical measurements. We, of course, were locked into the same patterns.

The deer had their own ways though and we could not impose any routines we wished on them. An important part of our job was to adjust to the deer. For instance, they reproduced but once a year. Experiments had to conform to this annual cycle, each one took a long time, and you could expect to do only one of them in a season. This is not a field in which you can have an idea in the morning and immediately rush off to the lab to set up an experiment. Furthermore if something went wrong, you had to wait twelve months to try again. We were thrown into contortions trying to deal with the way fawns defecated and urinated while suckling. We had to make sure that their diets were adequate, and had special feeds designed for them. The relationship between the doe and her fawns, the diurnal activity cycle, and other patterns, were important meeting grounds between the deers' requirements and our treatment of them. Our activities and ideas were shaped by the deer at the same time as we shaped their lives, and there were times when it was very difficult to say which side was in charge. It was essential to our inquiries that we not be completely in control, or we would learn nothing new about deer.
Another very important kind of order consisted of the ways we made sense of what was going on around us, how we came together to take stock of our situation, surveyed its history and future, and decided what should be done. There were many features of our work that we had to control and plan for, and so there were many kinds of explanations. David and I discussed our career plans, and where this work fitted into them. When Elaine went on holiday we had to review our schedules and commitments in order to decide how to get her job done. Dr. Williamson and his students had to consider the finances of the research, most notably the cost of salaries and fees, when they proposed experiments that might clarify the problems they were facing. When we tried to sort out the difficulties the scale was causing us we had to examine our present data, consider what was known from previous work on the deer, and make plans for the next summer.

This brings me to the question of the privileged explanations, the graphs, tables, and interpretations of them, that we thought, perhaps, would tell us what these deer are really like. These explanations were what we call scientific knowledge. They had a special status, during the research and later. They were the explanations that other scientists would want to hear about, that would appear in the papers Williamson wrote. The scientist's written account is concerned with the results of his research and analysis. It is his product, the package that his peers need (he hopes) for their own research,
and it is designed to be short, narrowly focused and logically consistent. In the public arena, no one wanted to read about preparing for Elaine's holiday or the discussions of what to do about the scale, although at the time we did these things they were just as important to organizing the research as the graphs were.

I was present on several occasions when a graph that Dr. Williamson eventually included in a publication was worked up and used to assess the kinds of highly regulated activities that I described earlier. In mid-August of 1977, he plotted the milk consumption data from the previous six weeks, in effect composing a graphical history of the project to that point. The pattern he saw was more consistent than he had expected, which pleased him considerably. This treatment of data meant that he had taken over part of Don's burgeoning research problems, and as part of this move he announced several changes in the experimental procedures.

One month later, while David and Don were re-organizing the deer pens, a misunderstanding between them resulted in a doe being separated from her fawns. Because her young ones were not taking her milk, she dried up. In the next experimental run her fawns gained no weight while suckling, repeating a pattern that had puzzled us a great deal and caused great distress in previous weeks. The explanations we had given during the summer for zero weight gain did not make sense in this case, however. There were no equipment problems, and the fawns were not
defecating and urinating while suckling any more. They had
outgrown that. Dr. Williamson realized that she was probably
dry, and that earlier null consumption events may have been
caused by the same thing. Not only that, he now had a record of
the way the fawns suckled when the mother was dry. He went back
through the records of suckling observations and milk
consumption and found that some of the null consumption results
were accompanied by a pattern of many repeated short suckles,
instead of a few long sustained ones. This was the pattern he
had just seen with a mother who was almost certainly dry. As a
result of reviewing the data in the light of what had happened,
he now had a different history of the summer's work. Points on
the graph that had previously seemed to be artifacts were
possibly accurate measurements of milk consumption. In a later
experimental run that day it was clear that Dr. Williamson
looked upon the deer differently, and was trying to detect
events that he had not considered looking for previously.

I take this to be an example of the complex reciprocal
relationship that develops between emerging formal
representations and the various kinds of highly controlled,
formalized activity that scientists try to maintain. Dr.
Williamson reviewed and changed our work patterns in the light
of the graph he had made up. Later he reviewed his data and
re-interpreted his graph because of something the deer did
during an experimental run. This in turn can be traced to a
mixup in the re-organization of the physical order of the
research setting. Thus, this formal way of representing the
deer, which was almost like a dress rehearsal of part of a
written paper, was very much an intrinsic part of our ongoing
relationship with the deer, one another, and the anticipated
external audience of our work.

This graph eventually was included in a published paper, a
claim to know what deer are really like, that Dr. Williamson had
to submit to his colleagues if he wished to continue his career.
This is the root of his commitment to the empiricist world view.
If Williamson's claim to know about deer is accepted by others,
it becomes a fact. Scientists maintain it as a supposedly
self-sufficient element in a world of formal discourse. It can
be seen and used in ways that take little or no account of the
conditions of its origins or of the active role that Dr.
Williamson and the rest of us played in producing it.

The written report plays a complex role in any scientist's
career. The work of trying to write a coherent explanation for
his peers affects his own view of his research, which is the
continuation of a process that has been going on in his work
since its beginning. He hopes that other scientists will be
affected by it, will read it, and incorporate it into their own
work. If people respond to what he has written he may have to
defend it publically against criticism, and he may become better
known in his field. The paper may influence his access to grant
money, talented graduate students may seek him out because of
it, and it may help to get him a job or secure the one he
already has.

Williamson's research report, his claim to know, is certainly about deer living under the conditions that we established. It is also about the people who did the work, for it displays their special interests very clearly in their choice of the research problem, and the methods used. It is also very directly and explicitly about the work of other scientists, for treatment of them occupies a large part of the paper. It is a political document in a very real sense, intended to influence the deliberations and actions of peers in several ways. To say that "the facts it contains are not of this character in themselves", is to ignore the active nature of reading, and the practical affairs in which the "facts" must be accepted as fact and be used if they are to have any significance at all. The world of formal discourse in which facts are sustained is part of, dependent on, and meaningless without ordinary everyday research work. Perhaps the substance of my remarks can be summed up by rewording a famous philosophical conundrum: if a published research report falls on the floor of an uninhabited laboratory, where there is no-one to read and act upon it, does it describe reality?
Conclusions: the relationship between my methods and my view of the conduct of scientific research

Discussions of the nature of scientific knowledge have traditionally been conducted in terms of perception and logic. I have chosen instead to approach it as a matter of institutional ideology and politics, of which the traditional mode of discussing Science is an important part. If one is to approach scientific knowledge as I have done here, then the central methodological question posed is: how does a person who has been trained to enter the scientific profession (or the academic profession in general) remove himself far enough from its requirements that he can dispute the story the profession has about itself, but not move away so far as to lose contact with research and publication?

Ambiguity is important to this kind of inquiry. You must be sufficiently involved with the academic profession to experience what is at stake in what a given group of its practitioners do, and sufficiently independent of that group that you can compose a different kind of story from theirs about the work they did. This in turn means that you must have a different audience from theirs, and that relationship, too, must allow considerable latitude. My field work was only part of the strategy by which I achieved this sort of ambivalence and I have already explained how the relationship I cultivated with the ecologists enabled me to do what I intended. It is crucial to remember, however, that
the field work experience did not provide me with the narrative that I needed in order to make sense of my observations and new habits. I had to develop this new narrative and while doing so I still had to take care that I maintain an ambivalent position, only this time with respect to the social sciences. In order to complete my methodological discussion I must therefore show how my ambivalent relationship to social science enabled me to compose this narrative.

Conclusions I, continued: making sense

After the fall of 1978, I no longer worked in the lab. Apart from a few interviews in 1980 and 1981 my contact with David and Don and other people from the deer project and the rest of the biology department was mostly social. I would encounter them in the pub or go canoeing and fishing with them. From 1979 on I was in essence a writer trying to produce a coherent story about the research for people who had not been there and did not share either its tacit understandings or my sense of the question of scientific action. After two years of field work I had not found or developed an idiom within which I could make explicit the delicate interweaving of planning and doing and knowing that I had seen.

The task was complicated by institutional barriers; it had a political and ideological dimension. This was not because any one person opposed the project. On the contrary it aroused
considerable friendly interest but I still had to deal with university and department regulations and traditions. It was clear from the beginning that there was at least one entrenched requirement I would have to side-step if I were to succeed in producing a viable account of how scientists' activities lead to objective knowledge. I would have to avoid being required to write up the work in the positivistic idiom of discovery that natural scientists and many social scientists use when writing their research papers and graduate students use when writing theses. However involved with the research I might be, it was necessary that I not become committed to that way of representing it.

The nature of this problem might be clearer if I contrast my approach with another I might have taken. I could have learned about research by becoming a professional biologist myself. I could have enrolled in the Biology Department and been Dr. Williamson's graduate student. He would have required me to write up my deer research in the conventional scientific idiom, in a thesis and then in articles for professional journals. For him to do otherwise would be grossly negligent, for my scientific career would depend completely on my producing such accounts and standing behind them publically. Then of course I would be professionally committed to the story of my research that these papers and thesis portrayed. That would be very much at odds with the aims of this study because such portrayals specifically obscure much of the inquirer's active participation.
in the research process and all of the changes which occur
during it, and that is exactly what I did not want to do.

Instead I had to be more ambiguously related to the action
in the lab, operating at one remove so that I was not completely
drawn into the scientists' way of regarding their work, in spite
of my involvement with their research. I was far from
understanding the extent and the subtlety of the problem or its
rhetorical character, but I did know that whatever account I
wrote about research it would achieve some things that the
scientists' papers specifically exclude, and tell an altogether
different kind of story from theirs.

I knew that I would encounter similar issues in my
relations with conventional social scientists, and the ways that
they approach the question of what science is. I was aware
although I did not pursue the matter at the time that being able
to claim that their work is scientific is very important to most
social scientists, that they have traditionally considered
themselves to be emulating the methods of the physical sciences,
as revealed by the positivistic tradition. ¹ I knew that this has
made it rather difficult for sociologists to approach the
question of explaining the relationship between what scientists
do and what they know, for the methodology of the social
sciences is rooted in a tradition which holds that for Science
there is no such connection.

¹ Giddens, 1979, Chapters One and Seven.
In recent years historians and philosophers have criticized the positivistic view of Science and these critiques were soon accompanied by pioneering attempts to examine contemporary science first hand.\textsuperscript{2} Ten years at the same time the critiques contributed to a serious methodological crisis in the social sciences, and added to the interest of social scientists in developing a new and more viable view of science.\textsuperscript{3} That is to say, one of the drives behind the interest of social scientists in developing a new model of science has been their vested interest in defining themselves as Scientists. As an example of the sorts of confusions that this situation gives rise to, there has been a strong conviction among some sociologists that a Scientific (as nearly as I can tell) study of science is called for.\textsuperscript{4} I knew that the study of research must not be guided by the requirements of Science. Given that, I did not know what it could mean to assert at the outset that a study of scientific research should itself be scientific.


\textsuperscript{3}Giddens, 1978.

I was prepared to reserve judgement on this matter until later, and was convinced that becoming a graduate student in a mainline social science department would force my hand on this issue. I would be obliged to take courses and answer comprehensive examination questions on debates about naturalism, realism, positivism and the like. Having to take an explicit stand on these debates, having to accept them as defining the appropriate questions before I could enter the laboratory, would only confuse me. I had to look to the affairs of scientific researchers for direction on the nature of science and I had to be free to claim, if necessary, that my own work simply is not like theirs.

The decision to base my efforts in a transdisciplinary department was a practical political one, an attempt to secure the best available conditions for the study. Bluntly, it was necessary that I sidestep orthodox ideological indoctrination as much as possible. The Department of Communication gave me distance from both the ecologists' world and from the disciplines in which contemporary science conventionally is studied. Nevertheless a university department is a university department, and paradoxically it was my participation in routine academic politics as a graduate student that lead to my being able to develop the narrative I needed. The department provided an institutionally recognized niche within which I could work. In addition to being a suitable intellectual environment, it enabled me to apply to granting agencies for funding and gave me
some credibility in the eyes of other people studying scientists. This last matter is crucial to all aspiring scholars but it turned out unexpectedly that the practical work of trying to advance my career showed me how to reconcile the various elements of the ecologists' scene into a coherent account, and place them in relation to the way they account for their work, and in relation to social science. Here is how it happened.

Toward the end of the field work I began to engage the professional academic world on my own behalf for the first time. I began looking for other people who had gone into laboratories to study science-in-the-making, searching through journals for articles on the subject, writing letters to well-placed academics who might have heard of such studies. I learned that in the mid-1970's, a few people had decided to go into laboratories specifically to see how experiments are done. Each of them was from a different discipline or school of thought, and they began their studies without knowledge of one another. They showed a different emphasis from the earlier investigators who had done participant-observation studies in labs in the 1960s. For example, Robert Anderson and Gerald Swatez had studied large laboratories, focussing on the way they were organized. They also considered the research projects being done in the lab, mainly in relation to the external pressures that were directing the kind of research that could be done. At that time there was no call for a close examination of specific
experiments.5

In the mid-1970's, however, a few people had decided to do just that. June Goodfield began a five year association with a cancer immunologist in 1973.6 Bruno Latour spent 1975 and 1976 in a neuroendocrinology lab,7 and Michael Lynch visited a brain research group in this period. Karin Knorr did field work with protein biochemists in 1976.8 Sal Restivo and Michael Zenzen worked with physical inorganic chemists in 1977 and 1978, and Sharon Traweek studied particle physicists during this period.9 There was even an emerging generation of new graduate students who were being trained by this latest wave of scholars studying research. For instance, Marc Grenier worked under the supervision of Steve Woolgar and Roger Krohn, studying limnologists (fresh-water biologists) at McGill University.10

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I began attending the conferences these people went to, and presented some papers myself. It is commonly said that most of the important action at academic conferences happens away from the formal sessions, and I usually found that to be true. Meeting the people I have just mentioned gave me a sense of the scope and the origins of the questions that I was learning to ask which I could not have gotten by reading their work. In fact, little had been published then. Furthermore, each of the people I have mentioned developed his or her own distinctive theoretical approach. None of them was compatible with the way I have chosen to write this thesis. As a result I have not been able to incorporate these perspectives into this work in an explicit and systematic way, yet their authors have influenced me. From place to place, though, I have cited or quoted their work as an indication of my indebtedness. My thesis supervisor, Robert Anderson, and I, organized a conference on laboratory studies. I wrote papers, applied to the university and to funding agencies for scholarships, travel funds and conference support, spoke with journal editors and embarked on a limited reading of the literature of the social studies of science.

In each case, whether I was applying to a governmental agency for money, preparing a paper for a conference, or talking with another student, I was trying to provide an account of what had happened with the deer and the ecologists. The story of

\[\text{11}\] "Communication in Scientific Research". Department of Communication, Simon Fraser University, September 2-3, 1981.
ecological research was an integral part of my personal and professional relationships to specific persons and institutions. In fact it was the only reason that many of these relationships existed at all. The considerable effort of writing papers and grant applications, the anxiety of public performance, the willingness of certain institutional functionaries to provide funds for my efforts—all affected the way my field experiences were organized in my private thoughts and in my conversations with other students, my supervisor, scientists, and other people who study them. Explaining my work to others and incorporating their views into the story I was telling was a very large part of the process by which I made sense of it to myself. 12

After two years of divergent and frustrating analysis and writing which consistently did not come to grips with my implicit sense of what makes research tick, my political activities lead me to the following conclusions about my attempts to make sense of ecological research. In establishing and re-enacting my connections with the academic profession I produced accounts of ecological research. Conversely, in producing accounts of ecological research I incorporated specific contacts with the profession into my thoughts and actions. The story of ecological research was not like some passenger riding in the vehicle of my career but developed along with it and became a semi-autonomous factor contributing to its

progress.

It struck me that Dr. Williamson and the others had done something very similar, but with respect to different audiences. Moreover they had done this throughout their research, and not only after the experiments were done and they were writing papers. My activities had given me an insight into what the ecologists were trying to accomplish and how they went about it that I could not have developed without putting myself and my own work at risk as they did. In many ways they and I were doing the same kinds of things, and that is what enabled me to make sense of their work.

Conclusions 'II: main conclusions

It was in the wake of this realization that, urged on by Bob Anderson, I decided the key lay in portraying scientists at work in a world that was partly created through their own efforts. I wanted to write an account that would show how they drew upon their own resources, relationships with one another and their own understanding of their situation in order to maintain the coherence and continuity of their work. It would be necessary to depict such events as David, Williamson and I sitting together in the sun outside the deer barn, reviewing our data and trying to decide how and when to change the deers' diets, how to make the scale work and where to get research funds. Not only that, but this incident would have to be linked
to others events before and after in a way that would show how David and the others capitalized on opportunities, endured difficulties and worked to make all of this hang together. I did not want to write an account that would chop their lives up into a mosaic of examples that could be re-arranged into groups of the same kind, with one chapter being about "social structure", another about "funding", another on "hypotheses" and so on.

I became convinced while writing that the only way I could achieve this end was to organize the thesis around the developing action of their work, which meant that I had to introduce the reader to their world. If I were going to explain how Don and David developed their research projects, and why they behaved as they did, then my readers would have to know what these men were dealing with. For instance, I would have to provide enough background biology that readers could appreciate what they and Williamson were trying to do, and understand what was a stake in his decisions. Likewise, I would have to introduce the people and the relationships they had worked out among themselves if I were going to show how they decided what was really going on in their research. And obviously I was going to have to introduce the deer and equipment, for these did a great deal to set the course of the work and determine its outcome. The patterns of this world, as the scientists established them, were of crucial importance and had to be followed. This amounts to the assertion that these people's lives are important in themselves, not merely as examples of
I think this is a point of view that some novelists would find congenial, and along with it goes the belief that the general message of any story depends on the complexity and richness of detail with which it recreates a particular milieu and the efforts people make to conduct their lives there. The general significance does not proceed from reducing life to a series of simple principles in an abstract scheme which is then illustrated by a collection of incidents wrenched out of their context and relation to one another. In sum, I concluded that to show the significance of the people's activities in making science work I would have to recreate them.

I tried to recreate the researchers' points of view but from place to place I also had to stand back from the their world, and examine it critically. David, Don and Dr. Williamson repeatedly criticized what they were doing, of course. They went over their analyses, procedures, and the like, anticipating the comments of their colleagues and comparing the immediate situation with their previous experiences. My critique is very different. It concentrates on the form of their discourse and the place of that form in their affairs.

Some of the ways these people accounted for their work obscured their own participation in producing the knowledge that they claimed to have. There is nothing unusual about this. Everyone does it and for this reason it had to play a prominent part in my story of research. Chapter Four, for instance, has
some of the character of an introductory biology textbook, which is a fitting way to introduce the natural world as seen through the eyes of a biologist. Chapter Five is based on participants' recollections of the early years of the project. They portrayed their past to me quite selectively I am sure but they portrayed it to one another in a similar manner, for I was an accepted member of the group. The way these people re-enacted their past was part of the way they negotiated the nature of their present relationships to one another, and I used excerpts of interview tapes to stand in for the conversations that I did not tape (I decided not to use tape recorders in my field work—a decision I now regret). I did this to recreate their milieu, if not just as they normally did, then in a way that carried very similar messages.

I could not go on doing this indefinitely or eventually I would have accepted the scientists' point of view on their previous experiments: I would have accepted the ultimate form of their dominant stories about research. If I had been drawn into this positivistic view of their past, it would have been very difficult for me to make any other kind of sense of their ongoing research. Therefore, in Chapter Six I broke with this pattern. I analysed the papers they wrote about the work that had been done prior to my arrival and filled in what had been left out of them. This analysis distanced me from their point of view and prepared the way for looking over their shoulders, so to speak, in chapters Seven through Ten. These are based on my
field notes, in many places are taken very directly from them. It is crucial to remember, however, that the writing I have just described was a continuation of my inquiry into ecological research. I was not simply transcribing my conclusions about science. My present view of research emerged while I was writing, and each chapter was constrained and directed by previous ones. Writing Chapter Six stabilized my way of understanding the place of established facts in daily affairs and enabled me to assemble the following chapters from the field notes.

Likewise Chapters Four through Ten set the stage for writing the introductory chapters. There I had to define the questions the thesis deals with and provide a coherent account of my own activities. I found that I had no choice but to do so in a way that is consistent with my treatment of scientists. It was then that I understood how it is that the inquirer’s portrayal of himself and his portrayal of the people he studied are inevitably rooted in the same world view. I found that the attitude I had cultivated by writing the middle chapters prevented me from simply defining the thesis’ direction in terms of current debates. I realized that if I discussed my research as if I had been an objective observer working from a static viewpoint, as the empiricist tradition specifies, I would introduce the thesis in a way that is wholly at odds with what it says. Someone who has already shown scientists to be active inquirers cannot hide behind the shield of professional
objectivity. After some thought, I decided that I would have to account for my own activities in terms of involvement and change, decisions and consequences, situations and narrative form. As a consequence this thesis discusses two simultaneous inquiries: theirs and mine. It states both explicitly and implicitly that any inquiry is an affair of both the knower and the known and must take into account the relationship between them.

Conclusions III: three central propositions of this communications analysis of professional inquiry

In writing this thesis I have repeatedly stressed a point so simple and basic that its significance may be underestimated even now: a research project has to be done somewhere, by somebody. I contend that scientists' activities can be studied, and that what they claim to know can be explained by re-enacting and analysing research in the appropriate narrative form. I develop this assertion as three main interlocking claims about the formal knowledge that Dr. Williamson and his associates produced about deer, and its relation to the work they did. These are central to understanding the account I have written, and I shall state them briefly here before summarizing the view of research that I have articulated in this thesis.

First, the paper Williamson wrote proceeded from and in many places was explicitly about the relationship these people
cultivated with their deer and with their colleagues and supporting institutions. That is to say, I am proposing that an adequate explanation of the origins and nature of scientific knowledge must take into account the way that scientists are involved with both their subject matter and their supporting institutions. Accordingly, I have taken the "working world" or "working situation" very seriously. I use this term to indicate the wide range of deer, people, information, events, political connections and attempts to understand that were created or gathered together by Dr. Williamson and the others at the Research Forest and at Western University in order to do their research and pursue their careers.

Second, there is a complex mutual relationship between the formal world of public discourse and the local, supposedly informal world of research action. A scientist's job is to contribute to a world of formal discourse. The definition of "formal" that I use here is taken from the Oxford English Dictionary: "Done or made with the forms recognized as ensuring validity; explicit and definite as opposed to what is a matter of tacit understanding". The forms recognized as ensuring validity are those of public representation, namely graphs, tables and equations. Producing an explicit, definite specification of how deer behave under given circumstances was Williamson's primary objective. His experiments involved him with the deer, but writing for an outside audience distanced him from them, even as he stood in the deer pens.
As the research proceeded one of our ways of understanding it, the description of milk consumption, was repeatedly reworked and became progressively more formal, more public, more removed from the immediate material circumstances of the work. Williamson could recover the connection if necessary, but he became more confident of the separation as time went by. The members of the group unself-consciously recounted the public stories of previous work done in the project, making no conscious discrimination between these formalized tales and stories that had not been rationalized and refined for public consumption. They also read the research reports published by their colleagues, using them to identify some of what was going on around themselves, to get perspective on their own situation and determine its likely relevance to work outside their lab. Consequently, the research activities were marked by a complex interweaving and mutual reworking of local and public discourse. The public world was brought into the lab, and the results of our efforts were added to the landscape of the public scene.

Third, producing and reworking the formal account was an integral part of the research process. Williamson and the others produced succeeding versions of it in their attempts to decide what they thought was going on, to negotiate their position in relation to others, and to determine what should be done. It is simply the case that knowledge is never completely static. It must be sustained by individuals in relation to what they are doing and in relation to their audiences. It is not a
description of a state of mind nor of an external reality.

What scientists claim to know cannot be accounted for by
minute examination of an isolated episode of laboratory
activity. Continuous temporal development is an absolutely
indispensable part of the story. I have therefore placed
considerable emphasis on the temporal development of the
context, the researchers activities in it, and the delicate
connections between the two. I have done this in order to show
how the development of knowledge is part of the way that
researchers dealt with the consequences of their own actions and
those of the deer. Throughout I have endeavored to show how the
researchers and the deer influenced one another and how this
relationship was essential and intrinsic to what was known about
these animals. Likewise, I have taken pains to show how the
institutional milieu and professional connections of the group
shaped and directed the relationship between the people and the
deer, so that scientific knowledge was produced.

In sum, in the view that I have proposed we have the
inquirer, the reflective organizer of the research, in
transaction with the context within which he works, by which he
is constrained and directed, and to which he contributes. I
believe that by taking this view of research and of the place of
knowledge within it we avoid the Scylla and Charybdis of
theories of knowledge, namely the subject and the object, and
may proceed instead to inquire into inquiry.\textsuperscript{13}

Conclusions IV: the applicability of this approach to other kinds of inquiries

It is reasonable to ask just how general my view of scientific research is. How much can one learn about the profession by studying one group? A great deal, I think. It must be obvious from the way I have written that I believe I could go into other scientific laboratories without having my present view shaken from its moorings. Whether or not it is this stable remains to be seen but for the purposes of this discussion I shall assume it will be.

I should make it clear at the outset that my discussion of the generality of this view will have nothing to do with demarcation criteria, with establishing standards in accordance with which one can distinguish science from non-science. This sort of debate appears to be quite important to scientists, for I have frequently heard scientists I know say that what some other researchers did was "unscientific". They even go so far as to dismiss entire disciplines, to discount their validity and declare them to be beyond the pale. Physicists are especially prone to doing this, and ecology is one of their common victims. This is the characteristic jostling and name calling of the

\textsuperscript{13}Sal Restivo of Rensselaer Polytechnic Institute has assisted me in coming to this formulation.
competitive marketplace and as I have said before, I do not intend to contribute to it here. In fact I consider the hazing ecologists receive to be an indication that they are scientists, for such behavior is wholly typical of the profession and no-one would bother to criticize ecologists if they were not very much a part of the scene. So far as I am concerned, anyone who performs controlled experiments or systematic observations in the light of some hypothesis, and must participate in the justificatory politics of the scientific profession, is a scientist.

I consider the question at hand to be: can a person who accepts the agenda, the literary form and the vocabulary that I have used here, go into a laboratory and conduct a successful inquiry into what is going on there? By "successful" I mean: can this person produce a coherent account of what was done there and how that resulted in what was known? I believe that some persons could, for two reasons.

The first is that scientists really do have a great deal in common, for the world of their profession is highly standardized. The standardization of scientists' experience begins in childhood, most obviously when they enter the school system, and is extremely advanced and refined by the time they reach university. If you look through introductory textbooks in calculus, genetics, or physics that were written at roughly the

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same time in different countries around the world you will be struck by how uniformly each subject is treated. Science students all over the world do similar practice experiments, learn the same factual material, articulate the same theories. University acculturates them.

Wherever the social relations of production of modern industrial society have been established the institutional culture of science has taken root. The material culture, the highly standardized laboratory equipment that is advertised in Science or Nature, is available world wide. Scientists use it to obtain standardized representations of highly standardized events which they must assemble into highly rationalized accounts. The formats of articles and in-house reports, of conferences and grant applications are well-known and, with variations, almost universal.

Universities, research institutes and the other bureaucratic institutions that house and otherwise support scientific research do not differ very fundamentally from one another or from government ministries or large corporations. A bureaucracy is a bureaucracy. It is concerned with rational administration and with extending centralized control. The very large growth of universities in recent decades was primarily a response to the huge proliferation of governmental bureaucracy and the agglomeration of corporations, which needed increasingly large numbers of suitably conditioned people for middle management and other professional functions. Scientific research
is conducted in niches of a world that is pre-eminently concerned with control, regulation and prediction at all levels of experience. It provides the materials and the bare bones of the context for researchers, subject to certain conditions, from which they can build their research programs.

This brings me to the second argument for the generality of my account. Most scientists have to deal with essentially the same general institutional conditions but within universities, at least, they must act as intellectual entrepreneurs. They must procure what the institution has to offer, set up, run and promote their research programs. They study a remarkable diversity of things, using a similar diversity of techniques and vocabularies. Each research project must be original, something not done before. Naturally, each lab is very different from every other lab, and so to study research you must be able to account for this diversity. Insofar as dealing with the particularity of experience is concerned, the generality of my approach rests on the way I have discussed location, decisions and consequences, involvement, and continuity. Specifically, I have tried to direct attention to the ways in which professional scientific inquirers create these qualities of their working lives.

My portrait of researchers is rather wooden compared to a novel, for instance. I have not probed into these persons' lives that far, or emphasised their individuality that strongly. After all, this is a form of scholarly analysis. I have been concerned
with the kinds of relationships between deer, researchers, and
the researcher's peers that institutional life permits and
encourages. I have shown these scientists as agents within a
bureaucratic system for it is what they have in common with
other scientists that concerns me most. I should also point out
that there is a question of privacy here. Williamson invited me
to study him as a scientist. His family life and other such
things are not your business. Furthermore, within the orthodox
view of science, discussions of such things are used as a way of
"humanizing" the image of the scientist. This actually has the
effect of sidestepping the issue of how research is actually
done and what controls it, because "humanity" is not
systematically and continuously connected to the research
process, within the orthodox view.

The self-consciousness of my account has been, among other
things, an attempt to underscore the fact that each situation is
unique, that the social scientist has to become involved with it
and be changed by it in order to understand it. It is only a
version of bureaucratic arrogance to expect that it could be
sufficient to stand back and apply some formula by which all
scientists should be known in future, to act as if understanding
people requires that they be reduced to clones. The persons that
an inquirer studies are active participants in their own lives.
This must be an important feature of the kind of account I
prefer, and the person who aims to understand what his
informants are doing in this way cannot exempt himself from this
consideration in explaining his own presence in the study.

Ultimately, the general applicability of my account has to do with its methodological character. Its primary concern is inquiry itself, with how inquiry is conducted. And although it has dealt with scientists, I think that a closely related approach would have been equally useful if I had been studying carpenters, housewives, lawyers, or children in a day-care centre. Academics tend to claim that they define what legitimate inquiry is, and they have taken over the field of human intelligence in much the same way that doctors have taken over health, with much the same results. But all of the other people I just mentioned also think, and most of them think more clearly than they are generally given credit for. Inquiry is a vital, central part of human life. Individuals' attempts to understand themselves and their surroundings, and to decide what should be done, are very important determinants of what they do and what happens in a society. A person's efforts to provide a coherent centre and a direction for his life are of the essence of his humanity. Any attempt to account for what people do, and for their relations to others and to society at large, must take account of the ways they make sense of and direct their lives. It is a crucial failing of our dominant intellectual traditions that they do such a poor job of this, whether by focussing on the individual too exclusively, or by treating society as a transcendental entity.
The case of scientists and of professionals in general is especially important because of the place of public justification in their lives. The institutions which require this justification dominate our society, and Science has an important place in these institutions and in the power relations of our society because it is an exemplar of legitimate knowledge. Therefore it is particularly important to establish a critical perspective on the way scientists account for their experiences.
Appendix 1: Tentative summary of research arrangements, handed out in May, 1977.

A schedule of events has been suggested for the current Deer Study Programme. Although it has been fixed with respect to days of the week, it is flexible and will be changed once the study females give birth to fawns. This schedule should be adhered to during the following periods:

1. Introductory trial observations;
2. Weekly during the first four weeks of intensive study, (Phase One);
3. Bi-monthly during the second phase of the study; and,
4. Monthly thereafter until the end of lactation.

Dates for these periods will be circulated following the birth of the study fawns.

The schedule currently envisioned is as follows:

<table>
<thead>
<tr>
<th>Day of Week</th>
<th>Feed Intake</th>
<th>Study Function</th>
<th>Numbers of People</th>
</tr>
</thead>
<tbody>
<tr>
<td>Monday (A.M.) Pellets</td>
<td>a) Measure Out Pellet Ration</td>
<td>2</td>
<td></td>
</tr>
<tr>
<td>Alfalfa Pellets</td>
<td>b) Bleed</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>c) Milk ???</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>d) Weigh fawns</td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>e) Reintroduce fawns to Does</td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>f) Clean pens</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Start 10:00</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Tuesday (A.M.) Pellets</td>
<td>a) Measure food consumption</td>
<td>6</td>
<td></td>
</tr>
<tr>
<td>Alfalfa pellets</td>
<td>b) Measure out daily food ration</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>c) Uncontrolled 24 hour observation on suckling activity</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>d) Weigh fawns</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>e) Do not clean pens</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Start 10:00</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Wednesday (A.M.) Pellets</td>
<td>a) Measure food consumption</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>b) Measure out daily food ration</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>c) Controlled 24 hour observation to measure milk consumption</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Day of Week</td>
<td>Feed Intake</td>
<td>Study Function</td>
<td>Numbers of People</td>
</tr>
<tr>
<td>-------------</td>
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<td>----------------</td>
<td>-------------------</td>
</tr>
<tr>
<td>Wednesday - Continued.</td>
<td></td>
<td>d) Weigh fawns</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>e) Do not clean pens</td>
<td></td>
</tr>
<tr>
<td>Thursday</td>
<td>Pellets</td>
<td>a) Weigh mothers at 10:00 A.M.</td>
<td>2</td>
</tr>
<tr>
<td></td>
<td>Alfalfa pellets</td>
<td>b) Measure food consumption</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Alfalfa hay</td>
<td>c) Clean pens</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Browse (occas.)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Friday</td>
<td>Pellets</td>
<td>a) Clean pens</td>
<td>1</td>
</tr>
<tr>
<td></td>
<td>Alfalfa pellets</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Alfalfa hay</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>browse (occas.)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Saturday</td>
<td>Pellets</td>
<td></td>
<td>1</td>
</tr>
<tr>
<td></td>
<td>Alfalfa pellets</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Alfalfa hay</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sunday</td>
<td>Pellets</td>
<td>a) Take fawns away from Does at 8:00 P.M.</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Alfalfa pellets</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Alfalfa hay</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

This schedule will only apply to those five females and their fawns under investigation.

Additional studies on milk deprivation and growth weight changes will commence in July on alternate weeks during phase 2, and a schedule of duties will be forthcoming at that time.
Methodology for the suckling activity and milk consumption studies are presented below. The methodology must be standardized by all observers to assure that accuracy is maintained throughout the duration of the study. This shall be accomplished during an introductory trial period preceding the actual studies envisioned for phase one.

General

Two observers will be utilized for each four hour observation period. Each observation period will be followed by an eight hour rest period. This schedule will be maintained over a continuous 48 hour period. During the first 24 hour period observations will be made on uncontrolled suckling activity in fawns. During the second 24 hour period the fawns will be removed from their Does and will be reintroduced to the Does at the specified times to determine milk consumption.

I. Suckling Activity Study

During the continuous 24 hour period of observations each pair of observers on each four hour shift will be responsible for recording events associated with suckling activity in fawns. One observer will maintain a watch on 3 Does and the other will maintain a watch on 2 Does.

Each observer will be equipped with a chart and a stop watch for each fawn. Events will be recorded, on the charts provided. Following the first 4 weekly periods of observation, it is anticipated that an event recorder would be utilized to facilitate the use of fewer observers. However, this tool will only be used should it prove feasible for such an application.

Data recording will consist of the following observations on each fawn.

1. Time of day (24 hour clock), suckling activity was attempted;
2. Duration, in seconds, of each suckling attempt;
3. Doe initiation of suckling attempt;
4. Fawn " " " ;
5. Doe termination of " ";
6. Fawn " " " ;
7. Success/unsuccess of suckling attempt;
8. Orientation of fawn to Doe when suckling attempted (from right or left flank, or from the rear);
9. Position of the Doe while nursing her fawns (standing or laying down); and
10. Fawn's ingestion of other items (pellets, water, feces), other than milk.
The following definitions and abbreviations will be used:

1. Suckling
   - An attempt by fawns to draw milk from the Does udder.
   - preceded by udder bunting
   - initiated when fawns mouth is placed on the teat.
   - terminates when either the fawn removes its mouth from the teat, or when the Doe removes the teat from the fawns mouth.

2. Suckling Start
   - The time of day (24 hour clock) when suckling attempt was initiated.

3. Suckling Duration
   - The actual time (in seconds) each fawn spent suckling on the teat.

4. Doe Initiation of Sucking Attempt (Nursing)
   - Done in various ways.
   i) Vocalizing (V) - Doe will bleat to distract fawn from sleep or other activity.
   ii) Walking (W) - If resting, the Doe will rise, stretch and move about in such a manner which attracts the fawns' attention and, if hungry, the fawn will approach the Doe and attempt to suckle.
   iii) Licking (L) - Prior to nursing a fawn, a Doe will begin to vigorously lick her fawn. If hungry, a fawn will respond by attempting to suckle. Often licking concentrates around the anogenital area.
   iv) Disturbances - All the above may be triggered by a stress situation either by an internal disturbance (I.D.) caused by other animals or the observers in the corral, or by an outside disturbance (O.D.) caused by people, dogs or other animals or sounds in the vicinity of the compound.

5. Fawn Initiation of Suckling Attempt (F.I.)
   - Done in various ways.
   i) Vocalizing (V) - A fawn will occasionally bleat either due to stress or in response to vocalizations or its sibling or other fawns in close proximity to it.
   ii) Teat Seeking (T.S.) - A fawn if hungry, frightened or disturbed will seek out the Doe and attempt to suckle. This may also occur as a result of licking (L), walking (W) or vocalizing (V) by the Doe, or as a result of vocalizing (V) or tail wagging (T.W.) by a sibling.
ii) **Tail Wagging (T.W.)** - Occasionally a fawn may be stimulated to arise from a resting position due to a visual stimuli received from a tail wagging of its sibling while the sibling is actively engaged in suckling.

   i) **Walking Away (W.A.)** - Doe will lift her hind leg and step away from the suckling fawn.
   ii) **Pushing Away (P.A.)** - Doe will push the suckling fawn away from the teat with either her head or her hind foot.
   iii) **Disturbances** - Doe will either walk away from (W.A.) or push away a suckling fawn due to occasional disturbances of either an internal (I.D.) or outside (O.D.). Differentiation between internal and outside disturbances can be denoted by the doe's head and ear positions and by her line of vision.
   iv) **Sibling Rivalry (S.R.)** - Occasionally the Doe will terminate the suckling of both fawns when the fawns become too aggressive on the udder.
   v) **Lying Down (L.D.)** - Frequently the Doe will lay down in an attempt to discourage her fawns from suckling.

7. **Fawn Terminates Suckling (F.T.)** - Occurs in numerous ways.
   i) **Walking Away (W.A.)** - Once satiated the fawn will simply walk away from the Doe.
   ii) **Break In Suckle (B)** - The fawn will not change the position nor will the Doe, but the fawn will remove its mouth from the teat, breath or belch, and resume suckling again. Usually this pause lasts a few seconds.
   iii) **Sibling Rivalry (S.R.)** - When two siblings are competing on the udder for a teat, one fawn may discourage the other from suckling. This is normally done when both fawns attempt to suckle from the same flank and one fawn attempts to gain positional dominance over the other.
   iv) **Dropping Down (D.D.)** - Occurs when the fawn's suckling is interrupted by an immediate disturbance causing the fawn to react by crouching on the ground. The Doe may or may not move away from her nursing stance.
   v) **Disturbances** - May be from either an internal (I.D.) or outside (O.D.) source which may be denoted from head and ear orientation of fawn.

8. **Incidence of Successful** (Succ.) **and Unsuccessful** (Uns.) **- Observations on suckling activity by other researchers have noted behavioral characteristics which may be used in this study as indicators of successful suckling.
   i) **Udder Bunting** - The fawns must physically bunt the udder to facilitate the "let-down" of milk from the udder. Should this not occur, when one fawn is suckling, the suckling attempt shall be considered
unsuccessful. Milk "let-down occurs about 30 seconds after bunting begins.

ii) **Posture of the Doe** - When successful suckling occurs, the Doe may assume a distinct suckling posture with the front and rear legs slightly extended and the head and neck slightly lowered. Frequently the Does countenance reflects a blissful and tranquil expression.

iii) **Tail Wagging** - Has been reported as an indicator of suckling success in young fawns, although not fully substantiated it may serve as a stimuli for the sibling to begin suckling.

iv) **Licking of Anogenital Area** - Occasionally, but not always, the Doe will lick the anal area of the fawn while nursing.

9. **Orientation of Fawn to Doe when Suckling** (Orient.)

   i) From **Right Flank** (R.F.)
   ii) From **Left Flank** (L.F.)
   iii) From **Rear** (R.)
   iv) **Lying Down** (L.D.) - Doe is lying down while fawns attempt to suckle.
   v) Normally L.F., R.F. and R. Positions occur when Doe is standing.

10. **Fawn Intake Other Than Milk** (Intake)

    Intakes are designated as follows:

    i) Fawns eating Pellets (P)
    ii) " " Feces (F)
    iii) " drinking water from Buckets (W-B)
    iv) " " " " Puddles (W-P)

II. **Milk Consumption Study**

    The procedure for determining milk consumption at each suckling will be attempted by weighing fawns before and after suckling in order to determine milk mass gained.

    This will be done by separating the fawns from the Does during a 24 hour period and controlling their access to Does at the approximate times as observed during the preceding 24 hour watch.

    Fawns will be removed from their holding area prior to the time of introduction. Their anogenital areas will be stimulated with cotton batten dipped in warm water to ensure defecation and/or urination prior to weighing. Once fawns have either defecated and/or urinated they should be weighed, and both siblings introduced to the Doe. Individual fawn weights shall be recorded.
The suckling activity shall be observed and, once completed, the fawns shall be removed immediately from the Doe and re-weighed. Should a fawn defecate prior to removal, the feces should be collected and weighed with the fawn. Individual fawn weights shall be recorded.

The fawn shall then be placed in the holding pen to await its next suckling period.

III. Fawn Feces Production Study

During the 24 hour period when milk consumption is being determined, all fawn feces must be collected, bagged, wet weighed and labeled with fawn number(s) and dated for future calorimeter analysis in the lab. This should not be too difficult to collect as the pens will have clear Plexiglass doors to facilitate the observation of defecation occurrence. Feces should be collected as soon as possible to ensure accurate wet weight determinations.
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