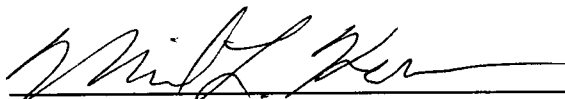
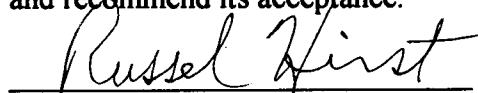


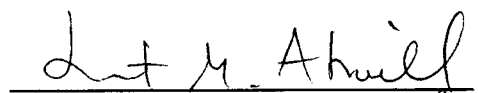
To the Graduate Council:

I am submitting herewith a dissertation written by Elizabeth Stouse Weatherford entitled "Invention and the Rhetorical Situation: Disciplinary Dynamics and the Adoption of Soil Conservation Policy." I have examined the final copy of this dissertation for form and content and recommend that it be accepted as partial fulfillment of the requirements for the degree of Doctor of Philosophy, with a major in English.


Michael L. Keene, Major Professor

We have read this dissertation
and recommend its acceptance:







Accepted for the Council:


Associate Vice Chancellor and
Dean of The Graduate School

**INVENTION AND THE RHETORICAL SITUATION:
DISCIPLINARY DYNAMICS AND THE ADOPTION OF
SOIL CONSERVATION POLICY**

**A Dissertation
Presented for the
Doctor of Philosophy
Degree
The University of Tennessee, Knoxville**

**Elizabeth S. Weatherford
August 1997**

Copyright @ Elizabeth Stouse Weatherford, 1997

All rights Reserved

DEDICATION

This dissertation is lovingly dedicated to my family-

my parents, L.E. and Katherine Stouse,

my children, Alice, William, and Emmett Weatherford,

and my partner in all, James William Weatherford.

ACKNOWLEDGMENTS

To Dr. Michael Keene, who persisted against all odds in supposing I would finish this dissertation, I wish to acknowledge an immense debt of gratitude. If it had not been for his guidance, support, good humor and all around good will, I could not have kept working. I would like to thank the members of my committee, Janet Atwill, Russell Hirst, and Dan McLemore for their help in reading and commenting on my work, for their tolerance of a hybrid content that follows unfamiliar paths, and for the generous encouragement and support they have given me for the duration of this project. The research for this dissertation has been carried out on two continents, so the patience and professionalism of librarians literally around the world has been taxed for this project. I would especially like to thank Margaret Casado, Marie Garrett, Bobbie Painter and the staff of the Off-Campus Library Services of John Hodges Library, at the University of Tennessee, Knoxville. In Western Australia, Valerie Blake, Jacqui Hanlon and Sharlene Larson of Muresk Institute, Curtin University, gave their unstinting help, as did the staff of the library of the Department of Agriculture, Western Australia, Perth. I would also like to thank Douglas Helms, National Historian of the Soil Conservation Service for making available many hard-to-find documents and for providing general guidance on the project; I also thank Dr. Stanley Buol of North Carolina State University for helping me with questions about soils and soil processes.

Thanks also go to my parents L.E. and Kate Stouse for their support and encouragement; thanks to mom and dad for so many things, too numerous to contemplate. To Alice, William, and Emmett, I am grateful for helping me keep a sense of humor and a sense of perspective, and for sending their love my way even when I didn't have time to spend with them. Finally, I would like to thank James Weatherford, my husband, my beloved, and my best friend, for the material, spiritual and moral support he did not hesitate to give, throughout the months and years that have been devoted to this project. Thank you, and thank you, everyone.

ABSTRACT

When science policy is adopted as a community goal, it is in the context of a rhetorical situation. This study examines how rhetoric and rhetorical principles, such as contextuality, authority and ethos, were integrated in the making of the knowledge about soils and it examines the elements of rhetorical situation: exigency or problem, audience, available arguments, or what can be said, etc. Because soils are extremely complex, being formed by the interaction of the factors of parent material, topography, biota, climate, and time, the most basic principles of soil processes and management escaped the notice of scientists when any of these factors were ignored or assumptions made about them. This was the case when soil science began in the US, their assumptions coming from their training in Europe, where rainfall (climate) much softer than was the rule on the continental US. By the mid-1800s, very bad agricultural conditions in much of the South indicated the effects of climate, acidic parent material, and cultural practices. After the war conditions were worse; tenancy and land abandonment were greater, degradation and erosion worsened. After 1862 the state land-grant colleges provided opportunities for scientific education. The Department of Agriculture was greatly expanding in the early 1900s, and a survey of soils in the US was also begun. The soil survey together with the soil science at the land-grant colleges created a situation in which the conditions on land in the South was seen by more trained observers (the surveyors) and the conditions were also recorded. At one and the same time, in the early 1900s, the soil survey and the land grant colleges were developing 1) a potentially effective audience, 2) the logos, or sayables, about the extent and degree of erosion damage, and 3) scientific rhetors who had the experience, knowledge, and commitment to see recognize the problem of erosion. These scientists, led by H..H..Bennett of the survey, also helped further raise awareness among the public andamong legislators. The rhetorical situation was complete when dust storms blew soil from Oklahoma, Texas, and Colorado into Washington, and conservation was established as a policy goal.

TABLE OF CONTENTS

INVENTION AND RHETORICAL SITUATION: DISCIPLINARY DYNAMICS AND THE ADOPTION OF SOIL CONSERVATION POLICY

INTRODUCTION

1. RHETORIC OF SCIENCE IN ITS CULTURAL CONTEXT

1.1. THE THEORETICAL CONTEXT	9
1.1.1 Modernity and its Legacy	10
1.1.2. Reaction against Modernity.....	12
1.1.3. Rhetorical Situation.....	13
1.2. RHETORICAL FACTORS IN THE ADOPTION OF SOIL CONSERVATION POLICY.....	16
1.2.1. Rhetorical Dimensions of Soil Studies	16
1.2.1.1. Rhetorical Elements in Soil Science–Language Issues	17
1.2.1.2. Soil Science is Rhetorical in its Orientation.....	21
1.2.2. The “Discovery of Time” and Perception of Rhetorical Situation.....	22
1.2.2.1. Rhetorical Setting for Soil Science in the U.S. at the Turn of the Century..	27
1.2.2.2. The Disciplinary Setting.....	29
1.2.2.3. The Focus of this Study.....	30
1.3. QUESTIONS ABOUT RHETORIC AND THE RHETORIC OF SCIENCE TO BE ADDRESSED	29

2. THE SITUATIONAL CHARACTER OF EARLY SOIL STUDY

INTRODUCTION.....	37
2.1. PRE-PARADIGMATIC AGRICULTURAL SCIENCE	38
2.1.1. England’s Agricultural Revolution–Technical and Management Innovations ...	45
2.1.2. Discoveries and Advances in Chemistry and Scientific Technology.....	51

2.2. THE NEW PARADIGM: AGRICULTURAL CHEMISTRY.....	61
2.2.1. Leibig And Nutrient Cycles: The Setting For Change	61
2.2.1.1. The Theory of Nutrient Cycling	68
2.2.1.2. Limitations in Leibig's Theory.....	73
2.2.1.3. Leibig Criticized on Grounds of Violation of Scientific Ethic	77
2.3. LEIBIG'S LEGACY TO THE SCIENTIFIC COMMUNITY	79

3. AMERICAN SOIL STUDIES: CONTEXT AND CONTENT

INTRODUCTION.....	85
3.1. SOIL MANAGEMENT PRACTICES IN NORTH AMERICA.....	84
3.1.1. Exploitation and Abandonment of Lands.....	84
3.1.2. Beginning of Interest in Conservation.....	89
3.2. SOIL SCIENCE STUDIES IN THE UNITED STATES: INSTITUTIONS AND OBJECTIVES	91
3.2.1. Bureau of Soils: Setting and Activities	91
3.2.2. Contribution/Influence of Geology in Soil Science	93
3.2.3. Rapid Government Expansion	97
3.3. SURVEYS IN THE BUREAU OF SOILS	101
3.3.1. Claims Advanced by the Bureau under Whitney	105
3.3.2. Reasons for Opposition to Whitney	111
3.3.3. Reasons for the Heatedness of the Controversy	123
3.4. WHITNEY'S BUREAU OF SOILS	129

4. THE SOIL EROSION ISSUE: RHETORICAL SITUATION AND INVENTION IN THE 1930S

INTRODUCTION.....	136
4.1. BENNETT'S ROLE.....	137
4.1.1. The First Phase: Bennett Identifies Sheet Erosion	138
4.1.2. Shaler and the 1908 Conference of Governors: Confirmation of a Commitment	148

4.1.2.1. The Prospect of Progress.....	148
4.1.2.2. The Outcome: Indifference in Congress and in the Bureau of Soils	155
4.1.2.3. Reasons for the Lack of Concern about Erosion in the Bureau of Soils	159
4.1.3. Bennett Works Towards his Goal	168
4.1.3.1. Bennett's Contribution to Classification:	
Distinctions Caused by Erosion	169
4.1.3.2. Emphasis, Awareness, and Discourse Space for Erosion	173
4.1.3.3. Bennett's Work in the Bureau 1911-1929: <i>Logos</i> and <i>Ethos</i>	180
4.2. SITUATIONAL FACTORS: THE DUST BOWL, END OF THE FRONTIER, AND THE DEPRESSION	188
 5. SCIENTIFIC KNOWLEDGE, SITUATIONAL CONTEXT, AND POSSIBLE ACTION	
5.1. REVIEW OF THE STUDY.....	197
5.2. CRITICISMS OF BENNETT AND THE SOIL CONSERVATION MOVEMENT	202
5.3. RHETORIC OF SCIENCE IN THE RHETORICAL SITUATION OF SOIL CONSERVATION POLICY.....	206
 BIBLIOGRAPHY.....	 212

INTRODUCTION

Rhetoric is the art of finding the available arguments in any case. A true rhetor must be a person of integrity, because the use of rhetoric requires the integration of knowledge, ethics, and fellow-feeling, the Greek *logos*, *ethos*, and *pathos*. Aristotle attributed a “strong and important position” to rhetoric because it is “through the instrumentality of [that] art that truth and justice are able to realize themselves through the decisions of men” 39(Grimaldi 1978: 173). Richard McKeon called rhetoric an “architectonic art,” meaning that it is a productive, systematic art. It embraces each phase of the process of civic decisionmaking or problem-solving from first insight, discovery, or conceptualization, to finding what arguments are possible given the components of a situation, to configuring the arguments and finding the appropriate tone and vocabulary to present the arguments. These processes do not take place hierarchically and chronologically from the first level down to the last, but rather the finding of suitable arguments is recursive, meaning that it must continually be adjusted to the particulars of context—the audience and the situation—and incorporate new knowledge which can also change with the situation.

Rhetoric is a form of practical reasoning, utilized when “the thing wanted is *at a distance* from the immediate action” (Gross 1990: 5). Rhetoric is “reasoning together,” and is revealed as epistemic when “the thing wanted” is a concept or a new understanding (Scott 1967). Decisions on the conduct of social values, law, and self-governance were the realm of rhetoric among 6th century BC Greeks; thus it was conceived as central to the conduct of democracy. Often “things wanted,” such as security for dwellings, a better harvest, or a community facility, were at a distance logistically or in time or both. Rhetoric was the instrumentality for making the decisions to take the action necessary to secure the desired changes. Stephen Toulmin gives the name *futuribles* to the better futures that can be made by community planning, planning being the closest contemporary equivalent to “civic virtue,” which the Greeks associated with rhetoric.

We still need rhetoric to make such choices and to examine our status relative to the threats and possibilities facing a self-governing nation. Now many of the decisions that are made by communities have to do with science and applications of science. These science policy decisions have been argued on the basis of scientific knowledge, and because science is now so powerful, we may not know the effects of an application of science until it has had unanticipated effects. For that reason, scholars with an interest in rhetoric and in science have yoked the analytical theory of rhetoric to science, as "rhetoric of science," the better to explore the making of science and the social and goal-oriented aspects of science. It is with the purpose of reaching a better understanding of how science and rhetoric can jointly contribute to better futures that this rhetoric of science project is undertaken.

The policy decision examined in this study is the adoption of soil conservation, which was intended to translate into action the concern of the public and legislators over the level of soil management in the US. The basis of this concern was that, for much of the time roughly from the 1870s to the 1930s, agriculture in the US was beset by serious problems, and declining soil fertility was one of the most serious. Despite important technical advances in agriculture over that period, yields remained stagnant. Although the causes of decreasing soil fertility were complex, some soil scientists directed attention towards erosion as a critical factor in this problem. Consequently, in the late 1930s and early 1940s in the US, the double-sided issue of soil erosion/soil conservation became prominent in public discussion and legislation. In 1935, legislation was passed establishing the Soil Conservation Service, the first such service in the world, with Hugh Bennett, the first director of the Service, widely credited with playing a critical part in its establishment.

A high level of public interest and awareness of soil management for conservation was sustained until the second World War, and during this period unprecedented

implementation of soil conservation projects was carried out.¹ The federal soil conservation effort supported the establishment of permanent pastures, strip cropping, soil conserving rotations, afforestation of hilltops and steep slopes, and adequate fertilization, in integrated plans for farms and even entire watersheds. Directly and indirectly, federal soil conservation measures contributed to the economic recovery of rural communities throughout the country. Soil conservation was good for the land and good for society—for both together.

This event is all but forgotten now, but decisions made and actions taken at that time have continuing and possibly greater influence today. Americans who are aware of the soil conservation efforts of those decades would think of it as a successful scientific event. And while they would surely agree that conservation was a material success and a scientific success, most would not think of it as a rhetorical success, because our conventional understanding of the two fields has been that science has nothing to do with rhetoric. But if there had been no conjunction between rhetoric and science, how were legislators persuaded to enact this science policy and to fund its implementation? And how did soil scientists justify hazarding their professional credibility to advocate for soil conservation? And finally, how did these scientists pose hypotheses to themselves about problems in soils without applying both practical reasoning (rhetorical logic) and general questions about the nature of things (dialectic) to their experience with soils and their capabilities? Rhetoric, properly understood as a problem-solving and decision-making agency, can help provide the answers to these questions, because it is through the offices of rhetoric that decision, or *krisis*, is reached in problems like these.

¹ Monographs inspired by the soil conservation movement may serve as a measure of this public interest at the time. They numbered in the hundreds and spanned from historical and academic books to farm or engineering guides. Some of these are Carter (1949), Carter and Dale (1974 [1955]), Graham (1944), Hall (1937), Jacks and Whyte (1972 [rpt.]), Lowdermilk (1953), Mickey (1945), Sears (1945, 1969) Chapman, Fitch, and Veatch (1950), Ayres (1936), Burges (1936).

In examining this soil conservation event, our purpose is to look for guidance in several areas, all related to what science can and cannot do and what rhetoric must be developed to do in natural resource decision-making. To understand why the decision to adopt a soil conservation policy was taken requires that soil conservation be examined in its setting, that arguments in soil science be regarded as situated, and adherence as contingent on context. This entails drawing out the features of the event's setting that resulted in its being adopted when and how it was. This in turn will entail exploring the development and concerns of soil science—more accurately the soil sciences.

Rhetorical analysis traditionally addresses problem or issue, arguments, audience, rhetor, and occasion. But if invention is the core of the New Rhetoric, inquiry should emphasize factors related to invention, such as, What features of situations have elicited what kinds of rhetorical invention in the soil sciences, and what topics, common and special, or disciplinary, were available at what stages? We can ask about the event of the adoption of soil conservation policy: What were the prominent features of this situation, what were their characteristics, and did they evoke different responses from different participants? Then regarding the subject of debate: What place has rhetoric in the soil conservation question or problem? Then in the area of participants—rhetors and audience: What non-rational aspects of knowledge-making are apparent in the development of soil science's contribution to the decision? What actors were most involved in rhetorical purposing, and, again, why? The most obvious question is: What created the necessary level of adherence to the call for conservation to trigger its adoption at last? In asking these questions, we can also test some of the views brought forth in the last decades by new rhetorical theory.

The first chapter of the study will discuss rhetoric as an essential participant in cultural and academic discourse, reviewing the relevant literature in rhetoric and rhetoric of science (or rhetoric of inquiry), exploring key terms and concepts, and describing rhetoric's territory and functions and the analytical strategies it provides. It will briefly cover the means whereby it has been revived (historical research in rhetoric and corroborating

developments in other disciplines) and the reasons for its revival (a situation perceived by New Rhetoricians, among others, requiring the influence of reasoning with humane values). It then addresses the significance of rhetorical situation, particularly for invention, and offers a general introduction to soil conservation issues in the context of rhetoric, covering briefly the rhetorical situation at the time, that is, soil management problems that presented in the US in the 19th century, and giving a rough sketch of their demographic and socio-cultural setting. When possible, rhetorical issues will be illustrated with examples from soil science, and the debates in rhetoric and rhetoric of science to which this study could contribute will be introduced.

Chapter 2 explores the articulation of *topoi* and problem solution in two soil studies, in England and in Germany in the 18th and 19th centuries. It also looks, in particular, at why the interests and approaches of these streams were so incommensurate, relating their differences to differences in socio-demographic and agronomic contexts. The chapter also compares the development of knowledge in these research areas with some components of rhetoric's claim that "scientific knowledge is constructed rather than discovered," by following out influences of culture or setting (sources of authority, assumptions, differences in physical setting).

Chapter 3 explores the dynamics in the soil science community in the US in the early part of the 20th century. The soil management situation in North America, and various conditions affecting soil management and soil science there, are examined briefly as background for understanding the continued deferral of campaigning against soil exhaustion and erosion. Chapter 4 examines the factors bringing about the policy enactment and the setting in which these factors coalesced; all of the conventional rhetorical factors—rhetor, audience, subject, occasion, speech—were involved. We look at the part Bennett and other soil scientists played, at the arguments that succeeded, and at the readiness of the audience and other elements of the rhetorical situation, all of which

combined in such a way that the discourse for establishing soil conservation policy was successful.

Chapter 5 discusses long-term benefits of Bennett's work as well as the basis of criticisms of his approach. It also discusses long-term effects of the soil conservation crisis.

Conclusions will be drawn on how the examination of rhetoric in soil conservation contributes to a resolution of the conflict of positions on such issues in rhetoric of science as the status of scientific facts and the role of the rhetor in creating a rhetorical exigency. The chapter considers what elements of the arguments presented for soil conservation were uniquely rhetorical, and why they offered sufficiently good reasons to bring about the choice for conservation policy. It also suggests courses of action or adjustment of direction that these considerations support. Answers to these questions may provide insights for our present urgent need to make good science policy judgments in natural resource use.

The benefits of this research are in three areas: rhetoric studies, natural sciences and policy or decision-making. A first contribution in rhetoric is that it extends and contributes to the work in rhetoric of science, thus further helping to bridge the gulf between rhetoric and science. Many of the rhetoric of science studies already undertaken deal with more abstract scientific events or discoveries. This case is based on science about a resource essential for human survival and one affected by human use: how is it different from other cases and are there guidelines that can be distilled from it? This research also provides an opportunity for examining how well the claims and goals of rhetoric of science match the part rhetoric played in this case: are the claims made for rhetoric justified? Finally it weighs in on differences in views, notably that between the "hard" and the "soft" versions of science in rhetoric of science research.

This study may provide benefits applicable to the natural sciences by making new connections between soil conservation, a successful science policy experience, and

rhetoric. This connection might make new insights available for consideration of current natural resource controversies, such as questions of ethical land use principles in "takings." Whatever useful ideas can be gleaned for natural resource questions may have to do with values, and why they must be highlighted. In research, for example, what findings about values might apply to the choice of subjects to study in the natural sciences? Can we find broadly shared "good reasons" and values that attract the adherence of a universal audience in the life sciences?

Finally this study may provide useful insights in the connections between rhetoric and policy for natural resource use. One obvious relevant question to be considered is whether rhetorical principles should be intentionally and explicitly promoted as a means of analysis and an agency of decision-making. The study should also suggest ways in which rhetoric can contribute to policy supporting scientific research or provide implications in other policy areas. In plainer parlance, we want to (1) get a better grasp of how and why rhetoric serves as a power or capacity to bring about desirable choices for the future as an instrumentality of a democracy, and (2) how we can learn to make better use of it.

The primary aim of the study is to promote the rapprochement of science and the humanities and the integration of human values in the use of natural resources and in technology choice. This focus reflects the timely concerns of the New Rhetoric to address issues of cooperation and communication, responsibility and community. The challenge of the New Rhetoric is to restore rhetoric to serve for the self-governance of a diverse people in a technological age, in a context of increasing pressure on natural systems and communities. This is the core benefit because a rhetorical orientation will give us the future focus necessary to see the importance of addressing difficult decisions head-on, rather than "backing into the future." With rhetorical competence as an efficient cause, matching available good reasons, both factual and ethical, to an appreciation of the context of global natural systems, we will be able to make informed decisions regarding natural resources, thus serving the final cause of a sustainable society.

CHAPTER 1

RHETORIC OF SCIENCE IN ITS CULTURAL CONTEXT

1.1. THE THEORETICAL CONTEXT

"The rhetoric of inquiry," so called by John Nelson in *The Rhetoric of the Human Sciences*, describes the exploration and examination of reasoning in the academic disciplines (1989). The robust development of the rhetoric of inquiry, and especially the rhetoric of science, in recent years seemingly confirms the claim of rhetoric to the need for a revival of that 2,500-year old art. Central to the revival of rhetoric, and acknowledged in the expression "rhetoric of inquiry," is the readmission of rhetoric's core feature, invention, to the canon of its functions. Two possibilities arise from this recognition that invention is central to rhetoric and critical to a robust practice of the art of rhetoric: scholars in rhetoric and many other fields are undertaking what is essentially rhetorical inquiry into the making of knowledge in diverse fields in the sciences and human sciences; and primary rhetoric can be reincorporated into the practice of civic decisionmaking.

Rhetoric of science or rhetoric of inquiry brings the questions and methods of rhetoric to the analysis of science, thus yoking science and rhetoric—the "god-term" and the "weasel word," as Richard Weaver called them. But Richard McKeon and others argue that the sciences have borrowed and incorporated the inventional methods proper to rhetoric,² and thus have achieved this high status at the same time that invention for political rhetoric had been denied to rhetoric itself—excised from rhetoric, in theory, in the 1600s and, in practice, far earlier by the impossibility of practicing political rhetoric freely under authoritarian regimes. Under these conditions, rhetoric slipped from a politically active, "primary" state into a devitalized and largely literary use, or "secondary" rhetoric, and so

²McKeon (1987 [1942]; 1987 [1971]); Ong (1958), Ede and Lunsford (1984), Halloran (1978), Miller (1983), Miller and Selzer (1985).

into its debased estate as “mere rhetoric.”³ Thus the concurrent rise of Modernism and of science took place largely in environments in which centralized power rather than deliberation determined the uses to be made of science.

Now, because of negative downstream effects of science as managed under Modernism and the associated Rationalism, science has lost much of its lustre, and the epistemic and civic uses of the powers and functions of rhetoric to analyze and to advocate are again becoming apparent. Rhetorical analysis as undertaken in rhetoric of science is being recognized as valuable both in understanding the part played by rhetoric in other disciplines during the term of its official eclipse, and in developing its capabilities for participation in future decisionmaking. The intent for this study is to make a contribution in these respects. Before introducing our study of rhetoric in soil science/soil conservation, we should examine a bit further the characteristics of the Modernist view, because this view establishes part of the background against which rhetoric of science is set, by having generated these downstream effects against which the New Rhetoricians and scholars in so many other fields have been reacting.

1.1.1. Modernity And Its Legacy

The start of the Modern era is generally set in the mid-1600s in Europe. Modernity represented a complete change of direction in style of thought. Stephen Toulmin attributes this change to the desire of the emerging philosophers of the period, notably Descartes, to avoid in future the genocidal, sectarian violence of the Thirty Years' War. Toulmin describes this change of direction in “The Recovery of Practical Philosophy”:

In the philosophical debate that was started by Descartes, everyone read questions about the *soundness* or *validity* of “arguments” as referring not to public utterances before particular audiences, but to written chains of statements whose

³ Kennedy (1980) describes the *letteraturizzazione* of rhetoric, that is, its “slippage” into secondary rhetoric as happening when, under repressive governmental systems, it cannot be used in public fora for self-governance. Cicero and Boethius paid with their lives for the right to speak the truth, which is the function

validity rested on their internal relations. For modern philosophers, the rhetorical question, "Who addressed this argument to whom, in what forum, and using what examples?" is no longer a matter for philosophy.... The research program of philosophy thus set aside all questions about *argumentation*—among particular people in specific situations, dealing with concrete cases, where varied things were at stake—in favor of *proofs* that could be set down in writing, and judged as written. (1988: 30-31)

In Toulmin's terms, modernity's "retreat" from Renaissance humanism brought about the apotheosis of "the written, the universal, the abstract, and the timeless" at the expense of practical concerns— "the oral, the local, the particular, and the timely"—characterizing concrete, substantive questions. Toulmin is particularly concerned with the effects of the retreat of ethics and philosophy into an analytically-sanctioned neutrality. This retreat from involvement with specific situations was accompanied by the formation of "dualisms"—fact/value, reason/emotion or faith, objective/subjective, neutral/biased—with the first term of each pair privileged and the second denigrated (Booth 1974: 17-18; Toulmin 1990). The resulting perceived inferiority of value, history, and emotion as substantial argumentative counters led to their abandonment.

Predictably, with neutrality and objectivity as ideals for proof in ethics and philosophy, and abstract, geometrical certainty the ideal in science, no method remained for addressing substantive questions. In a word, there was no longer a conceptual venue for making *decisions*—privately or publicly—for action based on *good reasons*, as this had been the realm of rhetoric, which was not just abandoned, but repudiated, by Modernist philosophers.⁴ The adoption of Rationalist ideals meant that the methods of proof used to secure *adherence* conducive to action were thus completely neglected by logicians and

of primary rhetoric, according to Kennedy, Grimaldi (1972), Horner (1980), and other scholars of classical rhetoric.

⁴ See, e.g., Locke in de Man (1978); "good reasons" are a focal issue in rhetoric for Wallace (1973), Fisher (1974; 1978), and Booth (1974).

epistemologists—for nearly three centuries. Rhetoric, on the other hand, includes the second terms of the dyads, incorporates human purposes in its functioning, and is thus capable of use in deliberation, the consideration of choices, which responsibility (in the nature of things) neither individuals, governments nor societies cannot avoid. Toulmin assigns Foucault's term, "*futuribles*," to the better and the worse futures that can result, respectively, from intentional choices versus failure to choose. The Modernist disdain for the elements of practical reason and the necessity of situated choice, he says, have left us "backing into the future." The revival of rhetoric suggests that making intentional use of rhetoric in public discourse, especially that related to choices in technology, offers the hope of better choices in the future.

1.1.2. Reaction Against Modernity

It may be debatable whether the negative downstream effects in public life and policy can be blamed on rhetoric's long exclusion from learned communication. But there is no question that, throughout this century and especially since the late 1950s to early 1960s, many writers in many fields have simultaneously undertaken efforts to overturn the Modernist view, largely because of their perception of distortions in learned communication that have had disastrous manifestations in the conduct of human affairs. To speak only of the New Rhetoric, all of the major figures in this field cited the catastrophic world wars and unanticipated negative effects of technology development and adoption as the causes of their turn to rhetoric;⁵ for example, McKeon wrote, "The war itself was an extension in armed conflict of differences concerning the nature of human communities and concerning the use of power, which had been based ideologically on what purported to be philosophical principles" (1987 [1957]): 62). Questioning of the purported objective, universal, and abstract foundations of science and philosophy as conceived in the seventeenth century is being pursued all over the academic map; in fields that, two decades ago, were pursuing strict objectivity, there seems to be an

⁵ E.g., Kenneth Burke, 1962 [1950], p. 29-30; Chaim Perelman, 1979 [1977], pp. 69-70; and McKeon, 1987 [1971], p. 11, and 1987 [1952], p. 198.

incipient return to qualitative research.⁶ This is a remarkable wholesale “change of mind.”⁷

Wayne Booth, writing in his 1974 *Modern Dogma and the Rhetoric of Assent*, mentions his own surprised discovery “that ‘everybody’ is suddenly wrestling with my kind of problem” (xv), and he is referring to studies covering from the early 1950s through to 1974. Acknowledging that he was in the company of many who were asking the same questions, Booth’s central question in his study is “When should I change my mind?” And Toulmin, writing in *Cosmopolis*, about the changeover in the early 1600s to the modern commitment to rationality, says that most scholars agree on the *timing* of the changeover (12), but, he asks, “Why did the focus of intellectual preoccupations change so drastically at just that time?” (35). The emphasis is Toulmin’s, and reinforces his focus on what circumstances at a given time could cause this revolutionary shift in the conceptual framework and direction of inquiry of the intellectual community of a whole continent. Booth’s company and Toulmin’s study, querying what are the circumstances at a given time, evoke the concept of *rhetorical situation*: “Not the rhetor and not persuasive intent, but the situation is the source and ground of rhetorical activity...and of rhetorical criticism” (Bitzer 1968: 6).

1.1.3. Rhetorical Situation

Rhetorical situation is the concept introduced by Lloyd Bitzer in a 1968 article of the same name. The concept foregrounds the part played by context in evoking rhetorical invention. In this article Bitzer asked what was “the nature of those contexts in which speakers or writers create rhetorical discourse: How should they be described? What are their characteristics? Why and how do they result in the creation of rhetoric?” (1). At the time Bitzer wrote his article, it was uncommon for rhetorical theorists to think about rhetorical situations— “...situation is not a standard term in the vocabulary of rhetorical

⁶ E.g., Muecke (1987), McCloskey (1983, 1989).

⁷ Noted as such, in, e.g., Fritjof Capra’s *The Turning Point*.

theory. 'Audience' is standard; so also are 'speaker,' 'subject', 'occasion', and 'speech'" –and their frame of reference for the realm of rhetoric was still more oriented towards its technical features. In contrast, to look at rhetorical situation is to look at features that make practical reasoning a necessity, always because there is a need for action or for choice. "Rhetorical situation may be defined as a complex of persons, events, objects, and relations presenting an actual or potential exigence which can be completely or partially removed if discourse, introduced into the situation, can so constrain human decision or action as to bring about the significant modification of the exigence" (6).

Bitzer established three essential constituents of a rhetorical situation. The first of these is *exigence*, the necessity for choice or action. Any exigence "is an imperfection marked by urgency; it is a defect, an obstacle, something waiting to be done, a thing which is other than it should be" (6). It must be able to be modified or, otherwise, it is not rhetorical: "death, winter, and some natural disasters, for instance—are exigences to be sure, but they are not rhetorical." Also, it must require to be modified by means of discourse, not simply by the use of a tool or by oneself.

Bitzer's second constituent is an *audience* that must be more than "a body of mere hearers or readers"; "a rhetorical audience consists only of those persons who are capable of being influenced by discourse and of being mediators of change" (8). *Constraints* constitutes the third element of a rhetorical situation. "Every rhetorical situation contains a set of *constraints* made up of persons, events, objects, and relations which are parts of the situation because they have the power to constrain decision and action needed to modify the exigence."

Standard sources of constraint include beliefs, attitudes, documents, facts, traditions, images, interests, motives and the like; and when an orator enters the situation, his discourse not only harnesses constraints given by the situation but provides additional important constraints—for example his personal character, his logical proofs, and his style. There are two main classes of constraints: (1) those

originated or managed by the rhetor and his method (Aristotle called these “artistic proofs”), and (2) those other constraints, in the situation, which may be operative (Aristotle’s “inartistic proofs”).⁸

Rhetoricians traditionally look at community and at cognition, but Bitzer’s proposal that “the situation, not the rhetor or the persuasive intent, is the source and ground of rhetorical activity” casts him as a “hybrid” theorist with a “sociological rhetoric” (Harris 1991: 285). His theory also gives rhetoricians a wider theoretical net, but one which is still legitimate. It allows us to ask what accounted for the shift to Modernity and what accounts for the case of the contemporary “change of mind” in which Booth and his many contemporaries are reviving, in this century, a way of seeing that scholars of the 1600s and up until now were persuaded to ignore or exclude. Why did people choose to exclude the particular, the timely, the oral and the local in the 1600s and why are we ready, *just at this particular time*, to embrace them?

The “rhetorical situation” can offer us a useful way of looking at both the changes of mind that brought Modernism and the current shift in perspective reacting to it. Furthermore, it is compatible with other views of primary rhetorical invention, both classical and contemporary. The concept of rhetorical situation seems fitting as a lens through which to examine a development of a significant success in science policy decisionmaking. This concept will serve as the primary resource in our rhetorical analysis of this event.

⁸ Bitzer (1968: 8). Since Bitzer’s article, views of “inartistic proofs” have changed considerably, recognizing the simple persuasiveness of language, so that any purposing, including scientific, can be seen as having inescapable rhetorical content. Bitzer’s comments about scientific audience are discussed later in the chapter.

1.2. RHETORICAL FACTORS IN THE ADOPTION OF SOIL CONSERVATION POLICY

1.2.1. Rhetorical Dimensions of Soil Studies

Four main points in the conservation movement of the 1930s suggest themselves as the focal issues in the analysis of the development of the soil conservation movement.

First, soil science is itself highly rhetorical. Like any other agency of discovery, it makes use of mimetic language and metaphor. How language questions are integral to knowledgemaking in soil science is illustrated with examples on debated points. More importantly, soil science is rhetorical because it is problem-oriented, and its research activities are intended to address the goal of maintaining or improving soil fertility in a context of rising population. Without this orientation towards a social goal there would have been no soil conservation movement. Second, soil science grew up in response to developments in context, which we are calling rhetorical situations. In Europe this rhetorical situation evolved from a deep intellectual shift brought about in part by discoveries in astronomy and geology. These discoveries contributed to a profound change in people's complex of concepts, particularly with respect to their relationship to the natural world. Third, in America in the late 1800s, when soil science was becoming established, it responded to a more immediate rhetorical situation of resource scarcity. As the nature of this resource scarcity came into focus, human resources were directed toward soil science. When soil science became a well-defined discipline, dynamics within this discipline developed into an internal rhetorical situation that constrained practice in and discourse on soil science and soil conservation. These constraints then would impact the response of the discourse community when (fourth) the main chance came and external circumstances, a particular set of factors, made possible the major thrust in conservation. The resulting policy is evidence of a rhetorical achievement. Each of these issues will be addressed briefly to set a context for the fuller discussion in the chapters that follow.

1.2.1.1. Rhetorical Elements in Soil Science–Language Issues

Arguments against the claim of objectivity in science and philosophy have been significant in the defeat of the Modernist view.⁹ In many fields, scholars argue that no observer can shed a lifetime's accumulation of language learning, education, and experience to consider some subject "objectively," that is, no researcher can approach the object of inquiry with a mental "clean slate." Moreover, such an approach is not even desirable, they maintain, because these factors are not dispensable; on the contrary, they enrich the observer, and are what allow him or her to discover new connections. Thus researchers bring presumptions based on language, education and experience, to any inquiry, and these cultural contributions to inquiry must be accepted as both inescapable and essential. There are also other widely acknowledged factors that defeat the pretense of objectivity in science, such as the availability of research subjects, aesthetic preferences—as for "elegant" hypotheses, and considerations of the status value of various kinds of scientific work.¹⁰ Soil science, like other sciences, shows the benefit to knowledgemaking of attention to the nuances of language. The influence of language, including the benefit of metaphor for discovery, is abundantly illustrated in soil science.

Negotiated Meaning: "Acid"

An example of the importance of language in soil science is the fortunes of the term "acid." The naming of "acid" substances had been vexed from the 1700s, when the name *oxygen*, meaning acid principle, was assigned to one element when hydrogen was actually the acidifying element. This name change involved the difficulty of overturning of an already developed understanding associated with the term, like that for phlogiston (Ronan: 1983). Bazerman (1988: 29) points to this term as indicating "how the

⁹ The impossibility of escaping individual perspectives arises from language based in individual differences (Richards 1971; Grene 1984), commitments to various interest group memberships (Perelman 1968, 1979 [1977a, 1977b]), and experience (Rorty; Polanyi). Miller (1979) reviewed these lines of study, which combine to demonstrate the impossibility of objectivity as claimed for the abstract disciplines.

¹⁰ "Presumptions" (e.g., Holmquist 1984); "elegance" (e.g., Hempel 1965); the "availability heuristic" in Fuller 1993: 205; status (e.g., Kline 1995: 207, 209; Lemaine *et al.* 1987; Johnson 1986).

accumulated knowledge of the field (represented by the literature) is incorporated into the language.”

[The term] *acid*, has been transformed through redefinition as chemical knowledge and orientation have changed. In Bacon’s day the word acid meant only sour-tasting; then it came to mean a sour-tasting substance; then, a substance which reddens litmus; then, a compound that dissociates in aqueous solution to produce hydrogen ions; then a compound or ion that can give protons to other substances; and most recently, a molecule or ion that can combine with another by forming a covalent bond with two electrons of the other.... (1988: 29)

There was still objection to the association of hydrogen ion saturation with acidity until the 1920s. For example, John Russell, Director of the Rothamstead Experiment Station, wrote in *The Fertility Of the Soil* that certain injurious substances had been observed in soils in the United States, where they had been much studied and “have given rise to considerable controversy.” Russell went on to object to acid as the name given in America to these substances recognized as harmful to plants and occurring in wet soils poor in calcium carbonate.

Nothing is known about these substances (in English soils, at any rate) but some years ago the fashion arose of calling them acids without any sufficiently rigid proof. It is distinctly unwise to prejudice future investigations by assigning a name already used for a definite group of substances to another that is yet unstudied, and we shall therefore adopt the nomenclature of the practical men and speak of such soils as “sour.”...Whatever the cause of “sourness” (the name commits us to no hypothesis), it can be remedied by drainage, lime and good cultivation. (1913 :36-37)

The point, exactly, of US soil scientists’ calling a hydrogen-ion saturated soil “acid” was to “commit to the hypothesis” of this condition, as “acidity” had been firmly connected with hydrogen since Lavoisier and Priestley.

The "Fit" between Nature and its Linguistic Representation: Two Examples

Another example of the connection between invention and language use, illustrating Bruffee's (1986) observation that the language scientists use determines what they know, is the case of the term "mulch" by soil text writers Lyon and Fippin (1913). Their text adopts and uses the term both for "dust mulch" as well as for mulches of applied substances such as straw. By applying this term for both, they were prevented from making distinctions and contrasts between the actions of cultivated ("dust mulch") and applied mulches, despite the fact that the treatments affect soils quite differently over time, dust mulch detrimentally and applied mulches positively. Their acceptance of the single implied definition prevented their being able to make a sound evaluation and recommendation for practice; indeed, it implied an unsound recommendation. As a heuristic tool, Stasis theory helps researchers inquire into definition, classification, and evaluation, and can facilitate exploration of issues such as this (as will be seen in Hilgasrd's case). The debate over distinctions was not taken to its foundation in this case, and persisted until the late 1920s.

Lyon and Fippin were by no means incompetent soil scientists, but had they questioned the meanings of their term they would not have missed the opportunity to promote better agricultural practice in that instance. By contrast, Hilgard (1906: 109 n 1) made an important distinction about soil aggregates, relating to his scientifically prior discovery of the connection of calcium to flocculation, a valuable characteristic in soil structure. Hilgard wrote that he preferred Wollny's term *Krummelstruktur* to F.H.King's "kernel" because "kernels are understood to be bodies rather more solid than the surrounding mass, whereas 'crumb' is generally understood as meaning a relatively large, loose aggregate." In making this distinction he was insisting on the linguistic recognition of the physical manifestations and effects of calcium's properties as a bivalent element in cation exchange. He was decades ahead of his colleagues in this recognition, which he transmitted effectively because of his fluency in the German language. Both Arieti (1976) and McKeon (1987 [1952]) discuss exposure to different cultures and languages and

environments as a possible aid to discovery. With respect to Hilgard's effect on soil science terminology ("crumb structure" persists as the accepted term) Bazerman (1988: 28 n 10) quotes Latour and Woolgar, who suggest that scientific persuasion is successful when attention is drawn away from the circumstances of statement creation toward a "fact," which appears to be above the particularities of a specific circumstance. In the authors' terms, "the processes of literary inscription are forgotten."

This act of creation of new knowledge through language is invention. "...[I]n the world of language words create 'things'," says Weaver (1970 [1961]:131), and "To find a word is to find a meaning; to create a word is to find a single term for a meaning partially distributed in other words. Whoever may doubt that language has this power to evoke should try the experiment of thinking without words" (1970 [1948] 45). Once it is recognized that language is socially and culturally situated and that it contributes to the establishment of what reality is perceived to be, there are obvious implications for scientific practice, namely that it is socially constructed and is inescapably rhetorical.¹¹ In particular this perspective applies to the "inartistic" proofs, such as these illustrations from soil science have shown.

Other, more conventional, aspects of rhetoric are also abundantly illustrated in soil science. The influences of institutional environment, bullying rhetoric or eristic by scientists in positions of authority, strong emotions evident in such situations, obvious audience effects on discourse, both institutional and collegial, all will be found in soil science. Inventional aspects of language are, however, most important in that they reflect experience and comparison to nature; that is, they contribute to contextual problem solving, which we now address.

¹¹ Studies on the social construction and/or the social nature of scientific knowledge and decisions include LeFebvre (1987), Bazerman (1983, 1991), Bruffee (1992), Journet (1990), Miller (1980, 1992), Odell, Goswami, Herrington and Quick (1983), Overington (1977), Shank and Vampola (1992), Waddell (1990).

1.2.1.2. Soil Science is Rhetorical in its Orientation

Soil science is a different sort of science from the science against which Toulmin argued. If physics, “the master science,” is abstract, universal, and timeless, soil science in theory and practice is anything but. Toulmin concedes that the natural sciences are “deeply grounded in experience” and so have gone “a long way toward redeeming themselves” (1990: 181). If a continuum were posited to run between Toulmin’s and Booth’s dualities, with theoretical science and *a priori* truth at one end and rhetoric and values at the other, soil science would have its place at the value end.

Soil science is a rhetorical science, not because of its pervasive use of metaphor, not because it creates its own compelling universe of meaning, and not because (as in any scientific publication) publications must demonstrate deference to authorities in the discipline—although all of these are true—but because of its orientation. Soil conservation and its parent soil sciences are rhetorical in that their basis of organization and their goals are rhetorical. Soil science has always been organized around a problem—to maintain the fertility of the soil, and its goals have always been rhetorical—to generate the adherence necessary for action, individual and, especially, concerted action to affect future conditions. Ethos and pathos are incorporated in these objectives. Peter Meyers, head of the Soil Conservation Service (SCS) in 1984 said, “Soil erosion is not a today problem; it’s a tomorrow problem, but you have to work on it today. Why squander what we have?” (Gibbons 1984: 388). It is common for soil scientists to express this value, that wasting soil is immoral.

If we consider what features a rhetorical science would have, we would see that the familiar cluster of descriptors, including *purpose*, *future*, *choice* or *action*, *probability* or *uncertainty*, *problem* or *issue*, *values*, *good reasons*, and *experience* apply to it. Soil scientists at the turn of the century and for the next two decades were developing the *topoi* that combined the few certain “soil facts” with contextual clues to try to address the exigencies of soil management in America at the time; this was not easy. And although

they believed they could eventually solve the problems, the great difficulty lay in finding the right solutions to extremely complex problems. Bitzer writes of rhetoric as discourse that responds “as answer or as solution to a question or problem” (1968: 5-6). An exigence is not a rhetorical exigence if it cannot be modified (like a natural disaster) or if its modification does not require the assistance of discourse.

An exigence is rhetorical when it is capable of positive modification and when positive modification requires discourse or can be assisted by discourse...The pollution of our air is also a rhetorical exigence because its positive modification—reduction of pollution—strongly invites the assistance of public discourse producing public awareness, indignation, and action of the right kind. (7)

Furthermore, solving problems requires materials of different kinds (e.g., concepts, data), and it requires that these different kinds of materials be examined via a kind of “lateral thinking,” rhetoric’s invention, which has what McKeon called the “architectonic” power of organizing data and concepts of different kinds and orders. Toulmin also accounts relevance as calling for interdisciplinarity. The need for rhetorical thinking to solve practical problems is abundantly illustrated in soil studies. In every stage of its development, elements not known or considered before, as well as elements from widely varying disciplines, have been included in the solving of the puzzle of soil processes and fertility.

Interdisciplinarity, which is a recognition that a given problem must be seen in a new, wider context, is a characteristic of soil science. E.G.Hallsworth (1987: vii) writes, in his preface to *The Anatomy, Physiology, and Psychology of Erosion*, “Problems do not appear with departmental labels attached to them,” and solving only one aspect of a problem often makes it worse rather than better. The highly complex problem of maintaining soil fertility proves his point; things did get worse during two periods at least when overly simple and theoretically-based approaches to it were followed. When in the early 1920s, for another example, terracing was relied upon as a lone expedient to stop erosion on sloping land, the problem worsened. On the other hand, the greatest progress

in research was made when the highly complex nature of soil began to be recognized, so that researchers worked largely on an empirical basis. Solving the central questions in classification (pedology) eventually involved chemistry, physics, and math from the fundamental sciences; geology, geography, and climatology from the earth sciences; botany, zoology, and microbiology from the biological sciences; and the (so-called) applied sciences agriculture, forestry, and engineering. Soil conservation efforts required contributions from these fields and, in addition, history, economics, theology, anthropology, and sociology.

The most important feature a rhetorical science would have, however, would be “good reasons.” “A logic of good reasons” is the reasoned basis for promoting the long-term health of natural systems that support humankind. In this respect the content of soils science/soil conservation makes it the most compelling imaginable of all possible arguments: “Without soils there would be no grass, no cows, no bread, no us” (Gibbons 1984: 362). The material for backing behind the claim for soil conservation is everywhere to be seen, and goes to the most fundamental issue of human endeavors. In this respect, the issue illustrates Weaver’s belief that rhetoric is an art of emphasis. And in attending to soil erosion—conserving the trust of generations before and preserving it for generations to come—a soil scientist is acting “on the ideal basis for human conduct.” As Walter Fisher wrote in “Toward a Logic of Good Reasons” (1978: 380) determination of what that conduct should be “is clearly the paramount issue that confronts those responsible for decisions that impinge on the nature, quality, and the continued existence of human life....” How does soil science, if it is indeed a “rhetorical science,” demonstrate a logic of good reasons?

1.2.2. “The Discovery of Time” and the Impact of Geology on the Perception of Rhetorical Situation

If we can imagine a rhetorical situation as a ring around rhetor and audience representing the immediate situation, such a situation might include very specific and immediate

elements: How many people came to a gathering—fewer than expected because of a big snow? Or more than expected because the issue resonated with people more than was hoped for? Is the audience homogeneous or heterogeneous, and in what sense? Wider rings of rhetorical situation embrace the immediate one, spreading both wider, out toward a more broadly construed audience, and deeper, going further back in time. Bitzer's reference to traditions and beliefs as constraints justifies this interpretation. Situations, therefore, reach back into history and, in the rhetor's vision, forward into the future. Many of the elements of the more inclusive rhetorical situation for rhetoricians before and into the early to mid-1900s also applied to soil scientists: at times a suffocatingly orthodox intellectual atmosphere, population pressure on the land and resources, technologically enhanced communication, for example.

The very first steps toward soil conservation were taken in the 1600s and 1700s, though where they would lead would hardly have been recognized as such at the time. There were two such developments. The first was the growing recognition, via archaeology and more common international travel, that there were civilizations that had become defunct, and places that once were fertile and prosperous, where there were now only rocky mountains and barren plains.¹² George Perkins Marsh's classic *Man and Nature*, published in 1864, was an early product of this growing awareness.¹³ This recognition testified to human agency in changing—destroying—natural systems irrevocably. That is, it revealed soil degradation by erosion, salinization, and plain exhaustion as potentially a rhetorical exigency rather than simply an unavoidable natural disaster. The second step came with what Toulmin called “the discovery of time,” geological time, that is. To appreciate geology's (and Copernican/Galilean astronomy's) intellectually transforming

¹² R.A.Long, of the Forest Service, spoke at the 1908 Conference of Governors about “physical geographers” (archaeologists) commenting on the contrast between the historical reports and the present reality of Biblical sites. He asked rhetorically, “Can these stony hills, these deserted valleys, be indeed the Land of Promise, the land flowing with milk and honey?” Long also cites, Cicero as designating ‘those engaged in forest devastation as the enemies of the public interests’, and he reports from Aristotle’s “National Economy” that “an assured supply of accessible wood material is one of the ‘necessary conditions of the existence of a city’.”

¹³ Perlin and Hillel offer two recent accounts of the interactions of civilizations and natural resources.

and revolutionary force for late 18th and for 19th century scientists, we must first recognize some intellectual features framing the possible conclusions available to their predecessors in soil research.

Before these developments, religion determined that the short-term teleological framework within which human experience was acted out made certain kinds of questions irrelevant, particularly those having to do with the material welfare (of religious rather than secular “man”) and to do with the long-term viability of the natural resource base. Questions about natural resources were sometimes asked, but were liable to be answered within that teleological framework. Daniel Hillel in *Out of the Earth* writes of the Bishop of Carthage, the future St. Cyprian, who wrote to the Roman proconsul in the third century AD about conditions in North Africa surrounding Carthage: “‘The world has grown old and has not retained its former vigor. It bears witness to its own decline....The husbandman is failing in his field....Springs which once flowed profusely now provide only a trickle.’ Being a Bishop, Cyprian attributed the decline to a lack of faith” (1992: 106).

At this time, and up until the time that solar-centric astronomy and geology became common understanding, God’s intimate concern with and control of nature was thought to have evidence in nature, and an anthropocentric view of nature at that (Toulmin 1990). Natural disasters might be seen as punishment for immoral living but were not recognized as resulting from misuse of the land. Hence natural disasters were rhetorical situations, but with the cause incorrectly attached to moral actions rather than to cultural practices which did in fact cause floods, dry up springs, and turn the land barren. This theological framework is a socially constructed one which, like Kuhn’s scientific paradigms or any cultural framework, conditions the “data” we are able to “see” by acting as a working hypothesis about the nature of experience.

Whereas ecclesiastics in Galileo's time and Leibnitz in a discussion with Newton could insist on the tidiness of God's creation—from an anthropological perspective—and the fact that it had to center around human creation, when the dust had settled around discoveries in these areas, it was undeniable that humans were small latecomers in the universal picture. The expectation of a teleologically-based, catastrophic end to civilization began to appear unfounded. That teleological grounding had been undercut by new perspectives from science and because religious authority was no longer strong and central to the individual and the community as it had been when community life could revolve around the centuries-long construction of a cathedral. Different interpretations resulted; some took the lessons of astronomy and geology to commend a perspective of nihilism, but for others it meant responsibility. No longer could flooding be looked upon as divine retribution for a generally dissolute people; now it was recognized, at least some of the time, as a natural outcome of deforesting a watershed and running out the land. As Bitzer says, "...and some natural disasters." Because of a change in concepts about the earth brought about by geology, some natural disasters were becoming recognized as exigences in rhetorical situations.

One of the first prominent soil scientists in Europe, Justus von Leibig, although a chemist, would have studied and worked with geologists, because Germany at the time was training numbers of geologists for land evaluation work. Leibig explicitly linked his efforts in the mid-1800s to synthesize chemical fertilizers with static or falling per-acre yields, growing populations, and the famine and hardship foreseen by Malthus in his 1798 treatise on population. If there could be a long future—and geologists were showing there had been an immeasurably long past—this population would have to be fed! Leibig and others made quite clear, in their writing and teaching, that they were aware of this problem and meant to address it. Leibig in particular was quite self-consciously rhetorical about the future effects of his own work.

Increasing population pressure had led to a keener interest among landowners in England of the late 1700s in the productivity of their lands. Many more progressive landowners began to take over the management of stock, pasture improvement and crop rotations, and the keeping of records on treatments and yields. The Rothamstead Experiment Station, which became renowned for the longest-running field trials on crop rotations and fertilizers, was the land of a private owner, J. H. Laws, who wanted to devote it to research; in the 1840s he started and then ran experiments with scientist Henry Gilbert for four decades.

This interest led to a strong trend in “scientific agriculture” and a tradition of lay science and a relatively high level of knowledge in the general farming population in England. E.J. Russell, director of Rothamstead in later years, addressed his fairly technical 1913 monograph *The Fertility of the Soil* to a wide spectrum of lay rural readers and gardeners, with the expectation of being read and understood. Indeed, in England, according to Russell, this sense of responsibility meant going beyond maintenance of current levels of fertility to raising the range of fertility—that is, its base level and upper limit—on a given property.

1.2.2.1. Rhetorical Setting for Soil Science in the US at the Turn of the Century

At the turn of the 20th century in the United States, there was a strongly contrasting situation. Rapid deforestation was rampant and the resulting flooding and incapacitation of major water transport arteries were severe. Thus in 1908, Theodore Roosevelt held a landmark Conference of Governors, the purpose of which was to hear presentations from the scientific community as a prelude to deliberating on natural resource policy. Geologist T.C. Chamberlain¹⁴ spoke at this conference, and stated explicitly his view of the implications of the discovery of geological time for the responsibility of the present generation:

¹⁴ Chamberlain was then head of the Department of Geology at the University of Chicago, editor of the *Journal of Geology*, and President of the American Association for the Advancement of Science.

The invitation to give thought to the resources that affect our future appeals to me with peculiar force, for my studies of the past decade have led me to the belief that the era of the earth's future habitability is vastly greater than we have been wont to think. We have grown up in the belief that the earth sprang from chaos at the opening of our era and is plunging on to catastrophe or to a final winter in the near future. Quite at variance with this, I have come to believe that the earth arose from a regenerative process and that it offers a fair prospect of fitness for habitation for ages to come. If this be true, it is eminently fitting that our race should give due measure of thought to the ulterior effects of its actions. These considerations especially intensify the problem of that resource [soil] upon which our civilization so profoundly depends.... Whether you are prepared to accept so large a view of the habitable future or not, I trust you will strike hands with me in the conviction that the probabilities of the future are at least so great as to render imperative the serious consideration of our obligations toward it.

In the late 1800s in the United States, symptoms of ruined land were everywhere. There had been Congressional Hearings over land abandonment with emphasis on New England in 1901. References to that situation were common in USDA publications. Flooding was widespread. Small mills and millstreams in the New England and southern coastal states were lost to use because of silting and gouging and damage to flumes and machinery. Humanity had had such impacts before, but never with such rapidity in such a landscape of plenty. Moral action requires a sense of the future and of potential for retribution for an act or lack of action. In this sense Chamberlain's rhetoric is charged with implications for moral action. While the morally right act in Cyprian's time was to atone for sins so that fertility would be restored, in the new era of geological time, and assuming sound soil science, moral feeling transferred to the true cause could be translated into the remedy.

At this conference, perhaps for the first time before a truly rhetorical audience, people were being characterized as a force in and of nature that they had newly become because

of numbers—population pressure—and their power to change a landscape. In Chamberlain's speech is the hope that the still-young nation would engage the responsibility, as a civic action, to put a stop to the abuse of the land. In this determination is illustrated, I believe, McKeon's argument that a people learn the virtues of self-governance in the process of experiencing the liberty to do so (1987 [1957], [1970]).

1.2.2.2. The Disciplinary Setting

Chamberlain's comments also show that the problem of soil degradation was perceived to be urgent. Accordingly, the government began to demonstrate commitment to soil science. Federal funding, on top of that supporting the State Agricultural Colleges and then the Experiment Stations, was provided to promote improved agricultural practices through Extension Agents. Levels of support for the Department of Agriculture continued a sharp rise. The Department's *Yearbook* grew in size and number of copies printed, and it commenced printing the *Journal of Agricultural Research* to foster communication among professionals spread across the country.

Soil science was among the target research areas, and one that was expected to produce results that would be useful and applicable for people on the land. This is an expectation of "science" that is quite at variance with what we saw was the geometrical ideal of Modernity. Toulmin's study led him to see the rationalist demands for certainty and systematicity as having resulted in a mechanistic ideal in holding geometry and physics, theoretical sciences, up as models for the natural sciences. But, in a passage that accounts for sciences as empirical and complex as soil science, Toulmin writes

...what is good in modern thought and practice has already gone some way toward *redeeming itself*. In particular the natural sciences, as they exist in the closing years of the 20th century, have come a long way from the mechanistic physics—or "natural philosophy"—that took shape in the 75 years after Descartes' manifesto in the Discourse on Method. Far from being formal systems based on abstract

theoretical ideals alone, with a “certainty” borrowed from geometry, today’s sciences are deeply grounded in experience.... (1990: 192).

It is true of soil science that it has moved, over some 300 years, from an insistence on simple and universal rules for soil fertility to a recognition of the extreme complexity and particularity of soil systems. It is also true that in its focus on problems arising from the interaction of people with a natural resource, it incorporated the “good reasons” not seen in the abstract sciences. In this progression, experience has been the chief guide.

1.2.2.3. Focus of this Study

This study is most concerned about effects of context, that is rhetorical situation, on the making of knowledge in soil science and the development of the soil conservation movement. Since invention was the aspect of rhetoric that was not accessible to rhetoricians for this long period, it is the aspect that should at this point receive the most attention. This study is intended to look both ways: it is intended to make a contribution to rhetoric by testing its precepts and claims, particularly that of the rhetorical situation, against a case of policy-making; it is also intended to make a contribution to policy-making in natural resources, because human pressure, our pressure, on nature, has built up in an era when decisionmaking was too little informed by rhetorical principles. I want to inquire in what respects a thoroughly benign policy decision was informed by rhetoric for the purposes of making any possible conclusions available for current decisions. Rhetoric of science is responding to a situation in which a legacy poor decisionmaking about nature is *pushing* us and the need to lose no more time is *pulling* us to deal with these issues. Consequently we will be addressing two central questions as we proceed.

1.3. QUESTIONS ABOUT RHETORIC AND THE RHETORIC OF SCIENCE TO BE ADDRESSED

The first major question relevant to rhetorical theory is whether Bitzer’s conception of rhetorical situation is useful for rhetorical analysis. This examination of the rhetorical situation of soil conservation can test whether the concept is useful and fruitful, and can

yield insights for either rhetoric or natural resource decisionmaking or both. In Bitzer's view, rhetorical theorists have neglected the situation in which persuasion occurs. For him, situation is the grounding of rhetorical invention and discourse realization. However, "The Rhetorical Situation" was countered, also in *Philosophy and Rhetoric*, by Richard Vatz in "The Myth of the Rhetorical Situation." Vatz's objections reflect the difference between an extreme Sophistic and an Aristotelian view, and offer a benchmark against which to test the concept of *rhetorical situation*. As "rhetorical situation" is the central construct of our study, let us reemphasize the main points.

The "rhetorical situation," Bitzer writes, is "a natural context of persons, events, objects, relations, and an exigence which strongly invites utterance..." (5). The first of the three constituents Bitzer says are necessary in a rhetorical situation is *exigence*, "an imperfection marked by urgency; ... a defect, an obstacle, something waiting to be done, a thing which is other than it should be" (6). A rhetorical exigence is one that can be modified only by means of discourse or "can be assisted by discourse" (7). Bitzer's second constituent of a rhetorical situation is an *audience*, not of listeners only, but of potential actors, potential mediators of the desired change (8). Bitzer's third constituent is "a set of *constraints* made up of persons, events, objects, and relations which are parts of the situation because they have the power to constrain decision and action needed to modify the exigence"; there are two main classes of constraints, and these are equivalent to Aristotle's artistic and inartistic proofs (8).

In Vatz's response in *Philosophy and Rhetoric*, he maintained that the rhetors create or determine the situation. They do this through independent determination of which facts or events are relevant and should be emphasized; they also create the situation by the way they use language to "translate" the exigence as they see it, so that "[t]o the audience, events become meaningful only through their linguistic depiction" (157). Political events, for example, "are largely creations of the language used to describe them."

As soon as one communicates an event or situation he is using evocative language. As Richard Weaver and others have pointed out, language is always value-laden. Clearly the adjectives into which a "situation" are [sic] communicated cannot be the "real situation"; they must be a translation. Surely we learn from Bentham that rhetors can arbitrarily choose eulogistic or dyslogistic coverings for the same situation: We have "leaders" or "bosses," "organizations" or "machines," and "education" or "propaganda" not according to the situation's reality, but according to the rhetor's arbitrary choice of characterization. No theory of the relationship between situations and rhetoric can neglect to take account of the initial linguistic depiction of the situation. (157)

Vatz's conception of language as "translation" is valuable. It is likewise a conclusion arrived at from many different persuasions and one which contributed essentially to the revival of rhetoric. But it may be objected that (1) some things many people are apt to "translate" to themselves in the same way, and thus come up with the same opinion about it (i.e., Hempel's "intersubjectivity"), and (2) these same people may well hold on to their own conclusions in the face of arguments which do not translate a given slice of "reality" as they do, that is, they will resist persuasion. The rhetor's argument must reflect an intersubjectively acknowledged version of reality. A further possible objection to Vatz's argument that the rhetor translates reality is that the rhetor's success may, indeed, depend on the situation being addressed. The more widely accepted are the goods or values that are perceived to be threatened in the exigency, and the more impelling the exigency, the more likely the rhetor will be to succeed. I hope to contribute with this study to a resolution of this question, of the degree to which the rhetor is responsible for existence or the perception of a rhetorical situation.

Another commentator on the rhetorical situation, Scott Consigny, regards Bitzer's and Vatz's positions as "antinomies" and tries to find and develop a middle position. He writes, "Bitzer's rhetor does not possess a special capacity which distinguishes him from

other problem-solving experts; he has no special power of disclosing problems in novel and indeterminate situations" (180). Consigny's intervention seems gratuitous, however, because Bitzer did not set out to articulate a full theory of rhetoric. The article does not elaborate on the rhetor but assumed that the rhetor *was* one, that is, can make use of the art of rhetoric. For the community of readers of *Rhetoric and Philosophy*, that should imply that the rhetor knows the commonplaces and special topics and how to use them, whether consciously or intuitively, and these are all that Consigny has to offer (181-184) by way of a new theory.

Consigny's description of topics as (1) *instruments* that are "places for the perception, discovery, and explanation of the unknown" is McKeon's characterization, and represents a long tradition of the inventional logic proper to rhetoric; but his description of topics as (2) a *realm* includes only the rhetor's immediate situation (181-182). Bitzer, on the other hand, seems to mean historical conditions—things that embrace the rhetor or audience but not necessarily on a personal or immediate level, but forces or a convergence of (sometimes great) forces, that happen to make a certain time the right time. Is this a justifiable position? Bitzer's point in emphasizing that a situation invites—practically demands—fitting speech from the rhetor—was that if the time is not right to make an argument, the rhetor will not be moved to make the argument, seeing that other conditions are not in place to make the argument take hold. Bitzer's concept does not belittle the rhetor or his skill, as Consigny implies, for to take up that role a person also has to have great learning and integrity and courage—these too, I believe, are taken for granted as the rhetor's qualities in Bitzer's essay. These are questions that I would like to test against the situation in which soil conservation policy was adopted.

The second question addresses the depth and extent to which rhetoric inheres in science. In part, this question reflects the issue of the distinction between dialectic, "questions about the nature of things," and rhetoric. If such questions are inevitably affected by traditionally rhetorical features of inquiry such as language, emotional commitment, and

experience, and if they cannot be asked except from *someone's* perspective, perhaps this distinction should be abandoned. Ede and Lunsford, Halloran, Miller and Selzer, and many other scholars in rhetoric and in composition studies argue that this distinction, established by Aristotle to protect the independence of the practical arts, should be abandoned. Allen Harris, concurring with the contemporary view that collapses the distinction between dialectic and rhetorical reasoning, says that "science is rhetorical, leaf to root..." (1990: 17). And he sees it as rhetorical not in superficial respects, but "forensic to the roots, not just in the classical sense of finding true interpretations of evidence by weighing the cases that insistent advocates offer for opposing interpretations, but in the modern extension of the term—weighing physical evidence to solve a puzzle..." (1991: 297).

A contrasting view is that of philosopher of science Marcello Pera. In *The Discourse of Science*, Pera writes that "Scientific discourse is rhetorical in a constitutive way, because scientific claims are accepted only if they persuade an audience (community) within which they are put forward and debated through an exchange of arguments and counterarguments whose outcome depends in no way on the passions and personal commitments of the protagonists, or on the style of the text" (1994: ix). Here Pera holds that dialectic remains distinct from rhetoric, even denying, apparently, the commitments that Kuhn attributed to members of paradigms.

The issue of the pervasiveness of rhetoric in science can be addressed partly by looking at the status of scientific facts. To Gross they are strictly "socially justified," the "brute facts" have no meaning or potential meaning in themselves; meaning is constituted entirely by the successful practice of rhetoric in a science whose primary object is persuasion. Pera states that facts reflect a "nature that provides answers by saying 'yes' or 'no...'" (Pera 1994: ix). Bazerman expresses the view, in *Shaping Written Knowledge*, that

Science has taken empirical experience as its major touchstone, so that in the process of negotiation of meaning, empirical experience not only constrains the range of possible meanings but is actively sought in the attempt to establish stable meanings from the negotiation. Thus, whatever may be the source of statements, the fate of statements depends on the experience generated by them. In this way science has made nature its ally. The claims that endure do so precisely because (within the particular set of problems and activities considered important) they have been able to ally themselves closer to nature than their competition, so that in the long run, one set of terms rather than another proves more fundamentally useful in carrying on activities. (1988: 312)

Bazerman's rhetorical analysis leaves room for a "realist's" belief in fact. Is the conviction that rhetoric contributes substantially to science compatible with a belief in "facts," or must a rhetorical view of science require that if meaning is negotiated, there are neither facts nor objectives in scientific practice beyond persuasion? The question of the status of fact can contribute to the exploration of my question of the degree to which rhetoric is "constitutive." Is it constitutive only in the sense Pera allows, or is it more fundamentally constitutive, in the way that McKeon, Burke or Harris or Bitzer see it, as being a means of analyzing a problem, identifying its aspects and dimensions, and working towards the resolution of it.

My particular focus in this rhetoric of science research is to see how dynamics within and outside of a scientific enterprise affect it; specifically, I am interested in how they affect *invention*, how they affect what the science *elects as its goals*, how it defines these goals, and what *rhetorical* steps it takes to carry them out. To pursue answers to these points, I will ask a number of questions of the soil science situations, as I am able to reconstruct them: What features of context or problem evoked what arguments and kinds of arguments? Did rhetors differ in their characterization of context and therefore in their response? How does soil conservation demonstrate rhetorical invention? How is it a response to rhetorical situation?

I also want to ask what influence “facts” have on invention, and what kinds of “facts,” for example, what soil facts and arguments were available at what times and how did this availability affect the approach toward soils and soil conservation. Finally I would ask in what ways rhetoric helped bring about soil conservation, and what can we learn from this policy-establishing development that can help set equally beneficial policy commitments in the present and future. Following through with such questions, with the focus that has been set out in this chapter, should make it possible to address the central issue of the interrelation of rhetorical situation and invention in soil conservation.

CHAPTER 2

THE SITUATIONAL CHARACTER OF EARLY SOIL STUDY

INTRODUCTION

In the "old story," philosophy and science find truth that is objective and universal. Rhetoric of science claims otherwise, demonstrating that the truth of science is situational and a function of the knowledgemaking of social groups in historical times and places. Sciences dealing with the same definite question must come to different conclusions in different places and times, because the expression of a problem varies, as do the goals and experience of the investigators. Whenever and however arrived at, socially constructed knowledge then becomes the logos of rhetoric, which is continually compared against the slice of nature it is meant to describe, and the bits and the tentative picture they suggest are worked into the continuing knowledgemaking activities of the group. This perspective comes into focus when various groups are engaged in the study of the same subject with different results. The rhetorical view of this outcome is that problem solving is motivated by ethical and emotional reasons conjoined to those of knowledge—and all lodged in the (local, temporal, particular) problem situation; thus different local and temporal problems are likely to be reflected in the knowledgemaking activities and findings of a given group.

Scientific knowledge of soil has developed as the picture of a puzzle emerges when more of the pieces are found (with the meaning of *invenire* intended) and integrated into the whole. There are some universal aspects of soils (the most critical being that plant and animal life everywhere depends on them!) but what is universal has only emerged from comparison of the particulars. In general, however, soils are marked far more by diversity than by uniformity or universalities.¹⁵ Soils and soil fertility began to be studied in the 1600s, but the diversity, as well as the extreme complexity of soils, began to be

¹⁵ In the writing of this study I have relied on numerous soils texts, including works by: Arakeri and Donahue (1984), Baver (1940), Brady (1984), Buol, Hole, and McCracken (1980), Jacks (1954), Jenny (1941, 1980), Joffe (1949), Pearson and Adams (1967) (articles by Adams and Pearson, and by Coleman

recognized only by the 1900s when they were being studied across all of the Western societies. The results of the initial stages of soil study bring out that what is seen about soils, as objects of scientific study, varies with setting—temporal, social and local. The point is made by looking at the initial stages of soil science and its development through the nineteenth century.

2.1. PRE-PARADIGMATIC AGRICULTURAL SCIENCE

Although agricultural practice had for centuries recognized the value of manuring and marling, the bases of the improvements to yields were not understood. “Scientific” agriculture did not really begin until the 1600s, and was still in its infancy until this century. The newly developing field of soil science had to start from scratch and take nothing for granted. Researchers began by asking what was the source of life, what was the unique “principle,” of life in plants. For the earliest investigators, the legacy of Empedocles’ and Aristotle’s elements restricted the possible responses to only one of four answers: earth, air, fire or water (Kellogg 1957: 3). Francis Bacon, for example, maintained that water provides plants’ basic nutrition, and the only function of soil was to support plants “in a vertical position” (Krupenikov 1990: 86). Krohn and Schäfer, in their sociological study of the science of biochemistry, touch upon the function of received or authoritative views in the development of theory. They recognize at least three forms of external influence on science:

[Firstly,] external influence can appear in the weak form of a stimulus to resource allocation and institutional guarantees.... Second, external factors can influence the content of theories. As a rule this applies in particular to the semantics of a theory: the habits of thought of the social superstructure preform the patterns in which empirical knowledge is expressed, as is often claimed for the Darwinian theory of evolution so far as it overlaps with economic liberalism. Such patterns may considerably influence the heuristics of research.... Finally, external factors

and Thomas), Russell (1957), Singer and Munns (1987), Stallings (1957), Thompson (1952), and Waksman (1938).

can influence the constitution of the subject matter of the respective research field. This is the case where the concepts delineating the subject matter of a research field involve social norms which cannot be held to be properties of a natural reality. (1971: 27-28)

In the case of early scientific studies, the received and still prevalent concepts of the ancient elements, and their associated terms—as in Krohn and Schäfer's second type of influence—seem certainly to have guided and restricted the possibilities of which researchers could conceive. The earliest experimental research in its early stages was governed by this categorization of matter.

Governed by this question of the four principles was Jan Baptista van Helmont's growth trial of 1635, widely regarded as the first true soil science experiment. Van Helmont planted a willow tree in 200 lb. of completely dry soil. He gave the tree rainwater and allowed it to grow for five years, during which time it was protected from dust. When he took up and weighed the tree, and redried and weighed the soil, the soil had lost only 2 oz., and the tree had gained 165 lb. The missing two ounces was considered negligible, within a margin of error, yet that minute amount accounted for the uptake of the solid elements essential for the plant's growth; these were the mineral salts (potassium and phosphorous) that Palissy had hypothesized to be significant, as well as magnesium for the chloroplasts. There were also calcium, silicon, iron, aluminum, sulfur, and other minerals in yet smaller—"trace"—amounts, none of which had yet been identified. Following the convention, or paradigm, of his time, Van Helmont assumed that fire, earth, air, or water would be the basis of plant growth; nothing then being known of photosynthesis, he ruled out air, dismissed as insignificant the small weight of lost soil, and concluded that water was the "life principle" in plant growth. The bulk of the plant's growth had of course come from atmospheric carbon about which there was not the least idea at that time, nor the means of measurement.

The period between 1630 and 1750 was one of “industrious search for the ‘principle of vegetation’” (Waksman 1938: 10-11). Some early researchers favored “earth” and earthy materials in this search. Even before van Helmont’s experiment, a 16th century “potter to French royalty,” Bernard Palissy, “regarded the ash left when plants burned as ‘a kind of salt’ and he thought this ash was the ‘principle’ of plant growth” (Kellogg 1957: 3); Palissy noted how various minerals affected the growth of plants but did not record these effects. Palissy’s identification of mineral nutrients was echoed by de Brosse (ca 1621), Glauber, looking in particular at nitric acid or saltpeter (mid-1600s), John Mayow (late mid 1600s), Stubbs (1668), Baldavius (1681) and E. Mariotte (1680s) (Krupenikov 1990). Both Palissy and Glauber enunciated a principle that Leibig was to emphasize in the 1840s, namely that the minerals that are removed from the soil must be returned to it. Glauber in particular “commanded great respect and authority among chemists, [and] generated a number of investigations and practical suggestions” into forms and processes of nitrogen, but because of its volatility and the still-rudimentary techniques of identifying and measuring different forms of soil nitrogen and other “salts,” experiments based on Palissy’s and Glauber’s hypotheses were “disappointing” (Krupenikov 1990: 88).

In other experiments, van Helmont had found (and named) gasses from various solids and fluids that had characteristics of their substance of origin; this work was some of the first to put into question the ancient four-part classification. Van Helmont’s work on gasses showed clearly that “the old earth-air-fire-water-classification had become unproductive” and that “the old system left chemistry a confusing muddle of ideas” (Ronan 1983: 384-5). Robert Boyle, one of the founding members of the Royal Society, repeated van Helmont’s experiment but with Indian squash and, after destructive distillation got “salt, spirit, soil, and even oil” as “they can be produced out of water” (Krupenikov 1990: 87). Boyle must, however, have felt some discomfort with his conclusions, and reportedly knew van Helmont’s research on gasses as well as on plant nutrition. Furthermore, Boyle showed his interest in soils as relating to plant production by circulating in 1665 a questionnaire to English farmers about soil properties and types and conditions of soils on

their lands (Tandarich and Sprecher 1994). For whatever reason, he must have felt a new basis for inquiry to have become necessary, because in *The Skeptical Chymist* in 1661 he called for "a new definition of elements to replace the ancient four" (Ronan 1983: 384). Shifting the grounds of inquiry in the sciences seems, for a number of reasons, to have seemed important to practicing or intending scientists of that period. One of these reasons must have been the intellectual climate, which was making an abrupt change from Scholastic methods of inquiry to the Modernist approach. Another reason would have been the simple possibility of being able to carry out experiments in an environment of somewhat reduced political tensions coinciding with the establishment of stable nation states. A final important reason was the pressure, felt by many, of the rising of population that attended upon reduced hostilities and the passage of two centuries of recovery since the ravages of the plague. In soil science as in other sciences, the mid-1600s is a period looked upon as a turning point, from which all the critical steps were taken.

Studies by the English botanist John Woodward, published in the twenty-first volume (1699) of *Philosophia Transactionae*, represent the next step in the development of understanding of soils and plant nutrition. Woodward "reopened the question" of the "vital principle" of plant growth by growing spearmint in rainwater, river water, and water from mud puddles in his garden. Woodward also apparently had van Helmont's experience as a starting point, and so was able to devise a more sophisticated experiment. He found that the plants raised with the muddy water grew the best, so that he concluded "earthy material must be the 'principle'" (Kellogg 1957: 3). Indeed, Woodward "seems to have grasped the essential idea of a flow of solutes to roots induced by the transpiration stream..." (Nye and Tinker 1977: 1). The muddy water was a primitive nutrient culture solution, about which he wrote: "It has been shown that there is a considerable quantity of (terrestrial) matter contained in rain, spring and river water; that the greatest part of the fluid mass that ascends up into the plants does not settle there but passes through their pores and exhales up into the atmosphere: that a great part of the terrestrial matter, mixed with the water, passes up into the plant along with it, and that the plant is more or less

augmented in proportion as the water contains a greater or lesser quantity of that matter; from all of which we may infer, that the earth, and not water, is the matter that constitutes vegetables” (Nye and Tinker 1977: 1-2).

With Woodward’s work, it seems to have been settled that “earthy materials ” play an essential part in plant growth, while it was also acknowledged that water was also essential. At this point, the question became “What role does the soil play?” Surprisingly, this imperative—that there would be or must be only one factor accounting for some phenomenon—once it had been laid to rest as applying to factors of plant growth, seems to have persisted as applied to the soil. Researchers transferred the idea of “one source” from among all possible sources to “one source” of nutrient material from among the materials making up the soils. An exception was Georg Ernst Stahl, famed for his phlogiston theory in the 1720s, who, although a “vitalist,” believed plants used both minerals and organic compounds from the soil, in small percentages, as well as water and air, in large percentages (Krupenikov 1990: 87). Stahl excepted, most researchers continued to be guided by the prescription to consider only one source of nutrient material until and including Leibig in the 1840s.

“Vitalist” theory offered the prospect that plants created all of their constituents, including mineral, from the humus of the soil under the influence of moisture, air, and light by the power of their own life forces. Meanwhile some naturalists of the 17th century, also crediting the plants’ life force, believed water and air were the source of all construction materials; they “attempted to show that plants withdraw only water from the soil and that manures act favorably upon plant growth only by supplying heat and moisture. According to these naturalists, the living power of animals and plants enables them to construct all tissues, including even the mineral constituents, from the atmospheric air and the soil water” (Waksman 1938: 10).

With such assumptions, little technological support and little theory behind them, these researchers had their work cut out for them. Of such periods in scientific fields, Kuhn writes,

the road to a firm research consensus is extraordinarily arduous.... In the absence of a paradigm or some candidate for a paradigm, all of the facts that could possibly pertain to the development of a given science are likely to seem equally relevant. As a result, early fact-gathering is a far more nearly random activity than the one that subsequent scientific development makes familiar. Furthermore, in the absence of a reason for seeking some particular form of more recondite information, early fact-gathering is usually restricted to the wealth of data that lie ready to hand. (1971: 15)

Philosophers of science Carl Hempel (1965: 139) and Stephen Toulmin (1958: 41, 45) have also remarked that a preparadigmatic stage of a science is extremely difficult. Indeed, a science in this stage evokes derision from more established sciences, with the term "natural history phase" being used for it, meaning that there is little or no theory with which to guide observation and distinguish relevant from irrelevant "facts." Also, soil science at this stage could barely be so designated if, as Toulmin (1953: 45) says, "explanatory power is regarded as the stamp of a science." In fact, the study of soils was in such a state until nearly the turn of the twentieth century, and until then farming continued to be guided by empirically supported practice reaching back to Greek and Roman traditions (Viets 1977). Some form of manuring was universally practiced, and marling, a much more sophisticated practice, was being carried on in England even before the Romans (Russell 1913); these and other practices had been profitably followed for hundreds of years, with no hint of theoretical explanation in sight.

Kuhn likewise likens a science in this phase, whatever may be its subject, to natural history:

This is the situation that creates the schools characteristic of the early stages of a science's development. No natural history can be interpreted in the absence of at

least some implicit body of intertwined theoretical and methodological belief that permits selection, evaluation, and criticism. If that body of belief is not already implicit in the collection of facts—in which case, more than “mere facts” are at hand—it must be externally supplied, perhaps by a current metaphysic, by another science, or by personal and historical accident. No wonder, then, that in the early stages of the development of any science, different men confronting the same range of phenomena, but not usually all the same particular phenomena, describe and interpret them in different ways. (1971: 16-17)

The questions being asked about plant growth and soils in the 1600s showed, definitely, the features of which Kuhn speaks in this passage. Soil/plant relationships are so complex, and in the 1600s, technology was so elementary, that there was no certain way to know what was evidence or how to interpret what was entertained to be evidence. Krupenikov writes of “the study or, more correctly, compilation of information on the productivity and geography of soils during the 16th-18th centuries...” (1990: 96). Certainly, accurate interpretations of van Helmont’s work, or quantitative testing of Glauber’s or Boyle’s or Woodward’s studies, were not to be possible until after the phenomenal advances in the “chemical revolution” of the late 1700s to early 1800s, with the result that incipient theory could not be supported thereby. Nevertheless, what had been achieved at this point by science, empirically, by observation and recording (but a principle long reflected in practice) was the unambiguous recognition that some essential contribution to growth was indeed from the soil. The greatest factor contributing to that recognition was research by controlled-variable experiment, with the stages of refinement of the experiment being traceable through the work of successive investigators. This beginning would lead to science for agriculture involving laboratory work, with studies on growth of plants in mineral solutions, to pot culture, and later to field plots, such as those of Lavoisier who carried out both field and laboratory experiments, of Boussingault using both his laboratory and his own property in Alsace, and of Lawes and Gilbert at what would become England’s famed Rothamstead Experiment Station.

2.1.1. England's Agricultural Revolution—Technical and Management Innovations

From 1699 to 1733, Jethro Tull, Oxford-educated for the bar, worked and succeeded at developing some revolutionary implements for agriculture, the avocation he chose, rather than practicing law. Before Tull, heavy wooden implements were used to break up the soil, which then had to be beaten with mallets to prepare a seed bed. Beating was later replaced by harrowing, with the harrow being drawn as many as 20 times over the seeded fields, the horses treading it, Tull opined, "as hard as a highway" (Russell 1913: 52). Desperately needed new crops—turnips, clovers, and cultivated grasses—had recently been introduced from Holland, but a better means of preparing seedbeds was needed before the new crops could be widely used. In the first decades of the 18th century, Tull developed a rotary-action seed drill and a "horse hoe" or cultivator. In combination these implements made it possible to plant wheat in rows and then cultivate between them. Before his invention, weeds commonly choked out the crops, because the seeds were broadcast or sown randomly, and field workers could not pull the weeds without trampling the growing grain. Also before this, to check weed growth, farmers resorted to the destructive practice of bare winter fallowing.

At the time of these inventions, what made plants grow was still an open question; Tull's view was that small particles of soil enter the plants via the roots. Thus he wrote, "Too much earth, or too fine, can never possibly be given to roots...and earth is so surely the food of all plants, that with proper share of the other elements, which each species of plants requires, I do not find but that any common earth will nourish any plant" (in Hopkins 1910: 300-301). Tull believed that if soil were made fine enough by cultivation, "plants [could] take in small particles of earth, especially clay" (Kellogg, 1957: 2), and he therefore insisted that frequent cultivation meant good soil management. While the innovations were certainly effective, the analysis of why it worked was incorrect. Tull's analysis was based on observation of a few soils only ("any common earth"), where it was used, in a particular climate, and for the limited time Tull was able to observe. The success of his innovations contributed via ethos to the strength of his arguments, with the

result that his arguments—for cultivation to fineness and that all soils would respond well to this practice—were well received and became established as the norm (overcultivation has often been the rule since then). The prominent soil scientist, Charles Kellogg, wrote more than two centuries later, that this is “simply one example of how a practice, which is good on some kinds of soil, may be established through observation and advocated for all kinds of soil for the wrong reasons” (1957: 2). The limitations—of time, place and particular substance—on Tull’s observations meant that his arguments, which should have been applied only situationally and with modifications over time, were overgeneralized and became regarded as the rule by which to measure good management.¹⁶

The success of Tull’s system lay in two factors. First, more cultivation brings about more rapid decomposition of soil humus through aeration which enables the soil microorganisms to release more carbon, nitrogen, and chemically held minerals for plant growth. This is a benefit in the short run, and can be practiced for a longer time on loamy soils in cool climates and under conditions of soft rainfall (as in England) than in hot climates or locations receiving torrential rains. Over time, however, even in favorable conditions, cultivation causes both chemical and physical deterioration of soil structure by exhausting the organic matter and depleting the important base minerals held by organic matter. This is unless the organic matter and minerals are substantially replaced and the acid/alkaline balance maintained. The second reason for the success of the new system was that crops now planted in rows and cultivated during the growing season, to prevent the weeds’ competing with the crop, gave much higher yields. This result was an unqualified benefit, since the burning needs at the time were how to intensify crop production and increase yields for rising populations. In this context, Tull’s innovations were positive and revolutionary.

¹⁶ Gross (1990: 4-5) notes a tendency to overgeneralization in the pursuit of scientific theory; see also overgeneralization about the four-course rotation (47).

Part of what was revolutionary about Tull and his work was that he, an Oxford law graduate, concerned himself at all with farming, which at the time was considered very menial work. Tull's was some of the first inventive work that had been done to lighten the toil and raise the productivity of workers in this still-medieval occupation. The backwardness of farming, the nation's growing population pressure, and the straitened conditions poor lands imposed on landowners and their tenants were important factors in another aspect of the agricultural revolution, innovations in management. In 1730, one of the owners of such land, Lord Townshend of Norfolk, and 46 years afterward, his neighbor, Thomas Coke of Holkham, began working "to turn farming from a menial, unprofitable job to a profitable, highly regarded one" (Long 1993: 54). Townshend and Coke began by using, building on, and popularizing bits of recent local agricultural innovations and in taking the leading role in managing their own lands rather than leaving it entirely to their tenants. By doing applied research and insisting that their tenants do the same, Townshend and Coke were among the initiators of England's agricultural revolution (Hervey 1970; Kerridge 1973).

Both had land consisting mainly of droughty sand on the coast in north Norfolk shire; under intensified cultivation and without the addition of large quantities of manure, it would soon have lost any residual cohesive humus. It could not be counted on for winter forage, with the result that when pasture failed there was much human and animal suffering. Traditionally, stock was slaughtered and the meat salted down before winter, because there was no pasturage to carry the animals over winter; under these circumstances herds and flocks could not be built up or improved, and manure was not available in quantity for spreading on the fields (Russell 1913). Whereas this situation or problem context must have been regarded or presumed by most people at the time as just the way things were, Townshend took it as a problem capable of resolution (Holmquest 1983). In this problem context, he began work to reclaim the soils. First he acted to improve the sandy soils directly by marling, and indirectly by working on alternatives to

the old two-year grain and fallow system, and second, he set a course of increasing numbers of livestock.

Marling had been practiced since before Roman times in England and its familiarity was attested by a proverbial saying,

Lime and lime, without manure

Makes both farm and farmer poor. (Hopkins 1910: 162)

Russell (1957: 196) explains that “sand in England [is] almost all composed of inert silica, containing no plant food and has no colloidal or base exchange properties, and consequently very little power of retaining water or fertilizers.” Marl is calcareous, and as calcium is bi-valent, by bonding with minerals in any available clay or organic colloids it promotes crumb formation or flocculation. This aggregation of soil particles helps keep sandy soil from blowing away. Flocculation also creates more macropores, and this structural characteristic improves water-holding capacity. Agricultural experience of the time recorded better physical condition and greater productivity—for whatever other reasons—for marled land, but also that it also needed to be maintained with organic manures.

In addition to marling, Townshend adapted the old crop and fallow system to accommodate a greater variety of crops and to integrate the recognized benefit of legumes in the rotation. Starting in the 1730s, he adapted a rotation from Holland—whence the new crops came—and developed the “Norfolk four-course system”; in it a four-step rotation replaced the ancient two-year crop and fallow. A four-course rotation, “applicable, perhaps, to all the soils of the world,” as presented in 1792 by Arthur Young (Kellogg 1957: 2) called for

- (1) Roots, cabbage, or pulse,
- (2) Corn [wheat],
- (3) Grasses, and
- (4) Corn again.

Another course called for

- (1) Roots (turnips) or cabbage,
- (2) Corn,
- (3) Grasses,
- (4) Pulse, or maize, hemp, or flax, and
- (5) Corn.

Turnips were later augmented by other root crops—mangolds and swedes. By using such a rotation over the fields of a property, there would be enough variation in the crops grown every year that if one crop failed, there would be another to fall back on: “Coke widened the range of fodder crops so reducing the risk of the disastrous hungry gap that would occur if one of them failed” (Russell 1913: 196).

Whereas the previous system had neither supported stock nor provided manure, the new system relied on both. This system both required and made possible greater numbers of livestock. Liming the soil made it more fertile in the short-term, with the result that the soils could support the year’s growth of root crops and hay for the stock and grain for consumption and market. But liming facilitates the release of nitrogen and basic minerals from what little organic matter there is in a sandy soil. Therefore, more manure is required to maintain long-term fertility, and the greater numbers of animals therefore complement the arrangement by counteracting this depleting effect of the lime. Thus, while liming makes the soil require more organic matter, with the stock that is now supported on the farm this is available in the form of their manure and from the residue of the root crops grown to feed them. In this system, livestock would be pastured in the grass and legume fields during the summer months and some hay would also be cut. In the winter they would be turned out into the turnips where they would forage until pasture was available again. In the year or two years of “Grasses” in this regime, the clover or mixed grass and clover rotation would allow for nitrogen fixation from the clovers and by other microorganisms. (The benefits to the soil of a long grass/legume rotation had long been recognized although nitrogen fixation by rhizobia associated with legumes was not

known, much less understood.) These practices were recognized as effectively meliorative, but without knowledge of why; particularly, there was no idea of the part played by microbiological activity and by organic matter in improving tilth chemically as well as physically, and by contributing to base-exchange capacity. This situation illustrates Böhme *et al.*'s view of the stages of finalized science, in which scientists (and in this case farmer-scientists) aiming at social goals are apt to accept a high degree of theoretical uncertainty and proceed, as pragmatists/empiricists on the measure of control provided them by the practice itself; absent theory, the English reformers were doing "what worked." Later, Laws and Gilbert's test plots, which measured the effects of different practices, were an important source of reliable guidance to theory about soil.

Townshend's rotations and other management innovations of the mid- to late 1700s contributed to the doubling of wheat yields from about 1780 and laid the basis for the industrial revolution in England. The movement towards improved management on scientific principles thrived throughout the last quarter of the century. At Holkham in 1778, Coke instituted annual shearing days—"Coke's Clippings"—which were the forerunners of the agricultural shows. Shearing days featured the newest agricultural inventions and provided an opportunity to the thousands who attended to exchange ideas and information about practices, and to exchange seed and planting materials. Furthermore, it provided a venue for trading livestock for breeding up, which was now possible as the four-course rotation enabled the overwintering of stock (Long 1993: 54; Russell 1913).

Scientific agriculture in England also gained an advocate in the 1780s and '90s in agricultural writer Arthur Young, who traveled in England and on the continent and reported on his observations of various agricultural practices. Young offered comparisons and recommendations in British agricultural journals in the 1790s. By this time, England had several very strong journals which actively supported farmers' interests in the latest agricultural improvements, and since the farming sector included tenants, land-owning gentry, and landed aristocrats (who spearheaded much of the improvement), it can be

understood why a broad spectrum of British society maintained an interest in these developments. From around the 1750s and certainly by the 1790s England was regarded as practicing the most advanced agriculture of all the European nations (Jacks 1962; McHenry and van Doren 1972; Rossiter 1975).

Now alert to the improvements in rural and national life that could be brought about by improvements in agriculture through science and management, England's agriculturists must have been intent on making a good thing better: they became massive importers of nutrients in manures and in feeds. Beginning in 1774, England employed bone as a source of phosphate, first using domestic sources, and later importing human bones from battlefields across Europe,¹⁷ and from catacombs in Italy. Although the first European to become aware of the guano deposits, Humbolt, was from Saxony, England were first By 1855, England was importing more than two hundred thousand tons of guano. At the end of the 19th century, it was importing nearly a billion pounds of oil cake, two hundred million bushels of wheat and one hundred million of corn, and was using over one hundred thousand tons of basic slag for phosphate in addition to much more phosphate fertilizing material, including one hundred thousand tons of bones from India, and bone ash from South America (Hopkins 1910: 323ff).

2.1.2. Discoveries and Advances in Chemistry and Scientific Technology

Before and during the period that soil-improving management and technical innovations were being undertaken in England, significant discoveries were being made in chemistry there, and also in Northern Europe, France, and Scotland. Northern European chemists Becher, Stahl, Wallerius and others were analyzing soils with techniques originating in geology. Joseph Black in Scotland had analyzed and classified calcareous marls by the late 1700s (Tandarich and Sprecher 1994). In the late 1700s, "the Chemical Revolution"

¹⁷ Sir E. John Russell, director of Rothamstead Experimental Station for thirty years, writing about the development of superphosphate and the fact that it replaced bones, which had become scarce, says, "indeed, Leibig in an angry outburst declared that [the English] were ransacking the battlefields of Europe [for bones]" (1957: 140).

brought answers to a number of long-standing questions. Between 1766 and 1781, the gasses oxygen, hydrogen, and nitrogen were discovered. Antoine Lavoisier established phosphorous as an element in 1772, drew up a table of elements, and with Thomas de Sassure, perfected the quantitative balance, sensitive enough to detect weight differences so minute that they were able finally to resolve the question of van Helmont's missing two ounces (Board 1994).

Beginning in 1807, Englishman Sir Humphry Davy discovered the elements sodium, potassium, calcium, magnesium and chlorine gas. From his work during the 1790s and into the first decade of the 1800s, Davy gave a series of lectures in which he reviewed the current chemical knowledge and in so doing he also listed and described all the apparatus and procedures used in experimentation related to soils and plant/soil nutrition. This compilation, published in England in 1813 (1815 in America) as *Agricultural Chemistry*, is often pointed to as a pivotal effort in standardizing experimental practice, establishing a shared vocabulary for agricultural chemists, and bringing about agreement (later changed) on what constituted soil research problems and data (Krupenikov 1990: 134-135). Studies of innovation have found that the development of tacit understandings among scientists are critical to the progression of knowledge; such understandings may seem “not like learning items of information, but...more like learning a language” (Collins in Frawley 1987: 93). Despite the many unanswered questions in the lectures, many agricultural chemists considered Davy the founder of the discipline, particularly for establishing these shared observable features (Wiley 1899; Krupenikov 1990).

In the pursuit of studies that led to soil science, a few experiment series stand out. Experiments by Priestly showed that, in the presence of sunlight, plants gave off from their leaves a gas he identified as oxygen. Selman Waksman in *Humus* (1938) summarizes other critical studies, including their strengths and their weaknesses:

Ingen-Housz clearly recognized that leaves absorb from the air a gaseous constituent which they use in the presence of light to produce the combustible

bodies in the plant. Senebier suggested that the process of CO₂-assimilation involves reduction of this gas in the presence of light and results in the excretion of oxygen; he believed, however, that the CO₂ necessary for the nutrition of the plants did not come chiefly from the air, but was absorbed by the roots from the soil and was brought to the leaves with the rising sap. The last conception was responsible for the fact that the humus of the soil was for a long time considered to be the true source of carbon for plant life. Only Th. De Sassure succeeded in clearly establishing the importance of the gaseous exchange in the nutrition of plants. (12)

De Sassure had established definitely that plants synthesize their organic matter from carbon dioxide and water, and give off oxygen, and that the CO₂ comes from the atmosphere, not from the soil. He also emphasized that while the minerals are in very small amounts "as compared with the total plant mass produced," they are essential and are derived from the soil, not from water or air. C. G. Hopkins, in *Soil Fertility and Permanent Agriculture*, stated that with this work, de Sassure "gave to the world the first definite information which could serve as a scientific basis for soil improvement" (1910: 327-328). De Sassure did not have a settled opinion on another important question, that is, whether the nitrogen was from the soil or from the atmosphere, but he believed that it might be in the form of ammonia gas, from animal extracts, or from some water-soluble form (Waksman 1938: 12).

Despite de Sassure's experiments, published in 1804, most of the chemists working on soil fertility at the turn of the 19th century were predisposed to the idea that the plant's carbon came from the soil. They therefore adopted Senebier's earlier and incorrect view that the plant's source of carbon dioxide is the humus in the soil, while they overlooked his correct conclusions regarding the assimilation of carbon dioxide and the excretion of oxygen (Waksman 1938: 12). Senebier's opinion on this matter contributed to the parentage of scientific content for chemists working in the humus paradigm; awareness of the high organic contents of fertile soils from long agricultural practice was the theory's

empirical parentage. Senebier's opinion that the plant's source of carbon dioxide was the humus in the soil, was published in 1792 and again in 1800. From then into the 1830s, the organic carbon-hydrogen component was held to be the chief source of plant nutrients; this was the view of the majority of agricultural chemists, including such prominent experimental chemists as Chaptal and Boussignault in France, Humphry Davy in England, A. D. Thaer, Carl Sprengel and others in Germany, and in Sweden, J. J. Berzelius and his students including Mitscherlich and Mulder (Tandarich and Sprecher 1994).

In the 1899 USDA *Yearbook of Agriculture*, Dr. H. W. Wiley, Director of the Bureau of Chemistry in the US Department of Agriculture, described the prevalence of the humus theory in the earlier era:

Previous [to 1840]...it was commonly understood that organic substances, such as sugars and oils, were the chief foods of plants, either in the fresh state or in the partially decayed condition known as humus. In fact, the attitude of chemistry toward agriculture in the first four decades of the century was so strongly marked on this point that the whole system of plant nutrition, as understood at that time, might with propriety be designated the humus theory. (219)

What caused this inability to recognize de Sasseure's and Ingen-Housz's experiments as definitive, and to take full account of their implications? The prominence on the disciplinary scene of a number of other elements might be recognized as contributing to a sort of cultural lag in this case.

The most obvious and pervasive element of support for the humus theory was, of course, that both experience and observation over thousands of years of agricultural practice underlay it. Incorporating or spreading manure, which included or produced humus, had stood for millennia as a hallmark of good agricultural practice, known to improve conditions in the soil and yields along with them. It was also by now recognized that humus could be increased with pasture rotations, which provided roots and decaying plant

material, along with manure, which would be incorporated into the soil when it was cropped again. Likewise, it was highly desirable to farm "new ground," land that is newly cleared of forest growth and therefore has topsoil with high percentages of humus. "From the oldest times down to the present," wrote E. W. Hilgard in 1906, "a 'rich black soil' has commanded attention and approval...the black and black-brown colors being almost invariably due to the presence of much humus...It is obvious that the farmers' judgment coincides with a high estimate of the agricultural value of humus" (283).

By this time, the new intensified tillage would have been making inroads into the organic matter stores of English soils, on properties where they were not being adequately replenished with manure, on Tull's theory that repeated plowings replace manuring (Waksman 1936:332). However, rotations to help restore humus were becoming widely adopted in England, whence they were beginning to spread to the continent. In his *Introduction to the Knowledge of English Agriculture and its New Practical and Theoretical Advancements* (3 vols., 1798-1804), A. D. Thaer included a description of these tested, beneficial, management innovations to promote their adoption in Germany. This "knowledge" was purely empirical, but it was proving reliable as a basis of practice for control (though not for theory, as will be seen).

As a result of this long recognized connection between fertility and organic matter, scientific interest in organic matter had begun early. Precursors to the humus theory had been on the scene for some time. Swedish chemist Wallerius had been the first to describe humus, in 1761, and his views on it were supported by reports of the fertility of the chernozem soils in Ukraine, Russia, and Hungary. In the late 1700s, Thaer gained reputation for this view with his work at Berlin University and the Agricultural Polytechnic Institute which he founded in Meglin on the management principles developed in England (Krupenikov 1990: 91, 92). Through the work of these researchers, humus was well established on the conceptual high ground on the issue. Wallerius associated humus with *magma unguinosum*, "the fat of the land," and regarded it as "the major source of

fertility" (Waksman 1938: 11). It was being frequently asserted, e.g. by Hunter and other essayists in the 1777 *Georgical Essays*, that "oils" and "fats" were the true food of plants. Hunter wrote, "I lay it down as a fundamental maxim that all plants receive their nourishment from oily particles incorporated with water, by means of an alkaline salt or absorbent earth" (Hopkins 1910: 303). Hunter is here speaking of the mucilaginous phase of decaying plant material.

It may be asked, whence do natural soils receive their oily particles?...The air supplies them. During the summer months, the atmosphere is full of putrid exhalations arising from the steam of dunghills, the perspiration of animals and smoke. Every shower brings down these oleaginous particles for the nourishment of plants. (303)

Again the issue of language arises; Waksman says of the humus theory that it integrated this language into explanatory theory in specific questions. For example, they held the action of lime to be that it dissolved the "fatness" of the humus, while the function of clay was to fix and retain this fatness. The available language for these phenomena was then connected inextricably to the covering theory. Waksman connects this use of language with the resilience of the misconception: "These ideas entered the agronomic literature, and the 'fat of the land' became synonymous with soil humus and the supply of plant nutrients" (Waksman 1938: 11).

Rossiter, in *Leibig and the Americans*, points out a further problem that was preoccupying Davy and other chemists at the time, the problem of the "transmutability of air particles into earth particles" by and within the plant. Believing that humus was plants' "more essential food," and that it supplied carbon and nitrogen to the plants, humus chemists had, nevertheless, to explain the presence of minerals which had been detected in their structures. Humphry Davy's hypothesis that mineral manures "acted upon" the organic food of the plant to "render it more fitted for the purposes of vegetable life" (Rossiter 1975: 16-17) conformed with the observable action of lime on soil physical condition, and thus provided a plausible account. However, although this

account had explanatory power for some of the phenomena related to mineral manures, it involved a self-contradiction and, furthermore, could not explain the process involved; this process was cation exchange, not identified until around 1850, and not accepted or elaborated until much later. Investigators in the mid-1800s found much to criticize about Davy, but in 1899 H.W. Wiley, Head of the US Department of Agriculture's Bureau of Chemistry, had this view of Davy's work in the early 1800s:

It is not to be wondered at that the views, even of the most advanced kind, concerning the composition of plants were of such and erroneous nature when we consider the state of analytical chemistry at that time. Sir Humphry Davy was, without doubt, the most skillful and accurate chemist of his time or of any time that preceded him, and the wonder is that he could have reached such correct results with the methods of analysis which he himself describes. (215)

By 1840, vitalist questions that had preoccupied Davy had been mooted and had dropped out of sight; this allowed for de Sussure to be rediscovered and "read in a different light" (Rossiter 1975:208 n 9).

Despite its vitalist orientation and problems in accounting for minerals and the source of carbon, humus became established as the predominant research paradigm. It was given authority by experience (in which judgment is rendered by nature), and by the earlier scientists. It was supported by Tull's authority, owing to his practical success which seemed strongly to attest to the rightness of his views (Waksman 1938: 11), but it was also precipitated by the exhaustion of humus in soils now more intensively cultivated, again on his authority. Kuhn's normative (rather than cognitive) influence of paradigms (1970: 107) can readily be recognized in these influences. The humus theory had developed into a weak, early paradigm, persuading by the force of effective practice, the ethos embodied in its practitioners and their views, and in its texts. This influence clearly affected the ability to "see," and see the significance of, experimentation that suggested a differing interpretation.

At the turn of the 19th century in Germany as in England, agricultural chemists were working on soil fertility problems with emphasis on the soil organic matter. The foremost investigator was A. D. Thaer, author of *Fundamentals of Rational Farming* in 1809-10. Thaer was regarded as the principal proponent of the humus theory; he stated in 1798 that "plants thrive principally on humus in solution" and in 1808 that "fertility of the soil depends largely upon it, since, besides water, humus is the only material which supplies the nutrients to plants" (Waksman 1936:13). One of the main undertakings of the first thirty years of the 1800s was to analyze various plants and parts of plants at different stages of growth and to identify the organic and inorganic constituents in them. Humus chemists at this time were concerned with identifying the compounds that constituted humus, and, to some extent, comparing these to the findings of the constituent analyses. Some of the experimentation had lasting value, such as Sprengel's work on humic acids and mild humus, and the techniques he developed; these remained current until at least the late 1930s (Waksman 1938: 24-25). But humus research, unfortunately, was in a state of confusion in the 1830s. Analytical techniques during the period had advanced and become more standardized, but there was no standardization of the terms of research, of the materials from which the humus derived, or of the environment in which it developed. There was a complex and proliferating terminology for the "humic acids" and a fundamental "lack of understanding of the process of humus formation"; decomposition processes were still thought to be far less complex than they were, chemically, and there was no awareness whatsoever, at this time, of micro-organic processes (Waksman 1938: 4, 34). Furthermore, "normal science" analysis continued to confirm mineral elements in plant bodies, and this constituted an anomaly, as it could never be adequately explained.

Lack of guidelines for uniformity of materials generated considerable problems.

Researchers working with detritus from a pine forest would have results differing completely from those of a researcher working in a deciduous forest. Waksman stated: "The forest literature is full of contradictory statements concerning the importance of humus in tree growth; some investigators claim that humus is highly important in forest

soils, because of its physical, chemical, and biological properties, while others go to the other extreme and state that the best humus is the one that is never formed." This is because something so basic as the influence of forest type was never included in the considerations. Extracts were being prepared with mineral acids from materials so diverse as decomposed tree stumps, sphagnum peat bogs, soils, manure composts, coal, "and even filter paper," and the findings seem to have been taken as comparable or identical (1938: 241, 236, 62). When soils were being treated with various mineral acids to extract the "humic acids," because of the variability of soils, each soil's composition yielded different acids. As a result, these extracts and their names were being "multiplied *ad infinitum*," and all this without the recognition that the acids produced were actually products of the laboratory, and not naturally occurring substances (Waksman 1938: 31, 28-29).¹⁸ Here, indeed was a case of "the facts" being socially constructed.

Waksman (1938: 62) summarized the problems of the humus theory as follows:

1. The introduction of numerous terms for designating the dark colored constituents of humus, without due recognition of the fact that these represented only preparations and not specific chemical compounds.
2. The persistent designation of various preparations of humus obtained by alkali treatment of soil, peats, coals, and composts, by terms which were introduced when organic chemistry was still in its infancy and when all organic compounds were considered as comparatively simple in nature.
3. The confusion introduced by non-critical comparison of artificial preparations obtained by treatment of sugars with inorganic acids with natural preparations from soils, composts, peat, or coal.

¹⁸ E.g., Waksman (1936: 333): "...the treatment of humus by alkalies and acids frequently results in a change in the chemical nature of the complexes which tends to render them even more resistant [to decomposition]" so that "indestructability" is not valid as a defining (definitional) characteristic of humus, as Thaer and Hoppe-Seyler believed. See also pp. 29 and 199.

4. Lack of knowledge concerning the role of microorganisms in the processes of decomposition of plant and animal residues leading to the formation of humus.

In a complex of research dynamics with these characteristics, fundamental scientific principles are not being applied. The confusion of data, in the first place, seems to have set the field back to the status of a "natural history"—recording observations without benefit of theory, not even "enough theory" to distinguish between "made" substances and natural ones, or to allocate importance to differences in origin of the materials under scrutiny. Prelli's interpretation of the stasis questions for scientific issues suggests that questions of *existence*, *meaning*, *value*, and *action* may be brought to the analysis of scientific problems (1991: 147). Although there should have been clashes at the stasis points of existence or evidence—whether these compounds exist in nature—and value—whether they were significant as objects of science or practical application—to test the associated claims, these questions seem never to have been asked. For example, as humus was considered *a priori* to be the proper food of plants, a blanket positive qualitative assessment was applied to these substances, so that Davy's distinction between mild and acid humus was lost sight of, and the problem of acid peat and heath soils continued undiagnosed. Another persistent anomaly that was not being addressed was the "nitrogen hungriness" of humus, which would not be sorted out fully until Waksman's studies in 1933 (Jenny 1961: 67).

Most damaging of all to the humus investigators, however, was the lack of an external goal, a feature that characterizes "finalized science." English management innovations in England and carried on by the Thaer in Germany, where they were bringing increases and stability in yields (Krupenikov 1990: 92). Thus the depletion of humus was recognized as a problem, but for humus chemists engaged in laboratory work, problem formulation extended to identification, beyond which no program had been proposed or projected to test and confirm the value of the substances under study. Nor was there any projection of an endeavor to produce and make available greater quantities of humus, or to determine

by any other means how to turn their work into something of social value. So, while researchers working on humus were justified in associating humus with fertility, they could not describe how humus contributed to fertility or demonstrate how humus was connected to fertility. Had they been able to establish these points, they would have been able to move from that recognition to recommendations to agriculture for increasing humus. The obstacles preventing these breakthroughs, as we have seen, were their belief in direct absorption of humus, the general disarray of terminology, and the absence of microbiological understanding and technology. As the humus theory nevertheless held the conceptual ground in 1840, as phlogiston had earlier, it remained the task of someone not in this tradition to bring the gaseous exchange findings of de Sassure, Ingen-Housz and Senebier to the fore. "The great change in agricultural theory" came in 1840, with Justus von Leibig's publication of *Organic Chemistry in Its Application to Agriculture and Physiology*.

2.2. THE NEW PARADIGM: AGRICULTURAL CHEMISTRY

2.2.1. Leibig and Nutrient Cycles: The Setting for Change

Justus von Leibig was already a respected organic chemist in the late 1820s. He had studied with the French chemist Gay-Lussac in Paris and upon his return to his home district, Giessen in Hesse-Darmstadt, he established and built up a laboratory there with the patronage of von Humbolt. At its initiation in 1824, Leibig's was the very first laboratory, and in the late 1820s and 1830s "one of the few," chemistry laboratories, in Europe accepting students. Leibig became renowned for his work to improve analytical techniques, mainly by standardizing them, and for his inspired teaching of students in pharmacy. He also was the co-editor, with Friedrich Wöhler, of a leading international chemical journal, the *Annalen der Chemie und Pharmacie* (Rossiter 1975: 26-27). Leibig was working in the mainstream of organic chemistry, which was dedicated to applying "the principles of natural philosophy to the development and nourishment of plants" (Krohn and Schäfer 1976: 35) and ascertaining the chemical processes of organic substances in nature or the chemical conditions of life. In 1828, Leibig and Wöhler in

collaboration synthesized urea in the Giessen laboratory. After Leibig and Wöhler's achievement, organic chemistry took "an entirely different turn...[as] most chemists became convinced that all organic compounds could be synthesized." Organic chemists then directed their efforts exclusively towards "the chemistry of carbon compounds," and so abandoned the chemistry of vital processes (Krohn and Schäfer 1990: 35-36).

Organic chemistry was in this state when Leibig visited England in 1837, coincident with the annual meeting of the British Association for the Advancement of Science. While there he was invited by the chemical section of the Society to prepare a report on the state of organic chemistry, covering the research that had been carried out since Davy's lectures and book. The report they received three years later was *Organic Chemistry in its Application to Agriculture and Physiology* in which, rather than addressing organic chemistry as it then was, Leibig had unexpectedly nominated agricultural chemistry as his topic and discussed the state of this stream of chemistry, and offered his own views on it. Given the opportunity of an audience for the report of contemporary chemists and other working scientists, Leibig wrote the first (1840) of his several volumes on the subject, not on carbon compound chemistry, but on agricultural chemistry, or "chemistry in the biological mode." The result was that Leibig's "adherence to the goal of a chemistry of vital processes led to a special development—theoretically as well as empirically" (Krohn and Schäfer 1976: 36). The appearance of the book was almost a completely new direction for Leibig and was a surprise to the Association; the publication of the book, furthermore, is now regarded as marking the birth of the science of biochemistry. Soil research itself took a completely different turn after the publication, in what ways we will consider shortly. But first it would be appropriate to look at the circumstances of this change of direction in more detail, as a review of commentators' conjectures on this point will reveal what they have regarded as elements of the rhetorical situation, that is, what set of circumstances Leibig was responding to.

Why and how Leibig took the direction he did at the time he did are a matter of speculation. It is clear that Leibig had been only mildly interested in agricultural chemistry before this time, or perhaps did not recognize the potential importance of the subject—for underdeveloped Giessen, for example. Before 1840, he had accepted and published only three articles in the *Annalen* on agricultural topics¹⁹ and had not himself written on agricultural topics. One possible explanation for the change of mind is that he was influenced by interaction with English agriculturalists and agricultural scientists; it is known that his visit with the chemist Thomas Graham “turned his interest toward” phosphates (Rossiter 1975: 27). While he might also have been influenced by scientists who were attending the Society meeting, or heard a paper on topics touching ideas he presented in the book, Leibig apparently did not attend much of the meeting, due to difficulty understanding the language.

Another conjecture Rossiter introduces is economic conditions prevailing in his region. Giessen lay “at the very heart of an extremely poor agricultural area of Germany” (1975: 27) in a time of rapid population growth. In the 1830s and 1840s, there were in Germany organizations formed to help impoverished families emigrate to America, and because the situation locally was so severe, the Giessen Emigration Society was one of the most active of these. Rossiter suggests that perhaps this group had approached Leibig to apply himself to “the humanitarian goal” of improving agriculture (46). Despite Leibig’s having lived in the impoverished Giessen area most of his life, however, Rossiter found this reason (pathos) unlikely as the trigger for his change of focus. Such a reason is countered, in her view, by the fact that there is no evidence that he had been interested in agricultural chemistry before going to England in 1827.

Rossiter also offers a third possibility, that he had been led to the problem by a paper of de Sature on fermentation:

¹⁹ These were by Macaire-Princep in 1833 on rotation of crops, by Sprengel in 1835 on the constituents of wood ashes, and by Boussignault in 1837 on nitrogen in cattle feeds (Rossiter, p. 28).

Leibig wrote the book in 1839 just after Sassure had published a paper on fermentation which discussed the humus. Leibig immediately noticed it and used it to bolster his own arguments for an oxidative-catalytic theory of fermentation, which was also his major concern in the second part of his book. Perhaps Sassure led him to the humus, and he then taught himself rapidly all he could about agricultural chemistry. This explanation seems fairly likely, since it would explain Leibig's great interest in the humus in 1840 and his feeling of rediscovering Sassure's earlier work. (1975: 28)

Assuming his rediscovery of Sassure led him to humus research, and that Leibig then quickly followed the threads of research to acquaint himself with the relevant issues in agricultural chemistry, he would have found that its paradigm had been established in a period when more was known about the gases in physiological processes and much less was accurately known of minerals, and that the humus paradigm had been followed with little amendment in the interim. It is certain that Leibig's review of the intervening decades' work would have revealed experiments—more numerous and more exact than those done by or available at the time of Davy's or de Sassure's earlier work in which researchers repeatedly confirmed the presence of minerals in plant bodies, but treated these findings as anomalies or counterinstances or offered a vitalist explanation for their presence. It seems certain that in the course of reporting on "the state of" the science, Leibig would have reviewed the record of experimentation and noted this recurring confirmation of minerals and uncertainty about their importance. Perhaps once he had become aware of the potential of chemistry to contribute to improvements in agriculture, Leibig pursued the idea further.

Krohn and Schäfer place more emphasis on the possibility that Leibig was sensitive to the demographic situation and its connection to agriculture. Certainly he was by the 1860s; in the seventh edition of *Organic Chemistry...* (1862) he explicitly advanced the conflict between increases in population and static production as an argument for the application

of organic chemistry to agricultural problems (Krohn and Schäfer 1976: 31); he wrote that productivity can only be sustained

...under two conditions: 1. If, by a divine miracle, the fields regain their yield capacity which folly and ignorance have taken from them; 2. If beds of Guano manure are discovered which are, for example, as large as the English coal fields. No reasonable person will say that the realization of these conditions is either probable or possible. For their self-preservation, nations will be compelled to slaughter and destroy each other in cruel wars in order to obtain balance....

From what Leibig goes on to say, it is clear that this threat was very vivid to him and that he was endeavoring to give it emphasis and rhetorical presence—illustrating from earlier and recent experience in his region—for his readers to be similarly moved by this threat:

...and if, God forbid, there are two years such as the starvation years of 1816 and 1817 then those who live through them will see hundreds of thousands perish in the streets, Add a war thereto and mothers, as during the Thirty Years' War, will drag home the bodies of the slain enemy in order to still with their flesh the hunger of their children; as in Silesia in 1847, the corpses of animals having died of diseases will be excavated in order to prolong the agony with the carcass. These are not vague and dark predictions, images of a sick phantasy; for science does not prophesy, it calculates; not *if*, but *when*, is undecided. (31)

Contrasting with Rossiter, Krohn and Schäfer see Leibig's problem solution as motivated primarily by the interests of society, that is to produce "big and ever increasing harvests for ever and ever" (1976: 40). If Leibig's "society," his cultural and geographic setting, is taken as the main impetus for his theoretical innovation, and Krohn and Schäfer do so take it, then factors about his surroundings would be taken as "default" factors he addressed in formulating it. Important factors would include the extreme poverty and high population of the agricultural laborers, and the very small size of the plots they worked, lack of capital for machinery or other improvements, and intensive management because of the high availability of labor (Rossiter 1975: 58; Yates 1940: 414ff.). Feudal

restrictions on peasants, it should be noted, were not lifted in the German kingdoms until after 1800, and not until after the 1848 Revolutions in many cases. The population was suffering for these reasons and because of the continuing drafts by the nobility on the resources of the region for defense and to pursue territorial ambitions, and because of military engagements taking place in and around Saxony. Perhaps these conditions explain Leibig's lack of enthusiasm for the visionary Norfolk innovations, in which long rotations depended on large properties under one management and, typically, the concern of the landowner in raising the productive potential of the land. While one might speculate that Leibig did not know about or understand the degree to which the English system was improving soils and yields on a sustainable basis, it is equally likely that he rejected this approach because small-holders would not be helped by it, lacking both the land and capital resources to undertake an approach to fertility based on rotations and more extensive agriculture. Furthermore, this view could explain Leibig's attitude of impatience, because a full four or five years' rotation would be required to get the full beneficial effect of the English system, while the farmers in the Giessen area needed to make bigger crops immediately.

Consistent with a response to a social exigency, Leibig's solution, agricultural chemistry, went beyond the discovery of laws of reproduction governing vegetable life—e.g., the descriptive goals of humus research—and had as its object the construction of reproduction cycles, in nature, that exceed natural goals for the benefit of man, that is, establishing norms that go beyond natural goals to address social goals. "Thus," write Krohn and Schäfer, "agricultural chemistry becomes a social science operating with the methods of natural science":

At first sight this seems to be the usual connection between science and technology: once discovered natural laws enable technical constructions within nature. But in agricultural chemistry the social purpose is not the *result* of scientific knowledge but the *condition* of scientific inquiry. Agricultural chemistry

does not provide insights into *why* agriculture functions but rather constitutes a design of *how it must* function. (1976: 40)

Agricultural chemistry so conceived would allow for the concept of “increase,” which is significant not in the framework of natural systems but rather within that of human goals. Because it foregrounded the concept—the “law of minimum return”—that what goes into a plant and is taken away is lost from that location, “the heuristic relevance of the category of the cycle can scarcely be overestimated” (Krohn and Schäfer 1976: 42). Leibig foresaw and worked towards the day that artificial manures of many specific kinds would be inexpensive and available and could increase the crop, without need of other capital expenditures.

The “complex of persons and things” making up this rhetorical situation included additional affective as well as cognitive influences. It is known that Leibig felt “insuperable disgust and aversion against chemistry's activities...” around this time (Krohn and Schäfer 1976: 46). This development in organic chemistry, carbon compound synthesis, may have made agricultural chemistry seem by contrast a relatively more worthy stream of study, aimed as it was at the practical goals of improving agricultural practices. He reportedly saw the current organic chemistry as “asocial” and without potential for useful applications in medicine, industry or physiology: he wrote to Berzelius, “If physiologists and agronomists do not use the culture of chemistry, no permanent and valuable progress can be expected in physiology or in agriculture” (Krupenikov 1990; Leibig in Krohn and Schäfer: 1976: 46). In addition, Leibig's writings make it clear that he was familiar with T.R. Malthus' work as well as the political economy of Adam Smith and John Stuart Mill. Krohn and Schäfer see Leibig's cycling of nutrients as reflecting the metamorphoses discussed in contemporary political economics, and Leibig as anxious to put chemistry to work to increase yields so as to prevent Malthus' prophecies' being fulfilled. In this regard, he criticized the Thaer school for relying mainly on technological improvements and managerial adjustments to raise incomes while, as Leibig said, exhausting the capital: soils. Thus with respect to both the

humus researchers and the Thaer school of practical agriculture in Germany, there was a “push” away from the old paradigm to complement the “pull” towards a new one.

2.2.1.1. The Theory of Nutrient Cycling

However Leibig’s change in perspective became possible, it brought a clear break with the past in agricultural chemistry. The novel and compelling aspect of Leibig’s report was his reconstruction of existing knowledge into a theory, the core of which was nutrient cycling. He assigned the sources of mineral elements to the soil and of the gaseous elements to the atmosphere. Leibig identified a handful of unassailable experiments, and, choosing among and reordering available “sayables,” e.g., about minerals and the source of carbon, he maintained that plant nutrition is a purely chemical process in which water, atmospheric carbon, ammonia, nitrate and the mineral salts of potassium, phosphorus, sulfur, etc., cycle through soil and plant and animal bodies and the atmosphere, entering into different compounds at different times. In its various aspects, Leibig’s hypothesis is referred to as the “mineral theory of plant nutrition” and the “law of the minimum” or “balance sheet” theory of plant nutrition, for its conception that nutrients disappearing from one site or structure will have been incorporated into another (metamorphoses). The concept of cycling gave rise to a second law—the law of complete return: “The main element of farming lies in the fact that the soil must get back everything that has been taken away from it. It is the unchangeable law of nature” (in Krupenikov 1990: 128). Thus, he asserted with de Sussure that the minerals, though present in small quantities, are nevertheless essential in plants and in animals, and are passed, in natural settings, from soil to plant to animal and back again.

Leibig cast his theory in terms of postulated origins, pathways, and destinations of nutrients; his building blocks were the achievements of his predecessors, but his conceptualization of their relationships was entirely new. He acknowledged this point himself:

In my agricultural chemistry I have simply tried to put a light into a dark room. All the furniture was there, even tools and objects of comfort and fun; all these things, however, were not clearly visible to the society using this room for their welfare and to their advantage. Groping haphazardly one person found a chair, another one a table, the third one a bed in which he made himself as comfortable as possible; yet most were blind to the harmony of the furnishings and their interrelationship.... Chemistry, this light of knowledge, can never without harm be removed from this room. (in Krohn and Schäfer 1976: 35)

This inventional coup was a step back from the details of the data of experimentation to assert a functional relationship among the entities in which these elements appeared. As Schlieffer says, in "Analogy and Example," in the journal *Criticism*, scientific modernism replaced synecdoche (causal explanation) with metonymy (functional explanation). Leibig's shifting of the scientific perspective, from the end points of the processes to the conceptual structure through which they have usefulness and meaning for human purposes, represents a good illustration of this idea. Metonymy is analogy using *accidents* of the subject of inquiry—which Leibig did in asking where elements with these features appeared and reappeared—whereas synecdoche, analogy based on essential qualities, focuses on an *essential* feature or quality of the subject, as the humus researchers did in persisting in research into the essential nature of humus. Leibig's was truly an architectonic use of rhetorical invention, to discern that there might be pathways taken by the elements which are otherwise known only through their appearances in different guises (various compounds) in plants, soils, and animals. Leibig's reconstruction of existing knowledge, Krohn and Schäfer say, formed "a comprehensive, in principle reliable, and applicable paradigm. It claimed competence to elucidate agricultural processes, to construct these processes according to human needs and to integrate economic and technical knowledge" (1976: 35).

With Leibig's view, the researcher reads the trail of the nutrients through their physiological and economic uses—soil nitrogen or phosphorus leaving the farm as crops and animals sold—and aims to find new sources or synthesize these elements so as to achieve the aim Leibig set, for “big and ever increasing harvests for ever and ever” (Krohn and Schäfer 1976: 40). Humus researchers were, by contrast, seeking to pinpoint the essence of humus by breaking down and analyzing parts of the humus materials, but without productivity-related objectives, and without reference to any postulated uses of humus. In the case of Lavoisier's work “much of the difficult work was done not in the laboratory, but in the refining the language in which the question is asked” (Bruffee 1986: 785). Leibig's most significant work, like Lavoisier's in this sense, was not laboratory work, but a schematization, arraying in a new pattern certain “facts” that had already been arrived at by Lavoisier, Sussure, Senebier and others (Hopkins 1910). This was a different kind of “seeing” altogether and, it might be argued, could be done only by someone who was a newcomer and not a party to the original paradigm; Kuhn (1970) discussing revolution in science and Arieti (1976) discussing the nature of creativity both remark that such an innovator is usually young and must be trained in the field but not committed as yet to a certain “camp.” McKeon (1987 [1966], [1971]) describes rhetorical invention of this kind as the architectonic theoretical art which acts to order all categories of information; here invention in that sense was clearly at work. And that it is rhetorical invention, not logic, is apparent in its chief goal, the social goal of determining suitable action to stave off the expected famine, a goal that clearly includes sympathetic and ethical motivations.

Much of the “fact” upon which Leibig based *Organic Chemistry*... was wrong or incomplete, but he attained almost instant fame with the book and its successors, which sold both to farming and to scientific audiences. Scientists readily adopted Leibig's work because, where humus offered no clear starting points or goals, Leibig's work entailed clear-cut research goals in carrying out soil and plant and animal tissue analysis that would have results meaningful in a designated context, nutrient cycling (Rossiter 1975: 34). For this reason scientists overlooked American Samuel Dana's *Muck Manual for*

Farmers (1842), which provided a better explanation of the action of lime and gypsum than Leibig's. "Unfortunately, in failing to pursue Dana's theory, the scientists missed entirely his valid views on the importance of reactions within the soil" (35). Leibig's vision was compelling for many scientists because it was internally consistent, experimentally fruitful, as a "finalized science" it met an acknowledged external need, and it was highly explanatory for some phenomena; if more of its basic assumptions had been correct, it would also have been predictive. Chemical techniques for the analysis of nitrogen and minerals were equal at that time to the task of making the determinations suggested by the "balance sheet" research agenda.

Because microbiology had not yet come on the scene, no theory, apparatus or routines had been developed for microbiology, so that humus researchers, by contrast, were attempting to analyze humus products without an adequate foundation (Rossiter 1975: 35). Leibig benefited from the confused state of research in the humus paradigm. This confusion derived largely from the difficulty of humus problems, but the Thaer school overlooked the significance of de Sature's findings on carbon—central to their research—for over 40 years. Like the perception of minerals in plant tissues as anomalous, this was an example of the invisibility of data, as Kuhn (1970) argued, if it was not specified by the theory of a certain research community. This fact was available to Leibig as a foundation-stone in his theory, and he used their disregard of it against them: "Leibig ridiculed the humus theory and revealed weaknesses in all the proofs for its support" (Krupenikov 1990:128). Selman Waksman, author of the still-authoritative *Humus*, likewise wrote that Leibig "ridiculed" the plant physiologists, stating "in his typically sarcastic manner: 'Vegetable physiologists consider the formation of woody fiber from humus as very simple; they say humus has only to enter into chemical combination with water in order to effect the formation of woody fiber, starch or sugar'" (1938: 14).

The disparity between the technical maturity of organic chemistry and the weakness of theoretical foundations and lack of external goals in the humus paradigm all constituted

significant elements of opportunity in the disciplinary rhetorical situation to be addressed. By contrast, Leibig produced a “coherent set of statements in text” (Frawley: 90). The fact that his theory was neat, coherent, and, as its name suggests, mathematical, meant that more straightforward questions could be posed, and the answers would be less equivocal. Especially in contrast with humus research, Leibig’s hypothesis was “deductive, comprehensive, and heuristic, and thus fulfilled the function of a paradigm” (Krohn and Schäfer 1976: 50). Because of growing interest in agricultural chemistry, Leibig’s book was excerpted and reviewed in the contemporary agricultural magazines, with editorial encouragement for farmers to undertake to test the recommendations, and subsequently farmers’ reports of their experiments based on his theories were also published.

The ideas caught fire, and there was a craze or a fad for soil analysis in the 1840s. In addition to the report for the Society (which, because it was commissioned, gave Leibig an unusually strong relationship to the audience), the book, and reviews and commentary in agricultural magazines, the “discursive space” of biochemistry was further promoted by Leibig in the *Annalen*. William Frawley, in *Text and Epistemology*, looks at biochemistry as an example of how discursive spaces are created for discourse in new disciplines. He relates how Leibig used editorial influence, “his position in control of the production and dissemination” of the *Annalen der Chemie und der Pharmacie*, to secure a hearing for his theories (1987: 91-92). Frawley notes that Leibig conferred on biochemical discourse the legitimacy of organic chemistry by piggy-backing it into the already-established discursive space of chemistry. Krohn and Schäfer (1976: 48) relate that

To this end Leibig in 1840 modified the traditional classification in the *Annalen* (in which investigation in agricultural chemistry would have appeared only under the item ‘mixed notes’) and had all relevant publications, including his own works and polemics with other scholars, appear in the *Annalen*. In addition, Leibig ensured that agricultural chemistry was given a prominent place in many of the handbooks and textbooks published.

Leibig's impact was immediate and strong. Kellogg (1957: 3) wrote that "the seemingly simple reasonableness of his views, his prestige as a scientist, and his skill in debate swept away all the alchemistic theories of plant growth." Leibig was so successful because of 1) current social conditions, 2) the disarrayed state of the opposing framework of research (humus) and the direction of organic chemistry, 3) the inventiveness, self-conscious internal consistency, and comprehensiveness of his theory, and 4) his commitment to launching it. Of greatest importance were the new way of looking at minerals and the establishment that carbon from air and soil minerals were essential for plant growth. Leibig was right to focus on the few certainties about plant physiology that had emerged from the normal science in Sussure's work and in the 1820s and 1830s, and to re-think what real processes these might reflect. His theory that elements cycle and reappear was the one aspect of his work to survive the ensuing scrutiny and criticism. A second area of success would be the long-term one of the development of synthetic fertilizers, available to agriculture as discrete inputs. A third critical positive aspect of his entrance into agricultural chemistry was that it reinstated a "problem focus" in the place of a "disciplinary focus." This shift recognizing and acknowledging the great urgency of a social goal motivated biochemists and set them an agenda of nearly a century of "normal science," for which Leibig is largely given credit. This was a period of intense scientific experimentation emphasizing exact measurement, with objectives of identifying isolated nutrients and tracking their metamorphoses through organic compounds, as well as developing new techniques and more effective apparatus.

2.2.1.2. Limitations in Leibig's Theory

Brilliant as it was in these respects, however, Leibig's offering to the soil science community of the time was overly theoretical; he insisted that critical problems were "susceptible of easy solution by well known facts" (Rossiter 1975: 23). Since it reflected little experience with agriculture, and that with soils in a limited area, it was oversimplified and reductionist, and bound to be misleading in application to other settings. Several aspects

of this oversimplification bear examination, since they strongly influenced the discourse atmosphere for other participants over several decades, affecting soil science and the soil conservation issue thereby. Leibig's theory seems to merit the charge of being "contaminated" *a priori* by the lingering presumption that there is only *one* factor most responsible for plant nutrition theory. On this basis, Leibig emphasized nitrogen in the 1840 edition and minerals in the 1843 edition of *Organic Chemistry...*, in each case implying that only the favored materials were essential. In contravention of the most common experience of soil management, manuring, and based on a single, later repudiated experiment, he associated nitrogen with carbon as being supplied from the atmosphere (Rossiter 1975: 35; Russell 1957). His dissociation of nitrogen from the soil was less a scientific move than a strategic one, to refute the old theory and distance the new from it as much as possible; when faced later with incontrovertible experiments, "Leibig conceded this point" but "only with considerable hesitation" because "to recognize that manures needed to contain nitrogen clouded the clear differences between the traditional humus theory and Leibig's own theory of minerals. This did not seem desirable from either a strategic or an institutional point of view" (Krohn and Schäfer 1976: 44)

Organic Chemistry... triggered a robust round of criticism from England. Although John Bennett Lawes had already been conducting research at his ancestral property, the nitrogen issue brought its formal establishment as the Rothamstead Agricultural Experiment Station (Russell 1957). Lawes and his collaborator, chemist John Gilbert, set up long-term trial plots testing and carefully recording the performance of the major crops grown in England under different rotations and fertilization schemes. Their plot records provided a basis for definitive answers on the value or destructiveness of practices tested—in this setting nature was appealed to for judgment, with answers in the form of bushels per acre. The response to Leibig was initially based on the question of the source and form of nitrogen, and after the 1843 edition, on the basis that nitrogen was less important than minerals. English chemists and the educated and participating large and small landowners

had taken lessons from experience with English soils, namely that, in most, nitrogen is the first limiting factor, followed by calcium (Hopkins 1910: 326ff.; Russell 1912; 1957). In 1856 Lawes and Gilbert, and Boussignault, independently, disproved Leibig's claims about both the source and the form of nitrogen.

A further area of oversimplification had to do with the process of chemical transfer. With de Candolle and Macaire-Princep, Leibig had attributed to plants the power of selecting and drawing nutrients up from the soil, a process he conceived of as a simple transfer. In England in 1850 and 1852, Thomas Way published the results of an experiment in which a solution of ammonium sulfate ($[\text{NH}_4]_2\text{SO}_4$) was passed through a column of soil and yielded a solution of calcium sulfate (CaSO_4), suggesting that the calcium had been replaced, or leached out, by the nitrogen and hydrogen. These were the first cation exchange experiments, and indicated the complexity of the chemical processes that occurred among soil constituents. But Leibig explicitly ostracized Way, and on a strategic rather than methodological basis; without trying to duplicate the experiments, Leibig commented "These experiments are very remarkable, and should be opposed with might and main" (Thomas 1977:230). Leibig's "contemptuous" criticisms of Way, in 1855 and 1858, instigated a debate, wasteful of time and energy, over whether this was a physical or chemical process (Kelley 1948: 6), when they were in fact, both, cation exchange being the means by which the chemical process occurred (a "bridge principle" [Hempel 1965]). The outcome of the dispute was that Way's pioneering work, rich with implications for many kinds of soil research, was virtually lost for the mainstream of soil research until the turn of the century, because of the overpowering influence of Leibig and the paradigm he established (Thomas 1977).

Rejecting the idea of biological life in nutrient transfer processes was another oversimplification. It is the biological connection that explains the effects of lime, long-observed to German speakers as well as English: *Kalk macht die Väter reich, aber die Söhne arm* (Lime makes the fathers rich but the sons poor [Hopkins 1910: 162]). There is

nothing about Leibig's theory that can explain this effect or the differences (and similarities) between lime and gypsum. Soil in his view was inert and passive, a "storage bin of nutrients" (Kellogg 1957: 3); this was a geological view of soils and a physical conception of the cycling of nutrients. He thus deduced that

as one ton of farmyard manure contains only about 30 lb. in all of nitrogen, phosphorous and the potassium, calcium and magnesium group, it is more effective, as well as being more economical, to supply these in the forms of simple salts which are readily available in large quantities and at low cost. (Russell 1957: 139)

In reality, however, manure or some equivalent is an essential addition to maintain the physical condition of continually cultivated soils and their buffering and exchange capacities, and to enhance the availability of nutrients. Furthermore, when introduced into soils, nutrients react chemically on the basis of texture, pH, and mineral composition of the particular soil, and the chemistry can affect not only availability but also the physical condition of the soil. Not knowing this, Leibig was led to argue that putrefaction (leading to humus) was not biological (Wiley 1898; Waksman 1938) and to assert, incorrectly, in his 1864 *Chemistry as Applied to Farming* that growth and yields would rise in proportion to the addition of nutrients (Kellogg 1957; Krupenikov 1990). In practice, a point of diminishing returns is reached, nutrients can become available or unavailable because of chemical bonding related to pre-existing soil factors, or they can reach toxic levels.

Lawes and Gilbert engaged nature as the "ultimate tribunal" of judgment of their argument for a soil source of nitrogen, and found that this chief problematic nutrient in Leibig's theory was supplied, not by the atmosphere, but from the soil, and was restored by manure and by grass/legume rotations. This presaged the recognition of microorganisms as the resolution to numerous perplexing questions of fermentation, which investigators frankly admitted were not understood. The discovery of microorganisms in the 1860s made possible the later discovery of their presence in association with the soil organic matter and aspects of that association: 1) microorganisms are provided a more favorable environment in the presence of plentiful

organic matter, 2) nitrogen is produced both by free-living and associative soil microorganisms, especially in undisturbed soil, and 3) the nitrogen-fixing legume rhizobia were responsible for increases in nitrogen content after pasture rotations of legume cover-crops. The approach taken at Rothamstead was simultaneously pragmatic and realistic, aimed at formulating a more adequate theory, and based on the well-known effects of legumes and of manure; for this purpose they set up long-term trial plots in the early 1840s.

By contrast, Leibig relied on analyses, having complained that the problem with agricultural research is that “it takes a year or even longer before one has all the results” (Krohn and Schäfer 1976:). Indeed, it can take decades and even hundreds of years. No indication of the complexity of either chemical or biological dimensions of soil processes, which are long-term effects, would have been forthcoming from Leibig’s methodology of testing tissue and soil samples, because it disconnects subjects from their natural settings or contexts, and is concerned with short-term, discrete inputs. Indeed, upon examination, many aspects of Leibig’s work feature a short-term view—his interpretation of the function of lime, disregard of changes in soil structure—which is also to say that he *was* interested in improvements that could be made in productivity in the very short term. Leibig had created such a furor, however, that a great many agricultural chemists and organic chemists, as he had intended, entered the now raging debate. A great deal more work in agricultural chemistry, especially soil analysis, resulted, and his errors were soon revealed as well as new advances made.²⁰

2.2.1.3. Leibig Criticized on Grounds of Violation of Scientific Ethic

From his very first work in agricultural chemistry, Leibig adopted an assertive, authoritarian, tone. The first *Organic Chemistry...* (1840) “asserted so much that it was hard to derive a clear-cut program for improved farming from it all.” The first edition offered in its three sections “(1) the destruction of the humus theory, (2) the elaboration

of the nitrogen cycle, and (3) the explanation of the role of minerals.” It was “more of a critique of the present state of the field...than a systematized treatise on agricultural chemistry” (Rossiter 1975: 30, 20). By 1843, Leibig had shifted from an emphasis on ammonia to minerals, but had done so without any foregrounding of this shift—no acknowledgment or discussion of a change of mind or credit or refutation of new or of previous sources of his ideas; his shift of emphasis was only recognized in the US when an “alert reader” of a farm magazine noted that he no longer favored charcoal as a nitrogen fertilizer (Rossiter 1975: 42ff.). Leibig needn’t have made such a shift, except that he aimed to distance his position from humus research as much as possible, despite the abundance of reliable research in that stream pointing to the importance of nitrogen. Greater care in selecting supporting theories and the attainment of a broad base of support, from many different kinds of proofs, are essential, Hempel (1965) emphasizes, for convincing scientific communities of explanations. Leibig’s errors and weaknesses would have been forgiven in the current atmosphere, where so much was uncertain, except for the fact that he adopted the ethos of authority rather than of a participant in a community of inquirers (Kelley 1948).

This disingenuous argumentative approach cost Leibig the goodwill he initially had from agricultural chemists; up to 1840, as a respected organic chemist, he had been held in high esteem on the basis of communality, but he clearly did not return the regard. Krohn and Schäfer (1976:46-47) state that Leibig explicitly opposed the humus theorists and physiologists not only on the grounds of his own mineral theory, “but also upon his understanding of the principles underlying effective opposition.” Leibig wrote:

Glancing at the history of science reveals that if a new doctrine takes the place of a prevalent one, the new doctrine will not be a further development but the very opposite of the old one. For a long time I believed that in agriculture it would suffice to teach the truth, in order to disseminate it, as is common in science, and to disregard errors; finally, however, I have realized that this was wrong and that

²⁰ Rossiter, Chapter 7.

the altars of the lie have to be shattered, if the truth is to be based on a firm foundation. (*Chemistry...* 1862, 7th ed.)

Leibig's strategic criticism in writing of other chemists was complemented by "strategic behaviour" (Krohn and Schäfer 1976: 35) to dislodge professors in the practical agriculture tradition and provide places for his own students, at which he was very successful.

2.3. LEIBIG'S LEGACY TO THE SCIENTIFIC COMMUNITY

There were also criticisms on the basis of practice. Leibig was known to sacrifice accuracy for speed in analysis, and to introduce or overlook some inaccuracies, including an unexplained discrepancy, in favor of his theory, of a magnitude of 100, between a current and a previous edition. This trend grew more apparent after his move to Munich (1852), when, no longer much involved in research himself, he seemed to have turned his inventional gifts to the defense of his work as orthodoxy or "organized dogma" rather than to problem-solving. Finally, his turn-around on ammonia and the failure of his patent fertilizer caused farmers in England and America to become cynical about "scientific farming," and to aim this cynicism at their own chemists and agricultural scientists (Rossiter 1975). These factors combined to produce an atmosphere of bitterness and acrimony in scientific discourse at the time. Waksman (1936: 20) wrote of Leibig that he was wrong after all, or at best, only partly right. But how could he foretell the important contributions to our knowledge of soil processes and plant nutrition that were to come from a new science which was being introduced by Pasteur and which became a subject for his caustic ridicule?

Written more than half of a century later, this statement (and others like it) by a prestigious soil microbiologist demonstrates how great was the "moral indignation" directed toward Leibig for his contravention of scientific ethos. But given these features and in light of the powerful insight that he contributed, does it matter whether Leibig insulted his co-workers in inquiry? Writing about argument in scientific communities, astronomy specifically, Allen Harris maintains that "fierce, unconstrained argumentation"

is productive (1990: 22); it elicits robust response and triggers new inventional forays. Ihde, while not overlooking Leibig's abuses, wrote that his book, *Animal Chemistry*, "served as a stimulus to biochemical controversy and investigation. Much speculation in the book ultimately proved without foundation. Nevertheless, it raised questions and these could be exposed to experimental investigation" (1969: 194).

But in castigating scientists in the humus/management paradigm with extraordinarily harsh language, Leibig apparently also made his impact felt to many as the proverbial intimidating "raised stick" of rhetorical theory (Johnstone 1980). His dyslogistic approach to opposing researchers, combined with his efforts to open university places for his students and those who adopted his paradigm, assured that the competing streams of study would lose participation and momentum. In fact, this was the case not only for humus studies, but also for the study of soil physics, in which Ewald Woolny's important contributions were overshadowed, and for Thomas Way's work in physical chemistry; progress in both was derailed by the focus on biochemistry (Baver 1940; Thomas 1977). Also, the concept of the "tyranny of language" in scientific research applies to Leibig's "law of the minimum"; Leibig referred to and promoted his hypotheses as laws, excluding virtually all of what had been promoted by the humus researchers, with the purpose of "breaking their altar"; in this he was unaware of how incomplete was his conceptualization of the (very complex) problem of fertility in soils. As "law" was implied, first, that the inorganic elements formed the complete set of factors about which one had to be concerned in order to maintain soil fertility, and second, that if all of the inorganic elements of a soil were being restored, then all of the soil management problems were being addressed. These invention-restricting presumptions affected interest and research in other areas for some time, that is, the law ruled out other areas—such as soil physics and electrochemical processes—as "places" to look for aspects of the soil fertility puzzle.

This effect was evident to soil scientists in retrospect. In 1977, Viets wrote that the soil fertility definition used by the Soil Science Society of America, inherited from Leibig's tradition, is "Soil fertility is the status of a soil with respect to the amount and availability to plants of elements necessary to plant growth." In Viets' view, this is far too restrictive, as, in reality, "A worker in the field of soil fertility must have a good understanding of the general principles of soil physics, chemistry, microbiology, and pedology...built upon the information supplied by these other fields...We are primarily concerned with the integration and synthesis of these other fields." Viets believes that "[multifactor] experimentation was probably held back by undue emphasis on Leibig's Law of the Minimum and its counterpart, Blackman's Law of Limiting Factors applied to photosynthesis" (1977: 243, 248). Warkentin (1992) notes that what we know is determined by the questions that we ask, and that Leibig, surely in some degree because of the context in which he worked, asked the questions that led soil science into an era of short-term studies of discrete inputs.

Under increasingly successful empirically guided practice, and in the company of participating and concerned landowners, English agricultural chemists tried to account for the successes with a focus on the most plausible element—humus. They developed successful practices, including the addition of mineral fertilizers as well as rotations, to maintain physical condition, nutrient status, and pH, and to raise the humus levels and the fertility of the soils. Leibig worked in an entirely different setting, with severe restrictions on land and expenditure, and his resolve to develop a system of fertility maintenance may have been strengthened and shaped by factors peculiar to his own problem focus. He redirected chemical study of the mid-1800s in a way that addressed the dire and immediate needs of his setting; he responded with studies that showed the way to discrete inputs, with the expectation of first-year increases, and a solution that does not rely on other capital outlays or large acreage for the benefits of rotations and diversification. Leibig's approach may also reflect the cultural presumption of a given set of practices—manuring, possibly soiling, intensive agriculture—because "balance sheet" soil

management without the addition of manure is liable to bankrupt the soil capital. Presumptions about the data of soil science and consequently invention about soil characteristics and processes seem, from these examples, to be highly colored by the social, cultural, and physical setting of the investigators.

The examination of these early streams of soil science contributes to the view that the aspect of science that Prelli calls its "interpretive face" is subject to the influence of social (including geographical and cultural) and disciplinary context. On the basis of the trends and events reviewed in this chapter, it can be stated that experience and the cultural presumptions that accompany the study of soils—very situated, timely, and particular—contribute immensely to "the truth" or to the facts that a researcher sees as "true" about the medium studied. Each stream represented a solution for the problem setting to which it was addressed, and the features of each setting suggested particular lines of inquiry. Soil science at this point was almost exclusively a north European enterprise, spanning from Russia to England at its geographic extremes. Presumptions of these soil scientists' settings, things that "went without saying," must then have been embedded in soil science as currently defined. Proceeding to look at early soil science and its relationship to soil conservation in the US, we will see what these presumptions were, and how they affected the study of soils there.

CHAPTER 3

AMERICAN SOIL STUDIES: CONTEXT AND CONTENT

INTRODUCTION

In this study, our central question is what part rhetoric played in bringing about the eventual adoption of soil conservation as a national natural resource policy. In adoption of policy, lawmakers make use of deliberative rhetoric, addressing matters of fact, ethics, and humanitarian solicitude. Matters of fact, *logos*, in the adoption of soil conservation, are presumably made available or presented by the soil science profession. Therefore in the previous chapter, partially establishing the background of that *logos* in soil science and in the eventual adoption of soil conservation policy, we were concerned with the development of knowledge in soil science in Europe and Britain, and with cultural factors affecting the making of knowledge in soil science in the contexts considered. In the present chapter we turn to the North American continent, to look at the development of soil science in the United States, the more immediate ancestry of soil conservation.

Since we are looking at the development of a rhetorical situation, a situation in which rhetoric for a certain action was successful, factors that might be regarded as affecting the rhetorical situation are of particular interest. Leading to the adoption of a soil conserving policy, important questions would be the following: Was there an exigency relating to soils in America when soil science was developing here? How great was it, of what kind, and what were its constituents? How did soil scientists perceive the challenge to conserve soil, and what was their approach to soil management? We should also ask how prepared were American soil scientists, by their experience, education, and exposure to the European soil science traditions, to "see" the exigency, and how relevant to the situation "on the ground" was their characterization of it? We will attempt to address these questions in this chapter in three sections. The first section is an overview of soil management in America, including the social/demographic and economic contexts of early American farming, looking mainly at the 1800s; a brief discussion of the growing

interest in conservation concludes the first section. The second and main section of the chapter consists of an examination of the orientation and activities of the American soil science community in the late 1800s and early 1900s, and aspects of the substance of their knowledgemaking as a community, related, whether directly or indirectly, to the question of soil conservation. The third and final section is a discussion of the responsiveness of the federal soil science organization to the need for leadership in soil management and soil conservation and an analysis of the focus of soil science in the context already examined.

3.1. SOIL MANAGEMENT PRACTICES IN NORTH AMERICA

The nature of the discourse in soil science in the US of the late 1800s to early 1900s would logically be based on the context of soil science studies. This discourse can therefore be expected to reflect concern with the characteristics of soils on the continent, with the maintenance of their fertility, and therefore with how they had been and were managed. In part, North American soil science would reflect the inferences and assumptions from Europe, as inherent in the topics and authority of the older tradition, and in part, just like European soil science streams in their own contexts, it would be determined by local conditions. In fact, conditions relating to soil management were radically different from those prevailing in Europe and England, so much so that early observers expressed shock at the differences and the conditions they saw when visiting the new continent.

3.1.1. Exploitation and Abandonment of Lands

Peter Kalm was a Swedish naturalist and one of many “sharply critical” visiting observers of the early American farming scene (McHenry and van Doren 1972). Kalm wrote in 1753 that

Agriculture was in a very bad state hereabouts.... The rye grows very poorly in most of the fields, which is chiefly owing to the carelessness in agriculture, and to the poorness of the fields, which are seldom or never manured. After the

inhabitants have converted a tract of land into a tillable field, which has been a forest for many centuries, and which consequently has a very fine soil, the colonists use it as such as long as it will bear any crops; and when it ceases to bear any, they turn it into pastures for the cattle, and take new grain fields in another place, where a rich black soil can be found and where it has never been made use of. This kind of agriculture will do for a time, but it will afterward have bad consequences, as everyone may clearly see. (in McHenry and van Doren 1972:171)

Kalm noted that the English "in general" have a more perfected system of agriculture than any other [European] nation, but because English settlers found such rich, deep soil in North America, they became careless. In particular, he noted the practice of allowing cattle to roam loose, so that the manure was scattered and could not be used on tilled fields. Not being manured, the field "finally loses its fertility, of course." Farmers would farm one field until it had been thus "deprived of its fertility," they would then leave it abandoned, move on to another part of their land, and treat it in the same manner (McHenry and van Doren 1972: 171).

George Washington, James Madison, and Thomas Jefferson all took part personally in the management of their lands, Jefferson at one point expressing despair in his inability to maintain the fertility of the soil, and later describing with some hopefulness a "hillside plow" designed by his brother in law for plowing on the contours of the sloping Virginia lands they both farmed . George Washington wrote "scornfully,"

The system of agriculture (if the epithet of system can be applied to it) which is in use in this part of the United States, is as unproductive to the practitioners as it is ruinous to the landholders.... The general custom has been, first to raise a crop of Indian corn (maize) which, according to the mode of cultivation, is a good preparation for wheat; then a crop of wheat; after which the ground is respited (except from weeds, and every trash that can contribute to its foulness) for about

eighteen months; and so on, alternately, without any dressing, till the land is exhausted; when it is turned out, without being sown with grass-seeds, or any seeds, or any method taken to resort it; another piece is ruined in the same manner. (Bagley 1942: 12-13)

The farmer then can “either recover the land which he has ruined, to accomplish which, he has perhaps neither the skill, the industry, nor the means—or to retire beyond the mountains—or to substitute quantity for quality, in order to raise something” (Bagley 1942: 13).

Why did this pattern of “exploit and abandon” prevail in the United States? It did so in large part, according to many commentators of the era and more recently, because of the vast stretches of land available for settlement. This circumstance led to a paradoxical pair of beliefs in the era of westward expansion: that “the land is inexhaustible,” and, at the same time, “the land will wear out, and the farmer will have to move West.”

A.F.Woods, in “The Wastes of the Farm” expressed this view in 1908:

One of the characteristics of farming in this country as compared with farming in Europe is the apparent wastefulness of American methods. The truth of this is conceded, and it is clear that the reason for it has been largely due to the vast areas of fertile land to be had almost for the asking and with the rapidity with which the land has been taken and utilized by men dependent largely on the resources of nature and their own brains and muscles. During this great preliminary expansion of agriculture, which has outstripped the growth of other organs of our social body, agricultural products have been produced at a bare living wage. Every economy has been required by the conditions prevailing, and it has been necessary to borrow largely from nature’s resources in order to live and lay the foundations for a better agriculture. (1908: 195)

Sir E. John Russell, in his *Fertility Of the Soil* (1913: 48), connects the American abuse of land both to the wealth of land and to modern transportation. Speaking of the

conditions of soil exhaustion in colonial and early republican North America, he says that "the exhaustion of the virgin lands constitutes the simplest case [illustrating extraction of fertility by crops] because everything is removed from the soil and nothing is put back. It could only arise under conditions of cheap transport facilities between the virgin land and the cities where the grain is to be consumed. Thus it is essentially a modern phenomenon; it never arose in so acute a form in England because such facilities did not exist until a complex system of agriculture was already established." The completion of the Erie Canal in 1825 and of the Chesapeake and Ohio Canal in 1836 and of 30,000 miles of railroad, more than that of all other countries combined, by 1870, are generally associated with two mutually reinforcing trends: one, the westward migration of farm families in search of new land and the other, the eastward flow of cheaper grain against which eastern farms could not compete (Bidwell and Falconer 1941: 181; Whatley 1994: 2).

In 1962, when he was director of the Rothamstead experiment station, G.V. Jacks wrote in "Man: the Fertility-Maker" that new societies are necessarily in a subsistence condition and must use more energy than they produce; such a society takes more fertility out of the soil than it puts back. That is why all the new societies formed of European stock in the last few centuries in North and South America "brought about soil exhaustion and its sequel, soil erosion. They could not help it..." (147). Early Americans' predacious approach to land and forest use may be understood not as unique to them, he suggests, but rather as typical of all pioneering peoples. Contrasting the land-use impacts of a settled people with pioneers, Jacks also observed that for some time, the terms of leases in England had encouraged improvement of soil fertility: "Many people think that the English system of mixed farming in the nineteenth century, coupled with the prevailing system of land tenure, represented an almost perfect example of conservation farming. In particular the tenant farmer was encouraged by the terms of his lease to improve soil fertility. When he left the farm, he was adequately compensated for any improvement he had effected" (148). This meant that there was incentive to make improvements of long-term benefit, the value of which would not have expired or become exhausted before the

end of the tenant's lease or period of tenure; liming fell under such a scheme, as would fencing, building structures such as terraces or dams, planting of perennials or making long-term improvements to buildings, and it was common for tenants under English lease laws to make such improvements (also Hervey 1970). Like Jacks, Russell noted that terms of leases commonly remunerated the tenant for any unexhausted or unexpired improvements. English land rental arrangements generally favored long-term benefit to the land as to rural capital, for example, by restricting the sale of potassium-rich products from off of the farm property; hay was to be fed on the property but could not be sold to be fed elsewhere (1957). To these arguments could be added another: that American farmers farmed as they did because of the scarcity of labor.²¹ Under single-family management, John Mitchell of North Carolina pointed out in 1882, farms frequently lacked labor, having too little to make it worthwhile to expend labor on improving land, when more could be had for the lesser trouble of clearing. Turner (1990) and Trimble (1984) note correlations between ethnicity and landcare habits. Turner wrote that Scots and Scots/Irish (immigrating before the 19th century improvements in the land ethic of those areas) were "notorious in the Old World for the rapaciousness of their land ethics. Raised in cultures with almost no traditions of wise usufruct, these Scots and Scots/Irish immigrants and their descendants helplessly recapitulated in the Southern Appalachians what they knew of land abuse" (1990: 36). Trimble reports the same of Scots/Irish settlers, but contrasted it with the reputations of immigrants from overpopulated central Europe, like the Germans of Lancaster County, Pennsylvania, who were "land hungry" as a rule. This category included the Amish, Mennonite and Moravian settlers, who, by settling as communities, resolved by voluntary association some of the problems of labor shortage. Some of this labor went to basic soil management practices—such as building fences to enclose animals and gather their manure, collecting and managing night soil, planting pasture crops, rotating crops, and plowing on the contour—that enabled these communities to sustain themselves in some of the exceptional early permanent agricultural communities other than the self-sufficient slave-holding plantations.

²¹ See also Sachs 1983; Kay 1984.

3.1.2. Beginning of Interest in Conservation

In the middle of the 19th century, the public was starting to become alarmed about the loss of forests and landscapes that they had collectively destroyed on the march across the continent. The closing of the frontier, the end of available land, impressed the message that the land did not roll on endlessly into the West, and the resources were not infinite. George Perkins Marsh's *Man and Nature* was an effort to chronicle the destructive impact of man upon the landscape, not only in present times but in ancient times. Marsh's idea resonated with others in other media; this resonance appeared in the visual arts in the work of the Panoramists and the painters of the Hudson school, and in the literary arts, especially romantic poetry. The gulf of distance between the past grandeur of Middle Eastern and Mediterranean civilizations and their present conditions was being recognized and questions asked for the reasons for the difference. In the 1870s, the American Association for the Advancement of Science began to pressure the government to establish forest reserves, with two central goals: to preserve forest resources and prevent a "timber famine" as well as to help stabilize surface water movement and decrease the loss of available water and damage from swiftly-moving surface water.²² Following these efforts, Yellowstone National Park was established in 1872, the Federal Forest Reserve system was established in 1891, and a National Soil Survey was begun in 1899. Conservation interests and action were centered in the more prosperous northern states.

In the southeastern states in general, meanwhile, conditions were quite bad. There was little improvement in agriculture, and few individuals or groups were interested in conservation. Although for reasons connected mainly with the Civil War, there were few highly qualified soil scientists working in the Southern states, one of the most highly regarded early soil scientists, Eugene Hilgard, took his first job in Mississippi before the

²² On background of forest policy, see Runte 1991, Heisley 1933; *USDA*... 1940, some early popular articles include Dana 1916, Smith 1914; scientific articles on the influence of forest cover on water movement: Sampson and Weyl 1918, Bennett 1921, Zon 1927, Lowdermilk 1930; on forest cover and grazing on erosion and flooding: Roth 1901, Paul and Baker 1925, Forsling 1931, Bailey and Forsling 1934.

war and stayed until 1874, working as geologist for the University of Mississippi (Jenny 1961). Hilgard conducted the first surveys of the region. The destruction of landscape he saw dismayed him. Writing later about the Mississippi Table Lands, northwestern and central Mississippi, where some of the worst gullying was occurring, Hilgard pointed out that this area, blessed with a three- to seven foot topsoil of yellow to brownish loam (loess), had at one time been regarded as the best cotton-growing portion of the state (1906: 219). But when this land was "taken into cultivation, little or no attention was paid to the direction of the rows of corn and cotton," with the result that fields were frequently plowed up and down hill. Such small infelicities led to gullies, readily formed in the "dead furrows," with the sometimes-torrential summer rains. These slopes would shed their soil, funneled in the downhill-pointing furrows, "in a muddy flood upon the lower lands." The following season the small gullies would be plowed over, and the rains would re-open them, and the process would recommence. Because of the loss of fertile topsoil through this treatment, these fields were eventually "turned out," that is, abandoned. Because they were abandoned without any replanting and therefore had no protection from rainfall, and because the surface was crusted and did not admit water, the rain water as runoff quickly cut further gullies. These soils have a loosely cemented sand subsoil which, when it is cut into, collapses in huge caving sections. These collapsing sections sent volumes of sand into the fields and streams below them, and left open new areas to be cut into by the next rains. "In this manner," Hilgard wrote (1906: 219), whole sections, and in some portions of the State whole townships of the best class of uplands have been transformed into sandy wastes, hardly reclaimable by any ordinary means, and wholly changing the industrial conditions of entire counties, whose county seats even in some instances had to be changed, the old town and site having, by the same destructive agencies, literally 'gone down hill'." This destruction of lands was greatly aggravated by the Civil War, during which, and for some time after, large areas of land once under cultivation were left to the mercy of the elements. Little of these conditions was known outside the areas in which they occurred, and few people were there to see them—an illustrative case of Toulmin's rule that "local, oral, timely and particular" are liable to be

disregarded. To that list, perhaps rural could be added, as describing “facts” that are likely to be undervalued because unavailable. Bagley reported that his research on land degradation in the South was impeded because such facts were largely unavailable or they were anecdotal and qualitative rather than recorded and quantitative (1946: 11).

3.2. SOIL SCIENCE STUDIES IN THE UNITED STATES: INSTITUTIONS AND OBJECTIVES

3.2.1. The Bureau of Soils: Setting and Activities

In what we are designating as the second stage in the progress towards soil conservation, a period roughly covering 1900 to 1927, soil studies became a true science under the leadership of Curtis Marbut, who served as director of the USDA Soil Survey Division from about 1910 to 1935. Before that shift had taken place, however, the influences of geology and chemistry had exerted distorting and somewhat contradictory influences on the soil scientists’ interpretations, both being very incomplete pictures of soil processes—soil science up until about 1927 has been likened to the blind men feeling different parts of the elephant. With the shift toward a genetic view of soils, a largely geological view of soil was abandoned for a view of soil as a “natural” or “living” body, and soil scientists undertook consciously to negotiate a definition of soil, doing this for the first time on the basis of the characteristics of the soil body itself.

The new science of studying soils as natural bodies with their own genetic factors is today’s “pedology.” It is usually associated with soil classification, and it may be called simply “soil science,” designating that it is the single stream of soil study that takes account of all of the soil’s properties. The development of the study of soils as natural bodies was important, because only when scientists arrived at a soil-genetic view of their subject could they see soil as important of itself and not just for the nutrients it supplies. The soil physical body is important in itself because it is the product of a unique combination of factors: climate, parent material, biota, topography, and time (Jenny 1941). Before the soil body gained recognition as unique and important, these features *in*

combination had no standing with the biochemists of the mid 1800s or the geologists and agrogeologists of the late 1800s. Due appreciation of the soil body itself represents, to an observer of the 1990s, an important step in the development of the science, one that contributed to the movement towards the adoption of a soil conserving outlook, and a necessary step for the soil conservation movement of the 1930s. To better understand how this is so, we will explore the impact of geology on soil studies and the steps by which the shift of perspective occurred.

The shift of perspectives was not smooth, and the main feature of the move was an extended stand-off that pitted federal soil scientists against a majority of non-federal scientists and in which even researchers abroad took sides.²³ In this debate, basic methodological and evaluative criteria in the conduct of soil study were brought into question within the disciplinary community. The existence of such a situation illustrates the applicability of the argument that non-disciplinary factors—e.g., timing, geography, ideology, politics, and economics—can affect the emergence of new ideas and their acceptance. In addition, the stand-off illustrates the point of rhetorical theorists like Burke who argue that rhetoric includes non-verbal features such as the threat of the use of power as an act of persuasion. Egregious use of power, ideology, other political factors, and even physical distance coincided as situational factors in constituting “what could be said” as well as “what was persuasive” about soils in this period, and especially from 1900 to 1910. Giving rise to this conflict was the affiliation of soil surveying and soil classification with the science of geology and the nature of background assumptions geology generated about soils.

We have already discussed how soil study was beholden to chemistry in the 1800s. By the turn of the century, as soil specialties were beginning to diverge more, one branch of soil science, soil classification/soil surveying or mapping, was similarly connected and subordinated to geology. Geology indeed had had a greater influence on the theory of

soils (especially because it was applied to classification, involving, therefore, definition), while chemistry was the chief means of investigating soil properties and constituents. As we saw, in the 1800s, the science of chemistry reached a point of great enough maturity to support agricultural chemistry as a derivative science. In the case of this period in the US, it was geology that provided the background for soil classification and mapping. Many situational factors contributed strongly to geology's becoming the foundation science for soil mapping in the US until about the mid-1920s. Because of the emphasis put on soil mapping in the federal soil science program, and also because mapping/classification implied and required definition, critical theoretical conflicts were exposed, conflict ensued, and eventually a knowledge-base was achieved that Marbut and others regard as the point that marks the true beginning of soil science.

3.2.2. Contribution/Influence of Geology in Soil Science

The first of these situational factors affecting the direction of US soil science in the 1900s was that geology was the basis of soil mapping in the nineteenth century. James Hutton in Scotland in the 1750s and A.G. Werner in Germany had initiated the first studies of what would be the science of geology (Ronan 1983; Toulmin and Goodfield 1965). Geology suggested a vastly older earth; for theologically constrained Europe of the time this meant that geology would not take hold in England or in France until the mid-1800s. Geology arose, Toulmin points out, not in theologically rigid England or France, but in "heretical" Scotland and Germany; Hutton's collected lectures, according to Toulmin (1990: 124, 218) were denied copyright in England on theological grounds. But Eastern Europe was not the ideological battleground that Western Europe was at the turn of the century, for from Germany eastward geology was adopted and enlarged upon, principles of geology and then agrogeology being applied to soil studies and especially to mapping.

By 1800, in Russia, minerologists were making associations between rock and soil in their physical, biological and chemical properties. V.M. Severgin is thought to have been

²³ E.g., Hilgard 1904; King 1908, 1911; Russell 1905; Hopkins 1904, 1910; Wiley 1904.

the first researcher ever to amass a collection of soils for differentiation, and also the first to determine the carbonate content of soils by boiling them with acids, a technique taken up by geology (Krupenikov 1992: 132), but developed by Humphrey Davy for soil studies. In the 1830s, while Lyell in England was still having to contend that geology was a valid science, many geologists, mainly Germans, were actively employed in land assessment and land mapping in eastern Europe and Russia.

As work in mapping went on, these geologists were conducting some basic soils tests and developing shared disciplinary practices. They developed “technical measures for preparing maps and had spatially shown in them the significant, particularly physical properties of soils and underlying rocks.” Furthermore, they developed geologically-based terminologies for describing soils. Some descriptors reflected the relative amounts of the major mineral-containing constituents, sand, silt, and clay. Other descriptors were necessary to designate soils formed “from crystalline rocks (granite, porphyrite, basalt...) and from sedimentary rocks (sandstones, clay, limestones).” A further terminology (in 1883) described “types of land cover,” namely “eluvials with residual soils, denudation equilibrium, residual denudation, of alluvial deposits, and eolian deposits” (Krupenikov 1992: 142; 137); these terms identified geological states and processes. Some strong disciplinary exclusivism seems to have begun around this time: “Those scientists considered agrogeologists, logically complained that agronomy, with no right to do so, had usurped soil as an object of research, narrowing down its interpretation to merely an understanding of the cultivable layer, which was also investigated one-sidedly and pragmatically” (Krupenikov 134). A work on German geologists in 1877 reported that soil science was “an essential...part of geology” not agronomy (Krupenikov 134).

“Spectacular fossil discoveries” made between 1790 and 1830 led to the very strong growth of the science of geology and of geology’s public profile in Western Europe (Torrens 1992: 40), gaining enough prominence to launch the creationist/ evolutionist battle. Around the 1850s, geology had an established scientific (Ronan 1983: 389-92),

popular, and institutional status—with geological surveys underway widely in Eastern Europe, to a growing extent in Western Europe, and beginning in the US—and agrogeology shared its spotlight. It is often pointed out in rhetorical studies that language choices compel the audience to a certain understanding of the subject under consideration, by characterizing the subject in some way. This characterization can be intrinsic in a name or term for a substance. From the time of the first attempts at soil study in the US, for example Edmund Ruffin's work in the 1830s and 1840s (Cutter 1895), until the official adoption of the genetic view of soils in the late 1920s, for purposes of classification soils were described largely in geological terms. Geological terms designated soils as "types of land cover," alluvial, loessal or residual, and distinguished among them according to how they came to be in a given place, or according to their parent materials, as from sedimentary or other kind of rock; this terminology indicated no essential difference between soils and finely ground rock. These terms carried a great measure of geology's influence, suggesting that the knowledge of what rocks the surface layer was derived from and where this material came to rest constituted, exclusively, the essential knowledge about a soil. Leibig's approach, while concerned with the chemical nutrients of soils, rested on a set of geological assumptions about soils, as it was exclusive of any consideration of possible internal dynamics within a soil, or its development *in place*. An alternative naming was by crops, "potato soils," or "corn soils," etc., for which, again, the characteristics of the soils themselves were irrelevant. Turning to the early soil maps in the US, we can see that a geological perspective of soil is clearly conveyed in their language.

The first known soil maps in the US were done by the Geological Survey of the County of Albany, NY, under the direction of the county Agricultural Society. They were published by the NY State Board of Agriculture in 1820 as a survey for a railroad bed, and they essay a classification of soils in "the first attempt yet made in this country to collect and arrange geological facts with a direct view to the importance of agriculture." The soils in the Albany report are discussed as "alluvial formations." More classifying maps

followed in 1822 and 1824 in New York, and an 1826 map of the geology of the Philadelphia environs includes, "discussion, in some detail, of the physical and chemical properties of soils" (Weber 1928: 85-86).

From 1840 to 1890, geological maps or geological surveys were conducted in Massachusetts, New York, North Carolina, Kentucky, Alabama and Wisconsin. All of these maps had a fundamentally geological orientation, but each recognized some other feature or features of the soils themselves— "The character of soils," "the characteristics and properties of soils," "the chemical composition of soils," "the physical characteristics," and finally in Owen and Peter, "the general physical and chemical properties of soils and the processes by which the soils have been formed, from the breaking down of the rocks and the influence of the different geological formations upon the character of the soil and upon the agricultural value of the land" (Weber 86-7). David Dale Owen and Robert Peter's mapping in Kentucky (1854), although connected with the geological survey of the state, was unique in making a systematic study of soils using chemical analysis; theirs was the first to give prominence to chemical analysis. Even though features of the soil itself were beginning to be described in soil maps the geological emphasis continued to be strong. Thus the Kentucky map, with its chemical analysis of the soils, nevertheless retains geological terms to describe soils.

Geology was applied to soils mapping in the US also because geology had already become better established as a science than any other branch of soil study. In Eastern Europe it was already established as the basis of soil mapping to determine the agricultural value of land for taxation. Other branches meanwhile were plagued with "wicked" problems; in humus chemistry, for example, microorganisms played roles that were not yet imagined. Agricultural chemists on the other hand had a full agenda of "normal science" objectives that Leibig had mapped out for them, and these concerns did not include soil classification. Geology was already well established in the US in university departments and as a governmental entity. In the 1870s, with some government

sponsorship, but largely by means of his own resources, John Wesley Powell led a survey party into the American West and their work led to the articulation of a system of laws governing landform development. All this was before any separate division or department in the government had been designated to study soil. Because of these two factors, geology already had maps, a tradition of mapping, techniques for making plats, techniques for some chemical analysis (e.g., for calcium carbonate, indicating marine effects) terminology describing landforms, personnel qualified to carry out this work, and the prestige of a revolutionary science. The agrogeological tradition of mapping with its techniques and terminology were consequently to lay the foundation of US soil mapping, the next main phase in soil science there, and to affect its direction dramatically.

3.2.3. Rapid Government Expansion

A further, and the most critical, situational factor leading to the crisis in theory was the institutional environment of expansion and optimism of the turn of the century; in this environment, mapping emerged as the most emphasized aspect of soil studies. Post-civil war initiatives to establish the industrial arts, including and especially agriculture, on a sounder footing were coming to fruition in the last decade of the century. The Morrill Act of 1862 established an independent Department of Agriculture and made provision for land-grant colleges specifically to provide education in the sciences and practical arts (and less in Latin, Greek and history). Former Confederate states having to be readmitted to the Union did not receive these benefits until approximately a decade later (Whatley 1994). Students from these institutions were coming into the workforce in significant numbers in the 1890s and 1900s. As a result of the establishment of the land-grant institutions, several states (19 states) established experiment stations for carrying out agricultural research to lead directly to application in their particular circumstances (True 1929; Knoblauch, Law, and Meyer 1962). Following on the success and obvious usefulness of these stations, Congress passed the Hatch Act of 1887, which provided \$15,000 annually for the further support and establishment of experiment stations to carry

out research in every state and territory. In 1914 Congress authorized a measure of federal support for agricultural research extension services in each state.

The US Department of Agriculture began printing an annual *Yearbook* in 1894, and the publication in early years not only chronicles but calls attention to the department's self-awareness of rapid and diverse expansion, the ready availability of funds (for programs, though salaries were lower than in state organizations), and the many and expanding areas of service. Bodies within the Department addressing these service needs included the Weather Bureau, responsible for forecasting and reporting data on droughts, storms and floods, and for relating weather conditions to agricultural production and the Bureau of Animal Husbandry with its responsibilities. The Bureau of Chemistry and the Bureau of Plant Industry had a wide range of responsibilities, but each was also carrying out some activities related to soils. The Bureau of Chemistry was responsible for research in elemental chemistry and its role in soil fertility, nutrition, and disease organisms, for monitoring the labeling of foods and conducting analysis of fertilizers, and for identifying infectious disease and declaring and managing quarantines; among its other activities, the Bureau of Plant Industry was undertaking research into nodular nitrogen fixing bacteria at the turn of the century.

This rapid concurrent expansion of state and federally sponsored scientific competency was new and intended to offer the public some official sources of qualified advice. On issues of soil management, farmers could look with confidence, in the last decades of the 1800s, to state colleges and land-grant institutions and their experiment stations for practical advice, and to the US Geological Survey (USGS), to an extent, and to the USDA Bureau of Chemistry for research results and some services. In each case, a few prominent scientists led the way with research that was as sound as the knowledge base of the time allowed. Bureau of Chemistry director H.W.Wiley was respected for his work on the chemistry of soil fertility, particularly his work on phosphorous, and for having the foresight to lead work in the bureau into the study of soil microbial life, especially those

forms that fix nitrogen in association with legume roots (this work later shifted to the Bureau of Plant Industry and was a principal interest of J.G. Lipman of Rutgers, who founded the Journal *Soil Science* in 1916). W.J. McGee, an independent geologist and hyrdologist, and Nathan Shaler, a Harvard geologist, both published and worked with the USGS, and both had an abiding interest in soils. Shaler's treatise on the origin and nature of soils, and their vulnerability to mismanagement, was accorded prominence in the 1891 United States Geological Survey. Shaler's work again suggests the close disciplinary connection between soil studies and geology.

Heads of experiment stations in this period usually were simultaneously chairs of their land-grant college agriculture departments. They were mostly educated in Europe, chemists and geologists, whose training would have been of high standard; much of the work of this broad cohort group, even from such a relatively early date, has withstood the test of time. They were agricultural scientists, generalists by necessity, in a field which became far more specialized during their tenures. "Much research contributing to progress in soil fertility and plant nutrition was done before the establishment of a formalized soil science, but it was done by men who called themselves agricultural chemists, botanists, geologists, etc." (Viets 1977: 243). Prominent among these were Samuel Johnson, a student of Leibig, who established the first state experiment station at Connecticut in 1875, the same year that E.W. Hilgard established the station in Berkeley California (Rossiter 1975). Hilgard's Mississippi Soils and Cotton Census reports of 1860 and 1880 are now regarded as early classics in mapping and classification on the basis of genetic theory, although they were received with indifference by all but a few of his contemporaries as the ideas were too advanced to be understood at the time.²⁴ Hilgard in

²⁴ Jenny also identifies unreservedly as an effect of cultural "vision" the fact that Eastern scientists did not recognize the value of his work on climate, because it involved arid-climate soils, to which their professional imaginations had not reached: "The soil scientists on both sides of the Atlantic, though thousands of miles apart, and speaking different languages, and possessing different cultural patterns, had nevertheless one common background: they lived in the humid region. Throughout their lives they were so sued to clouds and fogs and high rainfall that it appeared as norm; never would they dream that this environment would affect their scientific outlook. But it did. To these people there was one dominant soil

these studies in particular established primacy of discovery in the West of the importance of climate in soil genesis, and he gave cogency to his theory with monumental soils data.

Other prominent agriculturalists with a strong interest in and knowledge of soils included C.G. Hopkins, director of the Illinois station and author of the single most widely known text on soil fertility, *Soil Fertility and Permanent Agriculture* (1910). Hopkins was better known in his own time than was Hilgard because of his teaching as head of the Department of Agronomy at Illinois, his work with the experiment station, and because of his textbook. Hopkins' text was a major contribution because it set the available knowledge about soil fertility in a comprehensive volume in language and sequence that was accessible to any who would study it. Hopkins was explicit in aiming his language at the lay reader, and he carried out his intent effectively, as for example, when he described chemical bonds as "handshakes." Hopkins' reputation with soil scientists of today seems to be surpassed only by Hilgard's, and that because of the significance and scope of Hilgard's contribution to soil genetic theory. F.H. King of Wisconsin, who worked for a brief period for the Bureau of Soils, conducted research in soil fertility and management, and is recognized as the greatest soil physicist of his time. Also working at the time was Jacob G. Lipman, director of the New Jersey experiment station, who established the journal *Soil Science* in 1916; through personal interest and contributions to soil microbiology and by seeing that microbiology was given discursive space in the journal, Lipman helped to raise the state of knowledge about it (Waksman 1966). Other leaders in research including soil studies were C.E. Thorne, Director of the Ohio Experiment Station, E.B. Voorhees, chemist, at the New Jersey Experiment Station, William Frear of Pennsylvania State College and editor of *Agricultural Science*, and Charles Hunt of the Pennsylvania Experiment Station, where field trials for fertility studies had begun in 1882, and in 1910 were the oldest extensive field trials in progress in the US.

forming process: leaching of weathering products into rivers, lakes, and oceans. Their foundation of pedology was geology, the science of rocks..." (Jenny 1961: 71).

In the last decade of the century, the experiment stations had established reputations for research and were active in publishing their results in the growing number of scientific journals,²⁵ and the various state stations published Farmers' Bulletins on a range of topics for rural audiences. These were more widely distributed and read than the Federal bulletins (Meyer and Moldenhauer 1985: 92). Soil scientists had a growing number of venues to present their research and discuss ideas; these included the American Society of Agricultural Engineers, the American Society of Agronomy, and the Society for the Promotion of Agricultural Science and the Proceedings associated with them. Credibility of advice on soil management lay with the personnel of the agricultural colleges and the experiment stations. In the last quarter of the nineteenth century, no federal body was responsible for soil study, and the locus of growth in agricultural studies had been the state land-grant colleges only.²⁶ It was in this context that the most controversial figure of the period, Milton Whitney, began the work that led to his three decades as head of the Bureau of Soils in the Department of Agriculture.²⁷

3.3. SURVEYS IN THE BUREAU OF SOILS

The Weather Bureau had been transferred from the War Department to the Department of Agriculture in 1889 and thereafter began to study rainfall and temperature in relation to soil conditions, in particular, absorption of rainfall and availability of soil water.²⁸ In

²⁵ *Agricultural Science*, 1886; *Journal of Agricultural Science* 1907 (published in Cambridge, England), *Nature* 1834, *Science* 1885, *Soil Science*, 1916, *Journal of the American Society of Agronomy*, from 1908, *The Cultivator* (1834; later the *Country Gentleman*), *Appleton's Popular Scientific Monthly* 1848, *Journal of Agricultural Science* from 1905, *Journal of Agricultural Research* from 1913.

²⁶ Salaries were also much better at state institutions and in other branches of government than those the Department of Agriculture could pay. The Secretary laments that staff cannot be long retained at current rates of compensation (*Yearbook* 1896: 16).

²⁷ For example, Gardner: "Milton Whitney (1860-1927) probably was the most influential of all US soil scientists over a three-decade period, ending with his death in 1927, because of his position as director of the prestigious US Bureau of Soils. At the same time he was the most controversial" (1977: 223). "...America's most influential and most controversial soil scientist, and Hilgard's anathema for a quarter of a century" (Jenny 1961: 89). The terms "controversy" and "controversial" appear regularly in modern accounts of Whitney's role (see also Simonson 1986, Helms and McCracken 1994, Viets 1977: 245).

²⁸ In 1890, while a professor at Columbia, SC, Whitney sent a paper he had written to Hilgard for comments; Hilgard was unable to respond because of "heavy strain of work and weak eyes." After the paper had appeared, Hilgard replied and objected to his ideas on soil moisture and his interpretations on mechanical analysis, in a "friendly tone." (Jenny 1961: 89) After Whitney's 1892 and 1893 papers, Frear,

1891, Whitney, at the Maryland Experiment Station, wrote "Some Physical Properties of Soils in Relation to Moisture and Crop Distribution," a lengthy report on the soils of Maryland, which was cooperatively published by the station and the Weather Bureau of the Department of Agriculture (Weber 1928: 87).²⁹ Whitney emphasized "the physical characteristics of the soil, especially texture, in determining the amount of moisture that the soil could transmit to the crops, and consequently the suitability of the soil to the crops" (Weber 86-87). This study was the first state soil map in the US that showed the distribution of soils classified according to their physical characteristics, and it related soil texture and other physical characteristics to moisture availability in crops. Whitney in fact relates fertility entirely to soil solution, with its availability affected by soil texture, temperature, and amount of soil moisture.

Whitney's work closely matched the Weather Bureau's area of responsibility:

The original object of the Division of Agricultural Soils was to supplement the work of the Weather Bureau by studies of the relation of climatic conditions of moisture and temperature under the surface of the ground to the local distribution of crops. As the water supply of the soil needed for vegetation depends not only upon the quantity of rainfall, but also upon the texture of the soil in permitting the rainfall to enter and be retained in the soil, there arose the need of classifying the soils according to their water-holding capacity. This constituted the original work of the newly created division. (Weber 1928: 87)

In 1892 and 1893, Whitney wrote three further papers from his current base at the Maryland Agricultural College. These dealt with soil moisture, soil texture, and the

alarmed along with other leading soils scientists by their tone and content, asked Hilgard to respond in *Agricultural Science*, which he did in June 1892. Whitney responded again (Sept. 1892) and the controversy continued. Whitney never gained the adherence of Hilgard's contemporaries, but did that of younger US scientists, trained in "the practical, agronomic curricula streamlined to meet local needs" (Jenny 1961: 90). During this initial controversy, Jenny writes, "Whitney gained many lay supporters and eventually he became the trusted favorite of the powerful Secretary of Agriculture, J. Wilson."

²⁹ This report is cited as "Some Properties..." in Jenny (136) and NAL (123); in Helms and McCracken (211; 231) and in Weber (87) it is listed as "The Properties..."

agricultural classification of lands. Another effort of Whitney's also showed his interest in distinguishing and classifying different kinds of soils—his collection and display of 1500 soil samples for the [quadricentennial] Columbian Exposition of 1892. Whitney could be seen as the person with the right interests and the right qualifications at the right place at the right time. Michael Worboys, sociologist of science, sees how sometimes in emerging scientific specialties “social mechanisms [can control] the emergence of new ideas” (1971: 95 n 15). So, as Weber reports, “The publication of the results of Whitney's investigations...was followed, in 1894, by the establishment [by Secretary James Wilson] of a permanent unit for carrying on this soil work, and by the appointment of Professor Whitney as its directing head on March 3, 1894” (1928: 87). Of these same events, Jenny (1961: 93) writes, quoting Whitney's biographer, that Whitney “gained many lay supporters and eventually he became the trusted favorite of the powerful Secretary of Agriculture, J. Wilson. ‘The...Department of Agriculture was so impressed with the necessity for study of the great problems of soil as set forth in Professor Whitney's Bulletin...that it established in the Weather Bureau a Division of Agricultural Soils in 1894, and he was selected as its head’.”³⁰ Contributing to Whitney's success was the provision of maps in his report:

Whitney prepared a small-scale map of Maryland showing the area and distribution of the twelve “principal soil formations, or soil types.” The soils were characterized by mechanical analyses and were designated by geological names and crop adaptations. The papers displayed no interest in the soil body as an object of science, or of processes of soil formation. Yet, there was a map, and maps appeal to people. And the study met a long-felt want. (Jenny 1961: 91)

Added to these situational advantages was a spectacular and unlikely success Whitney had “before the ink on the [first] printed maps was dry” (Simonson 1986: 4). In 1899 the Division received its first federal funding for the mapping of “tobacco lands,” and was investigating land suitable for planting Sumatra tobacco in Florida. Whitney, who had

³⁰ Jenny's quote is from Whitney's biographer, H.G. Knight.

worked briefly in Connecticut, proposed the introduction of Sumatra tobacco in the Connecticut Valley because the texture of some of the soils there was similar to that of the Sumatra-growing Florida soils, and the summer weather was similar. Simonson writes

It seems farfetched now that introduction of a new crop would be proposed on the basis of similarities in soil texture and summer weather. Even more farfetched is the fact that the proposal was successful. As a consequence, Whitney was more convinced than ever that *soil texture* was a prime key to soil productivity. That conviction lasted for some years and led to acrimonious arguments. (1986: 4)

With the support of the Secretary and the encouragement of Congressmen who were provided 9000 copies of maps for their constituents, the Bureau of Soils and the mapping program proceeded (Jenny 1961: 94). The Division received its own allocations of funds and then independent status by increments, becoming the "Division of Soils" in the Department of Agriculture in 1897, and in 1901, it was elevated to the status of "Bureau" (Helms and McCracken 1994: 210; Weber 1928: 85). In 1898, Whitney adopted the large-scale mapping approach used by his colleague/employee T. H. Means to map alkali land in Montana, and this larger-scale work became the basis of mapping that began in 1899. Whitney had begun by working on soil moisture measurement in the Weather Bureau, and by 1901 had become the head of a new Bureau, in charge of all soils investigations for the Department of Agriculture. In this environment of expansion, Whitney—at the right place at the right time—through his work in geologically-based soil research, had arrived at a position from which he could conduct soil mapping, an interest he had expressed as early as 1896.³¹

³¹ On the establishment of the Survey as Whitney's lifetime achievement, Miller (1949); "Prof Milton Whitney..." (1927).

3.3.1. Claims Advanced by the Bureau under Whitney

Whitney's views are expressed in two main areas: soil fertility theory, about which he wrote in various contexts, and theory of soil classification, which was expressed in the context of soil mapping. From his initial work onward, Whitney's views in these areas were consistent with each other and with their geological origin: he maintained that soil texture was the key to fertility and that soil nutrients were carried in and made available to plants via the soil solution. Some of the warrants and implications of this claim appear in texts and testimony presented here; others will be brought up later in the discussion of the Bureau's work. Whitney supported his texture idea with the argument that "if the physical conditions of moisture and heat are favorable to the proper development of crops, plants can, in general, get all the food they need,...for all soils have a large excess of plant food over and above the requirements of even very large crops" (from early work as cited in Jenny 1961: 91). In Bulletin 22, 1903, Whitney wrote "It appears further that practically all soils contain sufficient plant food for good crop yields, that this supply will be indefinitely maintained, and that the actual yield of plants adapted to the soil depends mainly, under favorable climatic conditions, upon cultural methods and suitable crop rotation." Among the assumptions apparent here are that soils are adapted to or 'meant for' certain crops, and that the soil solution is the source of the nutrients plants use; these claims are both vulnerable, and only the second requires anything approaching technical arguments to dismiss.

From experimental work reported in Bureau of Soils Bulletins from 1900 on amounts of soil solutions in various soils and their nutrient content, he reported the conclusion that all soils had saturated solutions, that the soil solutions were in equilibrium with soil minerals, that this concentration of nutrient elements was sufficient for plant growth, that soil solutions constituted the source of plant nutrition, and that minerals in solution would be indefinitely supplied by natural processes:

In all soils there are rock particles or minerals containing phosphoric acid and potash, and in all the soil solutions that we have ever examined—and we have

examined hundreds of them from all parts of the country—you will be astonished to learn that the composition and concentration, which is the nutrient solution spread throughout the surface soil of the earth for plants to grow in and to gather their food from,— you will be astonished to learn that the concentration of this soil moisture is sensibly the same whether we examine your sandy truck soils on your river necks, your sandy clay wheat soils on the uplands, the Hagerstown clay in the valley of the Shenandoah, or the black prairie soils of the West. These minerals are contributing to the solution in which the plant feeds. As I have said, these minerals are difficultly soluble, but they are appreciably soluble. They are soluble enough to maintain a solutions which is amply sufficient for the plants to gather their food from. All soils having, broadly speaking, all of these minerals in them, have approximately the same composition in their soil moisture. This is a very astonishing fact, but, looked upon in the light of our experiments, it is an actual fact that all soils contain sufficient plant food for the support of plants. Further, when the plant takes into its substance some of the mineral matter from the solution, the solid mineral matter from the solution, the solid minerals in contact with the solution immediately dissolve, and the solution is restored to its former concentration. The exhaustion of the soil, therefore, is merely a relative phrase and resolves itself into the question of the rate at which the solution can recover itself. I may state to you that the rate is as fast on an acre planted in our ordinary crops as the demand made upon it by the plant.

(Farmer's Bulletin 257 [1906], 10-11)

Because fertilizers do not always give consistent results, for example, failing to give good results in very wet or dry years, and because, Whitney and Cameron state, the effects of fertilizers are not cumulative, they also argued that fertilizers do not contribute to the actual amount of plant food in the soil.

In his testimony before the US Industrial Commission in 1901, Whitney was being called upon to discuss the exhaustion of soils and abandonment of lands. Whitney spoke as follows

It is doubtless well known to the commission that there are large areas of land in the New England States that have been abandoned; that there are large areas in the Southern States that have also been practically abandoned and given over to waste; furthermore, that there are large areas in the far West which have once been settled and which have since been abandoned or are now held in very low esteem. The cause of the deterioration of the lands in the South has been commonly ascribed to the exhaustion of the soil, and this is the first consideration that I wish to take up. The exhaustion of the soil is due, in my opinion, to changes in the chemical and physical properties of the soil rather than to any actual extraction of plant food. A soil, to be productive, must render annually, as the crop needs it, a sufficient amount of food material in a form available to the plants. As a matter of fact soil is a difficultly soluble substance, composed mostly of silicates and aluminates, or difficultly soluble compounds of silica, alumina, potash, soda, and lime in various forms. Through atmospheric agencies, largely, these compounds are rendered more or less soluble and more or less readily available to plants.³² A fertile soil is one in which the weathering effects come in at such times and to such an extent as to render available to plants a sufficient amount of this plant food. If that weathering does not take place and the food material is not brought into a condition in which it is available to plants, the land is as poor as though it actually contained no plant food.

(1901: 4)

Since soil solutions moved upward, bringing nutrients from the deteriorating rock, through the soil's capillaries, Whitney maintained, the soil had to be maintained in a proper texture. So Whitney emphasized tillage, "intelligent cultivation," assumedly to

³² The relatively much greater importance of chemical to physical weathering was already recognized by many researchers at the time Whitney made these comments.

promote timely weathering effects, and rotations to achieve good texture so that solutions could move efficiently through the soil.

The first stage of Whitney's work, then, maintained that virtually all agricultural soils contain sufficient plant food for good crop yields and that this supply will be indefinitely maintained, that the soil moisture is a natural nutritive solution and that it has essentially the same concentration in productive and unproductive soils, and that this concentration is by natural processes constantly maintained of sufficient strength to meet the needs of crops giving good yields. Whitney's co-author Cameron was asked by the chairman of a hearing before the House Committee on Agriculture in January 1908, "...why is it necessary, or is it in your judgment necessary, ever at any time to introduce fertilizing material into any soil for the purpose of increasing the amount of plant food in that soil?" "Not in my judgment," Mr. Cameron replied.

Whitney may have felt forced to reorder his theory because in 1906, Whitney and Cameron advanced what must be recognized as an *ad hoc* hypothesis. Hempel discusses tests of theories among topoi, or issues of suasion in science, and states that if the initial theory is seriously challenged, alterations may have to be made in the auxiliary hypotheses or new ones introduced. An *ad hoc* hypothesis is one introduced "for the sole purpose of saving a hypothesis seriously threatened by adverse evidence; it would not be called for by other findings and, roughly speaking, it leads to no additional test implications" (1965: 29), and even if not patently absurd or false, it is nevertheless objectionable from the point of view of science because it has been introduced only for this reason.

This new hypothesis, promulgated in the early 1800s by de Candolle and revisited by Leibig, was a claim at the stasis of quality of soil, stating that soils become infertile when plant root excreta or exudates introduce poisons or toxins into the soil. In this theory, the principal offices of nitrate of soda, potash, and phosphoric acid are to correct toxic

conditions in the soil. That is, fertilizers do not provide plant nutrients but rather counteract poisonous conditions in the soil that have accumulated due to lack of proper rotation. Research implications were for finding toxic organic substances, but there was no strategy for detecting "selectivity" in roots' absorption of cations, or there was none delineated by the Bureau: "As we see it now, the main cause of infertile soils or the deterioration of soils is the improper sanitary conditions originally present in the soil or arising from our injudicious culture and rotation of crops. It is, of course, exceedingly difficult to work out the principles which govern the proper rotation for any particular soil" (Bulletin 55, 1909). The first version of this theory was published in Cameron and Bell's "The Mineral Constituent of the Soil Solution," Bureau of Soils Bulletin 30, 1905, which attributed acid soils to selective absorption of cations, leaving the acid anions in solution. Enough was known about exchangeable bases and their solubility at the time for this position to be countered.³³

In Bulletin 55, February 1909, the Bureau's position was as follows:

The soil is the one indestructible, immutable asset that the nation possesses. It is the one resource that cannot be exhausted; that cannot be used up. The general conception of the exhaustion of soils is that the crop removes plant food, and that unless we return some considerable portion of plant food to the soil it eventually becomes incapable of longer producing adequate crops....

The impression prevails that our crops take out phosphoric acid, potash, and nitrates to such an extent that the soil becomes incapable of longer supplying these plant-food constituents for a satisfactory yield.

As we see it now, the main cause of infertile soils or the deterioration of soils is the improper sanitary conditions originally present in the soil or arising from our injudicious culture and rotation of crops. It is, of course, exceedingly difficult to work out the principles which govern the proper rotation for any particular soil.

³³ This claim disproved by Emil Truog, Bureau of Plant Industry.

With USDA *Yearbook* and Bulletin publications and with Bureau of Soils survey work being or having been carried on in many states, word spread of the Bureau's teachings.³⁴ They were being reported in the popular press. In 1909 an Illinois newspaper ran the article "Artificial Fertilizers Said to be all Wrong," which opened "Artificial fertilizers—phosphates and nitrates, chiefly—act upon the soil as drugs act upon the human body, according to investigations just completed by the Bureau of Soils...." And in "Soils Not Wearing Out" from *The Homestead*:

A most comprehensive bulletin has recently been published by the National Department of Agriculture dealing with the question of soil composition. The facts and figures presented in this bulletin tend to show that there is not any immediate danger of the soils of the United States wearing out. Considering the fact that the farms of the United Kingdom, have been under cultivation for a thousand years of more, it is held by Professor Whitney that continuous cropping does not necessarily tend to decrease production. We believe that Professor Whitney's statements will come as a surprise to a great majority of our readers, because the average man labors under the belief that soils are gradually wearing out; on the other hand, it is a fact that our leading farmers, in every state in the Union, are not only able to maintain their crop yield, but they are increasing it from year to year. It is true that there may be annually some loss of mineral elements, but in ordinary good soils, such as our clays and loams, the supply of these minerals is so great that a five-hundred or even a thousand-year period will not reduce the supply to a point where production is materially affected. (in Hopkins 1910: 341)

This press coverage was giving Whitney even further public exposure. Secretary Wilson spoke to the press in 1909 in the same vein of optimism, giving out Whitney's view that

³⁴ The Department's policies were widely commented on as being optimistic. Russell— "a more optimistic report can scarcely be imagined..." (1905: 345); see also Hopkins 1913: 150ff; 1910: 341.

soils appear to be exhausted, but in fact are not.³⁵ Wilson's terms seem to echo those of Whitney from his "Testimony" of 1901. Wilson, (emphasis mine): It was a beautiful country that we passed through, but the farms generally did not show prosperity. Many of the districts looked depopulated. We saw plenty of children in the villages, but few in the rural regions. The country looked deserted. In fact, interest in agriculture seems to have declined. The soils in that state are not exhausted. In some cases they have become unproductive by failure to rotate crops, and again because there has been no change of seed.... We have searched the world for seeds which would flourish in all climates and all conditions, and we are going to increase our production by making use of them" (in Hopkins 1909: 342). Hopkins wrote of Wilson's soil statements in his book, *The Story of the Soil*, "All the world loves an optimist" (1913: 151).

3.3.2. Reasons for Opposition to Whitney

Soil researchers in the US and in England found much that was "radical" in Whitney's publications and a few had been rebutting his ideas since 1891. And trouble had been awaiting him from his appointment; there was resentment that a newcomer was being given the opportunity to lead a federally supported soil mapping effort. Hilgard had been pressing for decades for national soil mapping to be undertaken, and he had himself made, according to Jenny, the first true soil maps—the first in which soil is regarded as a natural body. Hilgard saw soils as complex natural products, affected over time by climate and native vegetation as well as parent material and topography, that is, Hilgard had identified and "correctly handled" the five soil genetic factors (Jenny 1961: 77). Hilgard's contemporaries Lipman, Hopkins, Wiley and even the geologist Shaler were among others who had arrived at the view that soils were independent natural bodies. Hilgard had also differentiated between soils and subsoils (distinguishing soil horizons), which was one of the chief features of classification by the Russian school, which system

³⁵ It was during Wilson's twelve-year term that scientists west of the 100th meridian found themselves crying out in the wilderness—no federal-level support for their objections—in the late 1800s, when blocks in the short-grass prairie were being surveyed into parcels adequate for farming in a humid (eastern) region but too small to support farming in that low-rainfall district (Sears 1971).

was embraced in the late 1920s. But Hilgard was on the other side of the continent and had no disciples to carry his message in Washington, so that the principles he had already made available for mapping were ignored or actually forbidden to Bureau soil surveyors, and his work eclipsed among the "younger generation of soil scientists" (Tandarich and Sprecher 1994; Jenny 1961). The sheer "optimism" of Whitney and Cameron's position, further, lent it an attractiveness not warranted on grounds of scientific soundness. The mapping proceeded on unsound scientific grounds (Jenny 1961; Simonson 1986; Helms and McCracken 1996).

Another reason for resentment over Whitney's work at the Bureau was that many scientists had seen and responded to the shortcomings in theories he had advanced, ever since 1891. They expressed frustration that the record of controversy and correction made no difference among Whitney's "lay supporters," notably the Secretary (to whom the theory may have seemed to satisfy the topos of "elegance"). The result of placing Whitney and his theories in such a position of credibility, they foresaw, was that the work of soil science would be arrested and even set back in the ancient history of soil science. Rothamstead director E.J. Russell alluded to this in his article "The Recent Work of the American Soil Bureau" by recollecting Jethro Tull's 1733 "speculations." Tull taught that if the surface of the soil is increased, roots have more surface to feed from. "Dung and cultivation," Russell wrote, "both increase the surface, the former by fermentation, the latter by actual attrition; both methods can be used, but cultivation is better and cheaper—a man's wages then being 1s. a day—and dung may even be dispensed with" (1905: 337). (Tull's actual words were, "Too much earth, or too fine, can never possibly be given to roots...and earth is so surely the food of all plants, that with the proper share of the other elements, which each species of plants requires, I do not find but any common earth will nourish any plant" [in Hopkins 1910: 301]). Having discussed Whitney's work from 1892, 1898, 1902, 1903 and 1904, Russell went on to say, "We are, in fact, back to Jethro Tull's position, that the farmer's business is to cultivate and not manure" (1905: 336). He saw in Whitney's teachings a near equivalent to Tull's—"any common earth will nourish

any plant" with proper tillage—but with the one difference of an intervening period of nearly two hundred years in which virtually all of soil science's knowledge had been accumulated. Whitney's apparent lack of familiarity with some of the most enduring findings discredited him as a soil scientist, certainly for the position of head of a bureau on soils.

Other criticisms Whitney's contemporaries leveled against him included his imprecise use of language. Jenny, discussing Hilgard's response to Whitney, points out that Whitney did not distinguish between the terms "texture" and "structure" (1961: 91-92), the first of which refers to the mix of mineral components and the second of which includes other soil characteristics, for example, about depth of horizons, aggregation, and capillary passages. Elsewhere in using the term "old soils" Whitney conflated the meanings soils that have been cultivated for a long time and soils that have been developing for a long time—chronologically "old." Russell (1905) quoted Whitney and Cameron from Bulletin 23 (1904), "While these figures will undoubtedly vary somewhat as a result of wider investigations, it is believed that they represent closely the average figures for the great majority of cultivable soils as regards these several constituents [phosphoric acid, nitric acid, calcium, potassium]. This statement Russell interpreted as meaning, not that the solutions are identical, but that they are of the same order of concentration. The "ambiguous sentence might seem to imply that the solutions are fundamentally identical, but the figures will not bear this interpretation" (1905: 333-334). In addition to the challenge to Whitney's language, this is one instance of many in which Whitney's evaluation of data is questioned, that is, claims at the stasis of evaluation, warranted by theoretical basis and method of experiment. As when claiming in his testimony that exhaustion was actually due to changes in the physical and chemical conditions, Whitney was partly right, but then his interpretation and subsequent research went in unlikely directions, with the result of wholly wrong conclusions, shown as such, for example, by King, "Toxicity as a Factor in the Productive Capacity of Soils" in *Science*, April 1908.

Finally, Whitney aroused resentment because of his “confidently announced,” very optimistic theories. The first disturbing aspect of Whitney’s optimistic theory-making was that announcing a general theory of soils centering on soil fertility was bound to create tension in a field short on theory. Announcing a clear-cut, simple (but wrong) theory may well have contributed to Whitney’s attractiveness for the Department, especially since it was so optimistic a theory, because it filled a perceived void. Whitney seems to have been attempting to put in place a theory of as broad a scope as Leibig’s, to address the acknowledged theoretical deficit in the field. In the Report of the Secretary for the 1908 USDA *Yearbook*, he made this objective explicit:

Broadly speaking, the investigations of the laboratories of the Bureau of Soils aim to elucidate and make clear, and if possible give quantitative expression to the interrelations between these fundamental and natural soil phenomena and the known methods of control, with a view to improving the latter, reducing them to a logical basis, and removing them from the conditions of empiricism in which they have so long remained. (1908:95)

But general awareness of the state of knowledge in the field was not adequate at that point for researchers to converge on a disciplinary theory adequate to explain soil characteristics and the management practices they require to remain fertile under the continuous cultivation most were subjected to. The “culture” of the discipline, or its paradigm, was still geologically oriented, and geology could explain very little of the world of the soil.

Although with the influence of Leibig’s research agenda and that of geological guidelines for mapping, soil science had programs of scientific activity, it was indeed still pre-paradigmatic in 1900 (see, e.g. Viets 1977). Leibig had advanced a general theory for the field—effectively dismissing its predecessor the humus theory—and his work had the long-term benefit of developing good apparatus and experimental techniques and prescribing fruitful research paths, but most of Leibig’s theoretical claims had been discredited. Soil

chemistry, physics, management, microbiology, etc., all had research agendas, but there was no unifying theory. Science sociologists Böhme, Van Den Daele and Krohn describe such a state in their article "Finalization in Science":

Before any general theory of its subject is in sight, a science more or less resembles the experimenting of the amateur inventors at the beginning of the modern era: the research follows no definite theoretical or methodological program; scientific strategies are widely determined by techniques of inquiry, the main goal being discovery, not explanation (pre-paradigmatic science in the sense of Kuhn...). In this pre-theoretical phase, a discipline has to some extent the possibility of selecting its research problems in line with given external goals. (1976: 314)

Continuing in Böhme *et al.*'s terms, the branches of soil science were proceeding on a functionalist basis, which is common when a valuable research goal is to attain control over a complex system; in soil science the goal was clearly to discover how to maintain soil fertility. (Soil fertility is indeed so complex that even now soil scientists acknowledge difficulty just defining it, let alone describing it, as Viets 1977: 243). A "reduction in theoretical claims" is characteristic of a field aiming for "the management of complex subject matters." Böhme *et al.* continue:

A complex system cannot be reconstructed unless its mechanisms are understood but it can be handled strategically, provided, whatever mechanisms may be operative, its most important functional structures are known. Of special relevance to the problem area under discussion is that the emergence of sciences with reduced explanatory claims renders possible a fusion of science and technology and an orientation of scientific development towards external goals without the interposition of a phase of theory construction in the traditional sense. This is possible because the lack of explanatory power of the science does not preclude the utilization of its findings for specific purposes (of control)....Functionalism, therefore, is a tendency enhanced by the complexity of the object. (1976: 314)

Knowledge of "important functional structures," although empirically rather than theoretically gained, was growing, and there was a core of knowledge representing hard-won results in this extremely complex field. Hopkins concluded his chapter XVIII, "Theories Concerning Soil Fertility" (mainly addressing the Bureau theories), in an emphatic tone, with a short list of "four great fundamental facts of plant nutrition [that] still stand against every test":

thus, Senebier's proof of the fixation of carbon, oxygen, and hydrogen by photosynthesis, De Sature's discovery of the presence and absolute necessity of mineral plant food, Lawes and Gilbert's proof that the soil must furnish the nitrogen for most plants, and Hellriegel's discovery of the fixation of free nitrogen by the bacteria of legumes always lead to the same conclusion whenever, wherever, or by whomsoever they are repeated. They are fully recognized as absolutely established facts, at least as well established as the fact that the earth is round. (1910: 342)

Studies of plot results at Rothamstead and the Pennsylvania experiment station had been published and were available for analysis, and Hopkins made careful heuristic uses of their records. His book includes well over one hundred pages of detailed analysis of plot studies from Rothamstead, Pennsylvania, Ohio, Illinois, Minnesota, and Canada. From long-term plot results Hopkins could show that it was known that nitrogen can quickly become a limiting factor and that phosphorous is often the limiting factor. Hilgard had gotten a long way towards reconstructing chemical weathering processes as affecting soils when he drew attention to climate-related differences in soils (1892). Among the many "open questions" under study was the ammonia-nitrite-nitrate sequence of nitrogen transformations and the part of microorganisms in fertility. Another was the movement of water in cultivated soils. Another was the cause of poor growth associated with acidity, which had not yet been linked to the (known) resistance to solubility of Al and Mn. The (known) ready solubility of the base minerals was leading into a more focused study of

cation exchange. All in all, there was a lot of knowledge that Whitney seemed not to take account of.

“Working knowledge” in soil science was plentiful, but explanatory theory was not. There was too much contradictory evidence in the many sub-fields contributing to soil fertility studies for anyone to be able to “reconstruct the system” theoretically. Again, Böhme *et al.* :

We can observe a particular tendency in the science system to establish the knowledge of functional relationships as a legitimate end of scientific inquiry. The recognition of functional relations, for instance in an input-output model [e.g., fertilizer-yield], has at all times constituted an important step in scientific advance. Yet it was always looked upon as being merely the “pre-form” of the scientific explanation itself, which was expected to establish the necessity of the relationship asserted by disclosing the mechanisms underlying it. (1976: 320)

Whitney was relying on a geological view of soils to offer a causal explanation of soil fertility, a coup if successful. Opposing him were researchers from state colleges and experiment stations, who knew that soil was more than rock particles of different sizes with some organic matter mixed in and nutrient-saturated solution rising between the particles. They had longer experience or greater awareness of the two-steps-forward, one-step-back history of knowledge gains in their field, and a sounder grasp of the processes of soil-plant relationships which did not make them optimistic about the “permanent fertility” of soils. They shared a conviction that maintaining soil fertility was a matter of life and death, and so they were willing to work in theoretical uncertainty, under a functional model. This solidarity of opinion about a flawed theoretical position could be regarded as “intersubjectivity” about the qualities of soils. In this they agreed also with better-informed lay people and scientists of other disciplines; thus, while exhaustibility of land was agreed upon by these soil scientists, their conviction was held not, as Gross

argues, because of mutual suasion, but rather because their view reflected qualities in nature.

The importance of the goal of maintaining fertility in the view of soil scientists outside the Bureau cannot be overstated. In his 1910 book, Hopkins wrote:

It is the first business of every farmer to reduce the fertility of the soil, by removing the largest crops of which the soil is capable; but ultimate failure results for the landowner unless provision is made for restoring and maintaining productiveness. Every landowner should adopt for his land a system of farming that is permanent,—a system under which the land becomes better rather than poorer. (xvii)

And H.W. Wiley said, in his address to the convention of the Association of American Agricultural Colleges and Experiment Stations in 1904,

The aim of the practical farmer is to return to the soil just as much as possible of what he takes out, and even a little more....No matter what we may do we must look forward to the time when the soil must be fed. We owe a debt to posterity. We have not right to take from posterity the means of subsistence. The agriculturist of today who does his duty to his farm must pass it to his successor in a more fertile condition than that in which he receives it...." (146)

Wiley's and Hopkins' emphasis on permanent fertility represents their effort to maintain discursive space for this topic in the face of growing exposure for the Bureau's "permanent fertility" stance. Their commitment on this point can be contrasted with further statements of Whitney before the Industrial Commission, 1901 (continued from that cited earlier):

To emphasize this statement, which may appear at variance with the general ideas concerning the exhaustion of soils, I would call the commission's attention to the many cases in which soils have been cultivated for hundreds and thousands of

years. [examples given: India, Egypt, Southern and Northern Europe, Holland, Denmark, France...] There is one phase, however, that it would be well to dilate upon here, namely, that with our increase in density of population and with the competition that has been going on, we are no longer satisfied with the yields that are naturally obtained from many of our soils, and we have resorted to the practice of fertilization in order to force plants to produce far beyond what the natural fertility of the soil will give. (4-5)³⁶

Here Whitney implied that the only treatment given to soils thousands of years in agricultural production is cultivation. And he went on to blame the concept of "exhaustion" of land on high population and the subsequent impatient demand for unnaturally high yields. As Russell commented, this view takes agriculture back to the time of Malthus. In his Bulletin 55 (1909) Whitney reiterated the observation that yields in Europe have risen, but with no allusion to fertilization, and at this point, he correlated population increases to rising rather than falling yields [my italics]:

Lastly, it has been shown from the statistics of European countries that the soils of the world are not wearing out, but that, on the contrary, after a thousand years of cultivation, with the introduction of better methods, *with the necessity of raising larger crops*, these soils are responding with an increased yield even over what they produced at the beginning of the last century.

F.H. King, possibly in response to Whitney as well as for his own purposes, toured China, Korea, and Japan for about nine months in 1909 to study agricultural practices, and he wrote his well-known *Farmers for Forty Centuries* on the ceaseless and comprehensive work of the rural population there to return to the soil every bit of matter having conceivable nutritive value. Hopkins, arguing explicitly in response to Whitney, went to

³⁶ Cp. Whitney, p. 79 in the 1907 *Yearbook*: the Bureau is studying fertilizers "in order to effect a saving in the fertilizer bill of the country. Field investigations have been made to increase crop production on extensive areas of different soil types which, with the usual methods, do not yield particularly remunerative crops. In many cases slight modifications of the methods of mechanical handling of the land increase the crop-producing power of such soils to a marked degree." Nowhere in text written by Whitney is there language suggesting a commitment to helping the landowner get really good yields.

some length in his book to correlate tons of fertilizing materials imported to Britain and Germany (since the very year Leibig announced his mineral nutrition theory) and increases in yield. He makes a point that tables he uses showing rises in yields are those Whitney used, but the latter omitted any reference to—and so implied the absence of—fertilizer use. Hopkins also responded at length on agricultural practices in China and in India, with a long passage commenting on desperate conditions in rural India (1910: 333-336), and a similarly long passage on China (1910: 336-338), responding to Whitney's comments on permanent fertility.

Meanwhile, Hopkins had this forward-looking view of the need and the propriety of high production:

Likewise the 100 bushel yield of corn is of immediate interest, for it has been produced—and can be produced throughout the corn belt in normal seasons with good farming on the richest and best-treated soils; and the production of large yields is an essential consideration, both from the standpoint of profitable farming and for the future support of a rapidly increasing population. (60)

Whitney had already drawn criticism over the theses he had put forth in his earlier work at the Maryland station and at the Weather Bureau, but “bedlam broke loose” in 1903, when he published Bulletin 22, *The Chemistry of the Soil as Related to Crop Production*. This bulletin had the fullest discussion so far of the claims of the Bureau, for example.

All our principal soil types, in fact, practically all cultivable soils. contain normally a nutrient solution which varies within comparatively narrow limits with regard to either the composition or concentration [of the soil solution]. It appears further that practically all soils contain sufficient plant food for good crop yield, and that this supply will be indefinitely maintained, and that the actual yield of plants adapted to the soil depends mainly, under favorable climatic conditions, upon the cultural methods and suitable crop rotation.

The Bulletin was published in late fall, and in mid-November the 17th annual convention of the Association of American Agricultural Colleges and Experiment Stations was held in Washington. Papers were presented by Hopkins, H.W.Wiley, and Hilgard (read in his absence by Thorne) which attacked the Bureau's bulletin in the strongest terms. C.E. Thorne of Ohio and E.B.Voorhees of New Jersey presented papers less overtly critical, Thorne's, for example, stating his "conviction" that conclusions relating to soil fertility should be tested in the field (the Bureau's rested on 20-day solution trials). F.H. King read a highly technical paper representing his own research for the Department. Whitney was there and also presented a paper. No-one but Whitney there at the time apparently knew that King was about to be fired and that his research, used as the basis for Bulletin 22, had been published without King as an author because he had refused to sign on to the interpretation given to his work.³⁷ Beyond challenging Whitney in person at this forum and in print throughout the rest of the decade, reaction to the Bureau's work took a number of other forms.

First, and in the same context, Hilgard had a resolution, requesting an examination of the tenets of the Bulletin, prepared to present to the Secretary of Agriculture. The Secretary appointed three examiners, all of whom were from the Department of Agriculture, one of whom was Whitney and a second was another "chief sinner" (Jenny 1961: 97). Evidently a "whitewash" resulted. A second response was to counter Whitney in *Agricultural Science, Science and Journal of Agricultural Research* on the basis of his claims in his own texts. King (1908, 1911), Hilgard, and Hopkins in the US, and, in England, E.J.Russell (1905) from the Rothamstead experiment station, also took this approach.

Hopkins' long-term approach was to consolidate and make available to the reading agricultural public and students a more accurate picture of soil conditions and fertility.

³⁷ Whitney had had three of King's six reports published as Bulletins. He rejected three others, and King, who returned to Wisconsin in January 1904, had the remaining three published privately with the approval of the Secretary of Agriculture. The papers "were in direct conflict with the thesis offered by Whitney and Cameron..." (Tanner and Simonson 1993; Simonson 1987: 69; on the use of King's data, Jenny 1961: 97).

This he undertook in his massive and comprehensive text of 1910, *Soil Fertility and Permanent Agriculture*. Not only does the text devote a chapter to prosecuting the debate with Whitney explicitly, but often elsewhere in the book the controversy is alluded to, and also elements of text are frequently ordered in a more overtly argumentative than pedagogical way, seeming to address specific arguments. Another approach was to tackle questions raised by Whitney's theories; opposition to Whitney in all these forms generated a boom of new knowledge. The relatively greater importance of chemical than physical processes was one very general precept demonstrated in response to the Bureau; a lesser but long-troublesome issue, the connection of Al and Mn toxicities and Zn deficiencies with soil acidity, was also recognized in the research work of this period. Mention of Bulletins 22 or 257 are frequent in opening paragraphs of articles from, roughly, 1907 to 1920.

In late December of the same year, Hilgard, on the advice of H.W. Wiley of the Bureau of Chemistry, wrote and initiated circulation of a petition to be sent to the Secretary requesting a committee of competent soil scientists to consider Bulletin 22. The petition had to be dropped, however, because many of the heads of experiment stations would not sign it. They were unwilling to antagonize the Secretary at the time when a bill (the Adams Bill) proposing increased appropriations for the stations was being considered. This was an opportunity missed for "striking while the iron was hot."

In 1908 Whitney and Cameron testified before the House committee on agriculture, at which time Dr. Cameron stated that many agricultural colleges were using the Bureau of Soils' bulletins as text-books and that their theories were being taught there. As recounted by Hopkins (1910: 349-350), Dr. Alfred Peter of the University of Kentucky then, in January of 1909, sent a letter to "professors of agriculture, agronomists, and agricultural chemists" in the state land-grant colleges and experiment stations, summarizing the teachings of the Bureau and asking "whether or not these views are accepted and taught" by the recipients; Dr. Peter proposed to publish the response data. In under a month, he

was sending out the results of his survey: of his 104 respondents, not one of the 102 from land-grant colleges or experiment stations accepted or taught them as established facts or without qualification. Dr. Peter stated that the responses came from 47 states and territories, "showing a very general interest in the subject of inquiry." There had been a letter from Dr. Cameron to Dr. Peter, which the latter enclosed, taking exception to his summary of the Bureau's position and restating it in his own words; respondents were invited to write again if Dr. Cameron's letter made any change in their view of the subject (none did). The publication date of Hopkins' book is 1910, and no further information is given about this approach to the Whitney-Cameron problem.

3.3.3. Reasons For The Heatedness Of The Controversy

The controversy involving Whitney was quite openly adversarial. What made it such a heated controversy? Examination of the controversy through a rhetorical lens suggests the reasons are of two kinds; the first metarhetorical and the second involving bases of argument. We will consider the metarhetorical aspect of the controversy first.

Co-opting Discursive Space

Burke describes as rhetoric a number of non-verbal persuasive initiatives, including and especially the use of symbols; one such initiative is the use of power (Burke 1969). There are two recorded instances in which Whitney took this route to promote his argument. In the first, already introduced, Whitney dismissed King from the Bureau service because his research conflicted with Whitney's theory. A second instance involved soil surveys written by Hugh Bennett and William McClendon; they were censored—their writing changed—because the reported conditions, again, conflicted with what Whitney wanted said. As had Leibig in his time, in his position Whitney controlled what was published as Bureau of Soils documents, and everything he approved for publication went to the Secretary with Whitney's name on it in a letter of transmittal.³⁸ He was able to control a

³⁸ Simonson speculates that Whitney regretted King's having published his papers privately, because when another scientist, George Coffey, presented work of which he disapproved, he released it, recommending

discursive space, and exclude from it what conflicted with his views, which he was setting up as doctrine.

William Frawley's discussion of doctrine in *Text and Epistemology* provide interesting commentary on the controversy involving Whitney. He writes,

If textuality results in analytic procedures in the linkage of text-based information, the consequences of such an activity are going to differ markedly from those resulting from aggregative or additive procedures. What does result is a sense of knowledge as an object of scrutiny. In this way, alternative systems of knowledge develop since individuals are subjecting various premises to different analytical procedures. Arguments across different systems of knowledge give rise to doctrine. The result is that textuality and analytic logic produce *doxa*. There is no such thing as doctrine unless there is also some body of ideas against which to judge the doctrine. Doctrine is no autonomous body of language which can always be brought to bear on problems; doctrine is argued truth, and argued truth can be achieved only through the subordination of text-based information and debates on the consistency of the information. In short, *doxa* requires the apotheosis of one system of knowledge over other systems and, consequently the suppression of the other systems...*Doxa* requires the maintenance and growth of one system *at the expense of others*: *doxa* is naturally connected with games of power. One begins to understand, then, why philosophies of science such as Kuhn's...are intuitively appealing.... (1987: 52)

Prior to Whitney's being appointed to head the bureau, soil scientists were carrying on normal science, creating "commentary within a discursive space" which was not theory-based but which had well-defined problems and recognized texts and "legitimated

publication but repudiating the manuscript "because *it did not represent* the view of the Bureau." Whitney's letter of transmittal gave, in criticism of Coffey's work, the basis of classification of soils according to the Bureau: "The primary grouping is based upon the origin and processes of [soil] formation, rather than upon the characteristics of the soils themselves" (Simonson 9, 10). (This is also one example of many of circular logic in Whitney's arguments.) By 1906, Whitney was referring to the findings of Bureau investigations as "laws" (Bulletin 257).

questions.” Texts which were particularly valued were the results from the well-designed, long-running, and reliable field plots at Rothamstead, and in that very time, King’s highly-regarded studies. “How to maintain or improve fertility in different kinds of soils,” a functionalist objective as we have seen, broadly states the most important open question, and it broke down into others, such as how to retain more nitrogen in soils, how to draw more phosphorous from soil reserves or how to prevent it from becoming immobilized, how to conserve soil water—by dust mulching, summer fallowing, or what? etc. The “normal science” activity of state experiment station researchers already in the field suddenly, with Whitney on the scene, was in danger of losing ground—and discursive space—of being co-opted or appropriated (as was the case with Hilgard’s alkali work), and of being cast as irrelevant or as heterodox science.³⁹

This did not escape the notice of Hilgard, who was watching carefully the progress of the Adams Bill (to provide funding to the experiment stations for basic research in agriculture) through the House so as to prevent any clause giving the “Wilson-Whitney combination” authority over the work at state colleges and experiment stations. Jenny reports from Hilgard’s letters that “such a rider was indeed to be attached,” with the result that Whitney would have sanction to extend his control over expression of scientific thinking on soils at state experiment stations. About this Hilgard wrote, “He [Whitney] is to have authority and is directed to ‘unify’ agricultural work in the US, and wipe out all heterodox science” (Jenny 1960: 99-100). In *Science* in Nov. 1904, Hilgard presented the scientific community with the challenge to defend their right to think and publish what they thought was right:

Such being the official orthodox doctrine, it becomes clear why [King’s report] showing pointedly the very reverse of the official doctrine to be true, could not receive the official approval and *imprimatur*.... It now behooves the scientific men

³⁹ Hilgard wrote in *Science* in Feb 5 1904 that the Secretary of Agriculture “has given expression to ...sentiments...that the universities are doing nothing for the promotion of agricultural science; and that at the time he came to Washington, and until the Bureau of Soils was established, nothing was known about soils—meaning, presumably, the soils of the United States.” (Jenny 99)

of the country to voice their emphatic protest against the dictation of official orthodox science of any kind, from the headquarters at Washington. (in Jenny 1961: 100)

Hilgard had responded to Whitney from the first letter in 1891, but he retired in 1904 and concentrated on his book. Hopkins was likewise working on a book, a major function of which was to respond to Whitney, and regain the ground he saw as being lost under the Whitney Bureau.

In Hopkins' 1910 book, nearly 650 pages long, and written to "set the record straight," the author stated his reason for his strenuous efforts arguing against Whitney's "toxic excreta" theory—it would have credibility because it emanated from the head of a Bureau of the USDA:

While this theory is advanced with no adequate foundation and in direct opposition to practical experience and to so many known facts of mathematics, chemistry, and geology, that it is in itself quite unworthy of further consideration, the fact is that it has been promulgated by Professor Whitney as Chief of the United States Bureau of Soils, and by Doctor Cameron, as the chief chemist of the same bureau; and, consequently it cannot be ignored. (313)

Hilgard and King expressed the same views—that the information had to be contradicted because of where it came from rather than because it was worthy of debate (1903 and 1908 respectively). Whitney's arguments elicited such vehement condemnation, not just because they were, in the main, wrong, but because they were presented as the official doctrine of the federal government, which—by virtue of the set of circumstances we have just examined—*they were*. Whitney's doctrine was allowing no discursive space for soil degradation, analysis of fertilizing materials, or analysis of change in soils; stated over and over again is the claim that soil degradation is apparent only, not real, and that the approaches to soil fertility taken so far are unenlightened vestiges of the past, misguided, or naive.

While the state college and experiment station soil scientists hardly took kindly to this attempt to shoulder them out of place in the public esteem and the discourse of their field, what they found most objectionable in Whitney's doctrine was the anticipated results of his advice being taken. They anticipated that landowners' resorting to tillage as though it would replace fertilization would leave the soil permanently exhausted. In Hopkins' view,

The possible enormous and irreparable damage of such teaching lies in the fact that even our remaining supply of good land will ultimately be depleted by the present practices beyond the point of self redemption, thus repeating the history of our abandoned Eastern lands, where the rotation of crops was the common rule of practice for more than a hundred years. (1910: 341)

The detrimental effect on soil structure of "tillage and more tillage" had been recognized by de Sature: "The destructibility of this vegetable earth is an undeniable fact; those agriculturists who have endeavored to avoid the necessity of manuring their land, by bestowing repeated plowings upon it, have been convinced of this fact by painful experience. They have seen the soil become gradually more and more exhausted, and their fields rendered sterile and barren, by the absence of vegetable mould." (de Sature in Waksman 1938: 332). Soil will show "loss of structural organic matter, deterioration in permeability, and excess accumulation of nitrate" [making it acidic] over as short a time as a decade, thus changing in the 'physical characteristics' Whitney was exclusively concerned with. But de Sature had also related the fertility of the soil to water-soluble mineral constituents, and suggested that they can be absorbed, exhausting the soil and rendering it infertile, without showing any differences in physical properties from those of a fertile soil (Waksman 1938: 348; 12). This was of relatively little significance among the store of empirical findings that went into the functional knowledge of the discipline for soil scientists, especially European-trained ones who were familiar with the classical and uncontroverted findings of Senebier, de Sature, Lawes and Gilbert, Hellriegel, and many more. Their concern was that farmers moved by the Bureau's suasion would not take steps to maintain soil fertility, and legislators would make no laws to support

agricultural security as related to permanent fertility. This is the second reason scientists were so upset—because of the implications for action of the Bureau's teachings.

Disastrous Implications for Action: the Fourth Stasis

In "Toward a Modern Version of Stasis," Fahnestock and Secor suggest that stasis analysis may be suitable as a field-independent heuristic for invention, and also offer examples of stasis as an analytical tool. Prelli (1989: 146ff) combines a set of scientific topoi with the stasis questions to create a matrix for analysis of debate in scientific questions. The stasis questions independently or combined with topoi seem quite suitable for analysis of this juncture in soils science, and may be seen to illustrate Fahnestock and Secor's view of the stases as recursive and complex. The points in Whitney's statements that most elicited the criticism of other soil scientists was that of action—*judgement* in classical forensic terms, the fourth and last or highest stasis. They were moved to speak out and write in the hope of preventing people from accepting Whitney's suasion and neglecting to work at raising the fertility status of their soils. Maintaining soil fertility was, in their view, a matter of the utmost significance.

Whitney's statements generally imply rather than state directly "fertilizers are unnecessary," but the implication is strong that farmers do not need to fertilize. His Bulletins do not recommend the use of fertilizers, but rotation, growing legumes, and "intelligent cultivation," since "practically all soils contain sufficient plant food for good crop yields, that this supply will be indefinitely maintained." In later Bulletins and Reports of the Secretary (e.g., 1907: 79), Whitney states the interest of the Bureau in studying fertilizers *so as to reduce the nation's fertilizer bill*. Despite the relative soundness of their position on soil fertility and the importance of maintaining it, Whitney's critics in research at the state experiment stations could not dislodge Whitney from his position as official spokesman for the best and most current knowledge in soil studies, nor could they effectively overcome the power of federal publications as official organs for the purveyance of this knowledge. This situation in the Bureau of Soils meant

that, as far as promulgation of effective and competent soil management advice was concerned, most of the first two decades of the 1900s were spent, not in the achievement of significant soil research and fertility conservation goals, but virtually "marking time" in the effort to address basic disciplinary questions. This being the case, as a discipline, "soil science" was not united in its characterization of basic points, and was in no position to promote an effective approach to soil conservation to a national audience.

3.4. WHITNEY'S BUREAU OF SOILS: ANALYSIS AND DISCUSSION

The initiation of a federal Bureau of Soils must have seemed, to the working soil scientists at the time, an excellent opportunity to launch a strong national discourse on soil management to maintain fertility. But because the Bureau of Soils evolved from a lesser office, and Whitney's position with it, there seems never to have been a review of potential administrators qualified for the work. Whitney was not as well-educated as many of his subordinates or many of the heads of the several experiment stations, and neither did he have a strong background in farm work; on issues of management for improvement of soil texture, he literally never specified even basic practices. As for having attempted to shift the ground of inquiry to a theoretical rather than empirical basis, Whitney failed in this for two reasons.

First, the theoretical position Whitney attempted to shift to remained unsound: mineral nutrients were not primarily carried in the soil solution, and texture was rarely a sole factor in problematic soils. While it may have appeared that too many basic connections between soil processes and soil constituents remained obscure, in fact, many of the fundamental soil facts had already been strongly demonstrated, even while some of the positions Whitney persisted in contributed to an appearance of continuing obscurity. At the core of the problem was that Whitney insisted on rejecting the sum of empirical lore that had been accumulating over several decades. Whitney maintained, even in 1922, that silica dry-ground in a mill to the dimensions of clay particles, .0020-.0001mm (2 microns to .1 microns), would display the characteristics of clay. Silicate material remains inert,

however, under this treatment, because it is not chemically active, whereas clay, a secondary mineral, is constructed of complex silicon tetrahedrons and aluminum octahedrons which chemically capture and hold exchangeable cations between layers of tetrahedron and octahedron sheets.⁴⁰ The chemically-active nature of clays was certain at least by 1913, when Sir E. John Russell described them as such, in his discussion of the textural components of soils, in his *Soils*, a monograph aimed at interested lay audiences. The heat-of-wetting of clays, known since 1901 (Baver 1948), and the tenacity of clays in holding water had been taken as demonstrations of this difference for over two decades at that point.

The second reason Whitney was not able to shift the discipline of soil science to a more theoretical ground was that he would not, or more likely could not, lead the way into a theoretically based soil science. His language in his publications suggest that he did not or was unable to recognize the significance of and connections among landmark research studies, either classical in the field or current. Certainly he made only rare mention of the work of others, and his practice was not to cite research. Hopkins stated to those present at the 1903 Convention of American Agricultural Colleges and Experiment Stations that Bulletin 22 was based "in part upon a cursory examination of the literature bearing on the subject...because of the large amount of existing data which appear to have been overlooked" (1904: 100). Hopkins goes on to discuss a "cursory" interpretation of data, from Rothamstead, that *was* consulted, but misinterpreted to support his own position. Whitney did the same with graphs from Rothamstead and elsewhere in his 1925 *Soil and Civilization*. Whitney's peers evince palpable anger at his interpretations in their writing, e.g., Russell 1908 and King 1911.

While he mentioned important aspects of soils research early in his work for the Department of Agriculture, Whitney seems not to have integrated these research findings into an understanding of soil processes. In a *Yearbook* article, he mentions Ruffin in

⁴⁰ Ammons 1996; Brady 1984; Frederickson 1952; Singer and Munns 1991.

passing but with no indication of the importance of his work (1899: 338). In the same article, he referred to soil microbiology studies at some of the state experiment stations, but he does not mention their work that had been in progress in the Bureau of Plant Industry (BPI) from as early 1895, and he seems never to have tried to attract soil microbiological studies from that Bureau, although these were primarily soil-based studies, and were to be pursued as such at Rutgers under Lipman. At the BPI, studies based on Schloesing and Muntz's 1877 and Hellriegel's later experiments, had shown the importance of microbiological activity to soils—soil fertility, soil structure, and soil water-holding capacity. If he had been more conversant with the literature of his discipline, Whitney would have wanted to integrate this and humus studies, also being conducted at the BPI, into the Bureau of Soils. (In 1927, the year of his retirement and his death, these areas of study were indeed integrated.) Of greater significance is the issue of acidity. Calcium was known as a flocculant from the mid-1800s, at which time it was used to separate organic solids from water so that water could be decanted off of the solids, making it fit for drinking. Most critically, calcium caused clays to form aggregates, thus improving soil structure and with it other physical attributes. Hilgard established this principle in an article in 1879, "On the Flocculation of Particles, and its Physical and Technical Bearings." Of this article, Jenny wrote, "The flocculation theory of good tilth is of paramount significance in the understanding of soil structure, and still constitutes a pillar in the fabric of theoretical and applied soil physics" (1960: 21). Although soil scientists and soil surveyors, the majority of whom, unlike Whitney, had farm backgrounds, showed evidence of being able to integrate such information, Whitney seems never to have done so.

By 1910 or 1911, approximately, Whitney seems to have tired of the battle with opposed soil scientists, as he seems to have continually drawn contradiction rather than approbation upon publishing. Bazerman noted in *Shaping Written Knowledge* that when "results are startling" or a researcher is "stepping on someone's toes"—in other words, when a researcher challenges accepted findings in the discipline—the "demands of proof

are higher...thus even standards for public argument are situational, depending on the degree of competition and conflict" (1988: 251).⁴¹ While Whitney retained his position as head of the Bureau, he withdrew almost completely from publishing, and he delegated the direction of the Soil Survey to Curtis Marbut. Between 1892 and 1909, he had written 47 publications. In 1909 and 1910, he wrote six Bulletins, all on fertilizers for soils characterized by the crop produced, e.g., Bulletin 62, "Fertilizers for Cotton Soils," and Bulletin 64, "Fertilizers for Corn Soils," and Bulletin 67, Fertilizers on Soils used for Oats, Hay, and Miscellaneous Crops" (based on the assumption, "not generally admitted," that soils are "adapted" to given crops). The conclusion Whitney states in these bulletins is that chemical analysis of soils for mineral nutrients was not a reliable indicator of yield, and that the application of fertilizers would likewise not guarantee a good yield.

After publishing these Bulletins, Whitney was involved in the reorganization of the Survey and later with the reconnaissance of potash reserves in light of Germany's control of most of the previous known world reserves (Miller 1949; Lapham 1949). In the last 16 years of his service as the head of the Bureau of Soils, Whitney wrote: a Bulletin on the reorganization of the Survey, two internal mimeographs, two short (2 p.) articles in *Science*, and his book (*Dictionary Catalogue*: 121-123), very little in comparison to the amount of work he produced in the previous twenty years. Open warfare in the exchange of publications ceased, but soil research both in and outside of the Bureau was being carried on at a frantic pace, a great deal of it generated by opposition. "No man can be entirely wrong, but undoubtedly the writings of Whitney and Cameron held back research on soil fertility, the use of fertilizers, and soil conservation for some time in the United States" (Viets 1977: 245).

Because of the conflict related to Whitney, institutional matters assumed disproportional importance during his tenure at the Bureau, for the first 15 years especially. State experiment stations resisted encroachments by the federal bureau on their work and their

⁴¹ See also Prelli 1989, p. 200, and Zagacki and Keith 1992.

audience (Jenny 1961). Despite the fact that the fundamental soil facts for preventing soil exhaustion were largely available, the Bureau was preoccupied with the Survey and with relatively unproductive research in toxic organic compounds of soils, the findings of which were meant to support Whitney's toxic excreta theory. This, the first federal organization for soil research and research coordination, could have taken a badly needed central leadership role. It could have done so by promoting the use of the now-standard practices and amendments such as restoration of organic matter with long pasture rotations, liming combined with NPK fertilization, and the use of legumes to fix nitrogen, the value of which practices had been substantiated in its own and other Department research.

Had it not been for its preoccupation with internal issues, the Bureau might also have paid some attention to the severe problems of water erosion in the southeastern states, problems that were also appearing on the loessal bluffs of Missouri and southern Indiana, Illinois, and Iowa. At the core of the Bureau's concerns was the conflict between the two early paradigms, geology and soil chemistry; partisans of both camps attempted to establish the basis of fertility within their own frame of reference. Both views reflected a geological appraisal of soils as pulverized rock, and regarded the nutrients as being held in solution in this medium; both have the fertility needs of the crop, rather than the nature of the soil itself, as the basis of assessment. As a consequence of the continuing predominance of these views, there was no agreement on the seriousness of erosion, the loss of the soil body itself. For his part, emphasizing soil texture and a soil solution rising from newly deteriorated rock, Whitney looked to the degradation of the underlying rock as the source of fertility; in the view of soil scientists who placed most emphasis on chemistry, loss of nutrients from within the soil was the primary cause of declining crop yields. Both perspectives placed little value on the soil body, which is why it was revolutionary for Chamberlain to emphasize the soil body in the 1908 Governor's Conference.

Beginning early in the Soil Survey, however, serious soil erosion as gullies was repeatedly being mapped on rich loess soils, and, in the southeastern states particularly, surveying revealed poverty-stricken areas of essentially sterile soils characterized by impermeable exposed subsoils, liable to extreme hardness and cracking with drying. These soils were being ruined or had been ruined by the particular combination of cultivation practices, crops, and climatic conditions in these regions. As with the answers to problems of soil exhaustion, the pieces of the puzzle were all available. But not only were these problems not being addressed, they were not even being acknowledged. In one of the first surveys that attributed the poor condition of the land to erosion, the passages discussing erosion were edited out, presumably by Whitney, as he had editorial control of the publications of the Bureau (Swain 1963; Buie 1961). Another survey, of Fairfield County, SC, in 1911, mapped 90,00 acres of once productive land as "rough gullied land" (Simms 1970: 8; Brink 1951: 21). These and similar surveys, however, were received with indifference both by the public and by the Bureau. The farming audience was, on average, enjoying greater relative prosperity than ever before; the decade before WWI became the benchmark ("parity") against which farm prosperity relative to the non-farming public would be measured. It was perhaps understandable, then, that the surveys did not raise an alarm among the farming or the non-farming audiences. But the Bureau of Soils disregarded these reports of erosion because it had its own problems. These were internal: the ruling exigency for the Bureau administration was to justify its research and establish credibility, which it never did without changing its theoretical orientation. During the period that Marbut directed the Soil Survey and under Marbut's leadership, soil science made greater strides towards the status of a true science than it ever had before, becoming established for the first time on a basis of inquiry relating to the soils themselves rather than to the rocks beneath them or the plants they support. But because Marbut's work was directed toward the resolution of questions of fact, the non-activism of the Whitney Bureau continued; Marbut wrote groundbreaking scholarly work but not guidelines for soil management. This important theoretical and disciplinary breakthrough came at a time when it seemed to state experiment station researchers that the need for

practical work in management was urgent. In the following chapter, we will look at the situation in which this urgent need was addressed, and applied soils work was taken up in the pursuit of soil conservation.

CHAPTER 4
THE SOIL EROSION ISSUE:
RHETORICAL SITUATION AND INVENTION
IN THE 1930S

INTRODUCTION

As we have seen in the previous chapters, soil exhaustion, soil erosion, and their consequences were a long-standing problem, prevalent in many of the older sections of the US. Geologists and soil scientists had written and said a great deal about soil problems, but still, in the mid-1920s, only the most intermittent and piecemeal remedies were being applied to this problem. The mid-1930s brought a significant change in this situation, and an approach to soil problems that has brought continued benefits since its inception. This change in approach was the assumption of responsibility for preventing the deterioration of the soil base. It represented a change of mind based upon the recognition that this resource was essential to the prosperity of future generations, and it represented an acknowledgment of the obligation of the present generations to the future. Virtually all commentators on this change mention one or two of a small set of factors, coming down to four factors that are referred to again and again as having coincided to bring about this change. These factors are 1) the end of availability of open land, 2) the Depression, 3) the Dust Bowl, and 4) the leadership of Hugh Hammond Bennett.⁴² Our aim in this chapter is to come to an appreciation of the interplay of rhetor and situation in the successful outcome of this case. Accordingly we will examine Bennett's use of rhetorical invention in formulating and solving a key problem, then his perception and response to the current disciplinary and legislative atmosphere and opportunities. Next we will look at the steps Bennett took to clarify the processes of erosion and to promote the perception of soil erosion as an exigency, and finally at the convergence of his drive with the three other situational factors that finally brought about the adoption of policy. In this

⁴² E.g., in Swain 1963; Batie 1983, 1984; Rasmussen 1982; Held and Clawson 1965; Helms and Flader 1985.

examination, the selection of features and events is guided by the concept of rhetorical situation and its connection to invention.

4.1. BENNETT'S ROLE

Looking first at the background for the role Bennett was to play, we should remember first that Bennett's role is widely looked upon as a "cause" of as great magnitude as the Dust Bowl and the Depression. In looking at his impact, the concept of multiple nested rhetorical situations comes once again into play, as Bennett responded to a context of many layers and dimensions, and in turn helped shape the context as perceived by his audiences. Bennett's impact in shaping the perceived context falls mainly into two areas. First, he contributed in a number of ways to the perception of erosion as a crisis or exigency. In doing this, he moved the various audiences from a state of passivity to activism on this issue, that is, he helped create rhetorical from non-rhetorical audiences. Bennett also showed the way to the necessary action. What impelled Bennett's commitment to this issue, when, throughout two-hundred years of erosion, no one else had made such a commitment? And what made possible his success, after twenty years of effort, when he himself could not succeed before, and where so many others who were interested had failed? For an answer to these questions, we can look at the multiple interlinking rhetorical situations within which Bennett's rhetorical work succeeded.

What sort of rhetorical situation did Bennett himself perceive? Bennett's biographers lay considerable stress on his work for the soil survey as developing his commitment to soil conservation, and on the background of his personal and family experience as contributing in turn to what he saw and how he interpreted it. Many other advocates—soil scientists, business people, journalists, legislators, and more—contributed to the eventual adoption of soil conservation. But among these Bennett is accounted to be the foremost advocate. Bennett has been said to have been "chosen by destiny" to fulfill the role that he played (Brink 1951: 7, 15; Martin 1959). Bennett's career recalls Bitzer's emphasis that the rhetor may feel compelled by the circumstances to act as speaker or leader; Simms, in

The Soil Conservation Service, echoes this idea in his wording about Bennett's early experience as a speaker: "By 1918 he had begun to write and speak extensively on the subject. At first, he disliked speaking and dreaded the ordeal of appearing before audiences. However, the urgency of his cause overcame his reticence" (1990: 8). Our first consideration is what made a rhetor and a national leader of an Anson County farm boy.

4.1.1. The First Phase: Bennett Identifies Sheet Erosion

In order to map the soils of the US, soil surveys had to be done in every county. It was into this undertaking that Bennett stepped when he began working for the Bureau of Soils in Field Operations in 1903 with a degree in chemistry and geology from the University of North Carolina. Bennett later reported that he did not at first appreciate the meaning of the signs he saw on the land. "The damaging effects of soil erosion were in evidence to right and left through the rolling farm country encountered in North Carolina, Tennessee, and Virginia during my first two years of surveying, [but] it was not until 1905 that I began to understand just what was taking place on the land" (Bennett 1959: 12). That understanding came during a survey in Louisa County, Virginia.

In the spring of 1905, Bennett was sent to survey Louisa Co, VA, with W.E. (Bill) McClendon. This part of middle Virginia "enjoyed a reputation" for having some of the worst soils in the country, despite there having been "an abundance of virgin soil admirably adapted to tobacco" in the 1700s when it was organized as a county (1905: 193). The Bureau wanted to find out why. Bennett frequently told the story of what happened on that survey as a sort of "epiphany," an awakening or a revelation about the processes of erosion:

Bill McClendon of Bishopville, SC, and I were stirring through the woods down there in middle Virginia when we noticed two pieces of land, side by side but sharply different in their soil quality. The slope of both areas was the same. The underlying rock was the same. There was indisputable evidence that the two pieces had been identical in soil makeup. But the soil of one piece was mellow, loamy, and moist

enough even in dry weather to dig into with our bare hands. We noticed this area was wooded, well covered with forest litter, and had never been cultivated. The other area, right beside it, was clay, hard and almost like rock in dry weather. It had been cropped a long time. We figured both areas had been the same originally and that the clay of the cultivated area could have reached the surface only through the process of rainwash—that is, the gradual removal, with every heavy rain, of a thin sheet of topsoil. It was just so much muddy water running off the land after rains. And, by contrast we noticed the almost perfect protection nature provided against erosion with her dense cover of forest. (Simms 1970: 7-8)

Bennett and McClendon had recognized “sheet erosion” by its effects—a thinning or total removal of topsoil. Sheet erosion is the most ubiquitous form of erosion, but up until then it had been overlooked because when the contour of the field is fairly uniform there is none of the tracery of rills or gullies. Rills and gullies are positive evidence, what Aristotle called “infallible signs” (1357b) of erosion, showing the scouring work of the water. But in seeing sheet erosion, the surveyors had recognized the process by what was not there or by what they could not see. They had to posit a version, probable, of “what happened” or “what was happening” from the clues on the landscape. Here narrative reason, involving the consideration of the part played by human action and leading to an explanation of the effects felt by them, led to the answer (McGee and Nelson 1985). How had the fertility been taken away? Their answer: sheet erosion.

By virtue of participating in an enterprise—the soil survey—that required them to report on the soils of Louisa County, Bennett and McClendon were required to find the means to express what they saw, and their jobs and scientific reputations depended on it. Their report, 21 pages, on the soil of the county included comments on some evidences of erosion. Examples of their findings on the effects of erosion on three soil types are excerpted here:

On Cecil clay they wrote

[it] is not an extensive [type] and is confined mainly to stream slopes. Many areas too small and irregular to appear upon the map throughout the county, representing severely eroded phases of other types known locally as "gall spots." ...Two phases were recognized, differing in the main in their origin. [The first represents] the original weathered products of the parent rock. It has a deeper red color and a higher crop value. The other phase represents the subsoil from which the original soil covering has been removed by wash.

On Cecil fine sandy loam

Through cultivation and washing the surface material on hillsides is removed so rapidly that the soil soon assumes a loamy character, and in a few years may lose almost entirely its original characteristics.

And of Meadow they wrote

All streams overflow during the year and deposit each time a layer of material derived from the various soils in their drainage basins....Considerable areas of formerly well-drained and productive bottom land have been rendered marshy and unfit for other purposes than pasturage through failure to keep the channels cleared of obstructions. (1905: 204-205)

Speaking to their professional community, Bennett and McLendon cast the influence of erosion in Louisa County in terms of description of soil characteristics. Meadow, affected by deposits of upstream soil types, was a type made of "frequently interstratified" dark sandy loam, silty loam, and sand. This "interstratification" is the record of the deposit of silt-engorged overflows, runoff from cultivated fields at a higher elevation. That the present condition of poorly drained meadow contrasts with a past condition of "formerly well-drained and productive bottom land" is a matter of local record, and part of the background research that went into the surveys. The story of repeated inundation and erosion, they are saying, is revealed by the cut. One of the Cecil clays was classed as a separate "phase" because it had lost its topsoil. They classed it by what was no longer there. Again and again throughout the report, Bennett and McClendon comment on changes in the nature of the topsoil.

Bennett and McLendon also refer to the soils of the county as being deficient in humus, and they nominate an integrated management strategy to improve conditions on the land: growing cowpeas, subsoiling, and deeper plowing, and using barnyard manure, green legume manures, lengthened rotations of grasses and legumes, increased stock raising and improvement of grasslands to incorporate organic matter and improve the soil generally. A few final observations are worth mentioning here. Bennett and McLendon describe the county as generally poor, and that the economic conditions are connected with the few kinds of crops grown. They write, "a certain acreage is put into corn from year to year, with little regard sometimes to the suitability of the soil," which then yields them very little. They also mention a community problem connected with the soils, namely that because the clay subsoil roads become almost impassable in winter, "vehicles of a broader tread have been advocated, but are not likely to come into general use without legislative action." Bennett and McLendon are here making some observations on human nature—that much is liable to be done according to convenience or convention unless society asserts the right to having a say in the county's affairs. Society will assert its priority in the disposition of resources if and when it perceives its ongoing interest in the resource or improvement being mistreated. Part of Bennett's task was to contribute to this perception and move government at any level towards the assumption of responsibility. He made it clear in this and later surveys that the local residents cannot assume it.

In this report Bennett and McLendon nominated "surface washing" as the cause of the problem they saw in Louisa County. Swain, in his account of the beginnings of soil conservation, writes: "As a native of the Piedmont area of the Southeast, Bennett had grown up with the problem of soil erosion, and like most of his neighbors he had remained unconcerned about it. His casual lack of interest disappeared, however, when he worked at surveying the soil" (Swain 1963: 146). Once having recognized that the eight or nine inches of topsoil had "vanished" gradually and unnoticed from the cultivated field

while the topsoil remained on the adjacent wooded area, Bennett and McClendon realized that they had seen this phenomenon taking place widely throughout the region. They detailed in the Louisa survey the causes to which they attributed the erosion. Noted there in writing are most of the fundamental elements that made their native South so susceptible to erosion, especially sheet erosion, both on physical and cultural grounds. The elements are absent or of lesser degree in the North. The amount of organic matter, the prevalence of clay soils and subsoils, and prevailing crops with their associated cultural practices combined to make southern soils highly erodible. These factors and their effects are closely interwoven.

To begin with the crops, since the time of the first settlements the traditional crops of the South had been clean-cultivated crops, or "row crops": tobacco, cotton, and corn. Space between rows and hoeing and/or "middle-busting" throughout the growing season to keep down weeds left the soil surface bare, and lack of soil cover is the first invitation to erosion. Bare soil is not always such a problem on newly cleared land when it is fertile and has good structure, but use over time depletes the base nutrients and the organic matter and breaks down the structure. Microbiological organisms must have a near-neutral pH and adequate organic matter in soils to thrive. They work the organic matter and break it down into available forms of nitrogen, ammonium, and nitrate, making bound organic nitrogen available for good crops in the short-term and releasing the carbon into the soil atmosphere. Other base minerals held on organic matter, calcium and magnesium especially, are also made available in this process. Cultivation breaks up soil structure and compacts it below the plow depth. At the same time it keeps the plowed soil aerated, which facilitates the breakdown of organic matter even more. Quantities of nitrogen are commonly lost just to cultivation, and the topsoil, of clay soils especially, gradually becomes depleted of nutrients and structure. Only the first few years, decades at most, of cropping a clay soil without amendments could be expected to give adequate reward to the labor of planting it.

As the organic matter becomes exhausted clay textured soils typically become more dense (“heavy”) and plastic. When organic matter is reduced, clay (the soil textural type that predominates in that region) takes on an even denser texture and is slow to absorb water and slow to release it. The loss of organic matter also means the loss of a substance which, more than any other, is able to improve the texture and water retention, and many other qualities, of soils; this was known early in the century (Hall and Russell 1911). The United States has much harder rainfall than most of Europe, so much softer that European soil scientists, when visiting the US, “can’t get over” the intensity of the rainfall (Buol 1996). In areas with hard rainfall, drops fall with great energy on soil surfaces unprotected by vegetative cover, they mechanically dislodge particles and move them downhill. This can occur on virtually any soil if the rain is here, and if the structure is disaggregated through abusive cultivation practices, the tendency is greater. Where slopes are uniform, and cultivation every year smoothes over any signs of erosion, this process continues unnoticed if erosion is thought of only in association with physical signs left behind—rills and gullies—and if no notice is paid to the colors of the water running off the fields or to the gradual thinning of the soil.

Why did Bennett and McClendon notice this kind of erosion? In his retellings of the event, Bennett makes it clear that the immediate situation, the juxtaposition of a good soil with forest cover and a washed soil of the same subsoil, provided them the crucial clues to this discovery. Soil versus no soil. They could see that the washed soil had no topsoil when the contrasting soil was friable and soft, with organic matter, among other things. The topsoil, with no “visible signs” of erosion, had simply gone over the years of cultivation. This experience was a turning point for Bennett, one that he referred to up into the last decade of his life. This strong impression he seemed always to have attributed to the shock of recognizing the true nature of a process he had lived with all of his life. At this juncture he and McClendon, both Southerners, had made sense of why southern soils had gotten so poor, *when there was no evidence* of erosion—no rills or gullies. In this case they had to report something that had not before been discussed in

their Bureau, so that they were drawn into rhetorical invention; by means of invention they arrived at their discovery.⁴³ They conceptualized what they saw in a certain way—as erosion that had happened without leaving the usually recognized scars, but had taken away all the topsoil. Having originated this idea, something previously unknown, they then began to see it all around them; as Bennett later said, “it was to the left and right of us” all over Tennessee, Virginia, and the Carolinas where they had been surveying (1959: 12).

Bennett and McClendon’s claim depended on intertwining issues of stasis. Fahnestock and Secor argue the importance of the stasis questions for invention in science, because definition, especially, can be so critical (1985). Here the soil surveyors needed to determine if there was a change of state, what it was, and of what kind or quality, with the eventual goal being to discover what judgment could be made on managing the soil, that is, what could be done about it. They were certain that the condition of the second, eroded, soil was poor; this was their position on “*qualis est*.” Likewise their observations that the soil hadn’t a porous texture and did not retain water were able to contribute to this judgment of value within the framework of the Bureau of Soils’ own criteria, as its Chief, Milton Whitney “was...convinced that soil texture was a prime key to soil productivity” (Simonson 1986: 4). But what was it that “happened” to bring about this change of state? “That it is”—that there is a change of state (first stasis), the end result of which they were seeing, was not in dispute. Indeed, the Bureau had sent them to find out why these soils “had a reputation” for being poor. However, Brink reports, “[erosion] was not the answer Washington wanted...The depredations of erosion were not the concern of officialdom at that time, but rather the theories of soil productivity which had to do with ...certain

⁴³ The soil erosion/conservation situation suggests itself as a case that the “strong form” perspective can be tested against. Advocates of the strong form of the argument for rhetoric in science, Gross, for instance, and Vatz, who argues that the rhetor creates the situation, would not be able to account for loss of soil productivity as “created” by the rhetor. What the rhetors have done is to discover a fact that was not apparent, through rhetorical invention, the facility for finding the truth in specific situations of uncertainty. The “fact” then undergoes the test of intersubjectivity in the setting of the scientific community; whether it is accepted by others depends in part on social and cultural factors. Bennett and McClendon were sure of their facts, but had difficulty convincing their superiors of the significance of them.

complex and harmful organic 'toxins'..." (57). The outcome of the experience for Bennett and McClendon was that they saw "what happened" in Louisa County as the gradual removal of the soil from the surface of a field "in a thin layer or sheet at a time, as so much muddy water running off the land after rains" and that the only positive evidence of this is "the red, yellow, and black colors of floodwater running away to the sea" (Brink 1951: 56; 156) and a different and impoverished soil substance remaining behind. A further result was that "some of their findings ...included in the manuscript failed to survive editing in Washington" (Lectures 1959: 13). Although "Washington" acknowledged the qualitative (poor) state of the soils and by its own judgment regarded texture as the main criterion of quality in a soil, they rejected Bennett's and McClendon's finding, which rested largely on texture, and in so doing revealed (yet again) the inconsistency of the official stance.

Bennett often reported this experience as the single event that most advanced his understanding of how there was erosion where he could not see it before, although he knew or thought that the soil was somehow impaired. Such a path to a "new discovery" and the place in it of the stases was noted by Kuhn, although, again, he does not use rhetorical terms.

Clearly we need a new vocabulary and concepts for analyzing events like the discovery of oxygen. Though undoubtedly correct, the sentence, "Oxygen was discovered," misleads by suggesting that discovering something is a single simple act assimilable to our usual (and also questionable) concept of seeing. That is why we so readily assume that discovering, like seeing or touching, should be unequivocally attributable to an individual and to a moment in time. But the latter attribution is always impossible, and the former often is as well.... [A]ny attempt to date the discovery [of oxygen] must inevitably be arbitrary because discovering a new sort of phenomenon is necessarily a complex event, one which involves recognizing both *that something is* and *what it is*....[D]iscovery is a process and must take time. Only when all the relevant conceptual categories are prepared in

advance, in which case the phenomenon would not be of a new sort, can discovering that and discovering what occur effortlessly, together, and in an instant (1970: 55-56; emphasis mine).

Kuhn's work is widely regarded as one that "opened Pandora's box" (Toulmin [1990] spoke of it as a "Trojan Horse") on the issue of the objectivity of scientific practice. His observation here is that "all the relevant conceptual categories [must be] prepared in advance" for discovery to take place. This is one of the statements that opened the door between the special arts of science and the art of rhetoric, traditionally regarded as separate, due to a distinction made by Aristotle. The critical discovery of sheet erosion as the unrecognized cause of so much soil degradation throughout the South was made because of the personal experience of the people who made it—knowledge tied up with their senses and emotions and values. Their personal experience in this discovery gave them the conviction that there was something important and dangerous in what they saw, and they wanted to express it aptly and give it due emphasis so that a change of action could be taken for the future; that is clearly the rhetorical function.

A further reason that Bennett and McLendon were able to make this "discovery" of something that had surrounded and affected them all their lives, I believe, is that their experience or tacit knowledge contributed materially to their formulation of the problem. The key to why Bennett and McLendon diagnosed the soil sickness when no one else had may be found in their training as soil scientists combined with a background on this type of soil, as in Bennett's early experience on a farm in eastern North Carolina. In calling him a "man of the soil" Bennett's biographers allude to his having been raised working on his family's small plantation on the red clay soils of Anson County in eastern North Carolina. Bennett's experience of land there would have embraced many kinds of knowing. The first of these ways would be a qualitative one, attached to emotions he would have felt for his home.

The prosperity of the Bennett family had always depended on its land, the plantation community, and the wider community. All of these had seen better times. A family detail noted by Bennett's biographer, Wellington Brink, illustrates this connection:

Bennett's great-great-grandfather, Captain Patrick Boggan of Revolutionary fame and a wise and foresighted man, endowed those of his seven daughters who married farmers—as most of them did—with a [water-powered] gristmill as a wedding present, claiming that every plantation should have such an adjunct as a matter of sound economy. (1951: 38)

By the time of Bennett's adulthood, Brink goes on to say, only ruins remained of those and the other of the 38 gristmills that had operated *in his home county alone*. The swimming and fishing holes he had known as a boy were silted in, and fields he himself had cleared were badly eroded. One of his brothers had sold out and pulled up stakes, not once but twice, to try his fortunes in the new lands to the West. Both times he returned within two years, having to start again. Bennett's family had held considerable acreage in the Carolinas, yet Bennett had to earn his college tuition by working on the farm and elsewhere. He worked as a chemistry laboratory assistant at the University of North Carolina, dropped out to work for two years to earn the rest of the tuition, and graduated somewhat belatedly in 1903. Bennett had personal experience of the community and economic effects of declining soil fertility, namely, that failure to keep the soil from "washing" was failure of responsibility toward future generations and to those downstream.

Another way that Bennett knew the land is what Polanyi characterized as "tacit knowledge" (Miller 1979). He knew the look and feel and smell of it, from old fields and new, wooded land and pasture, its texture when worked at different moistures and temperatures, and what it would grow. A cluster of terms that express this "tacit knowledge" would include *understanding* and *experience*. Bennett might further have a set of *presumptions* about how the soils he had worked would respond under various circumstances. He would have some stories (narratives) in which these experiences were

epitomized, and these would be the basis, when he was traveling as a surveyor, of a *resonant* response to the experience of others struggling on similar soils or to the observation of similar soils in various states of care. That he had such an organic experience of the soil of his neighborhood is evident from his biography, but that he built upon it with subsequent study and experience so as to advance empirical and theoretical knowledge, is indicated by the scientific boundaries later crossed through his research initiatives.

4.1.2. Shaler and the 1908 Conference of Governors: Confirmation of a Commitment

Coming near the beginning of his career, the Conference of Governors held by the first Roosevelt administration had a profound effect on Bennett, as he would often recall. Roosevelt was by far the most conservation-minded president for his time, and his record was not equaled until the second Roosevelt Presidency. In his autobiography, Roosevelt held his efforts on behalf of conservation to be the most important contribution of his administration; "history," writes Frank Ellis Smith, in *the Politics of Conservation*, "supports his judgment" (1963:91).

4.1.2.1. The Prospect of Progress

The years 1908-1909 were crucial for Bennett, prominent reasons for this being the Governors' Conference of 1908 and his reading of the work of Nathaniel Shaler, who was Professor of Geology and Dean of the Lawrence Scientific School at Harvard. Possibly while at the University of North Carolina, Bennett was introduced to Shaler's work by his geology professor, Collier Cobb, who had studied under Shaler at Harvard (Helms 1996). At some fairly early point, he had read Shaler's *The Origin and Nature of Soils*; it will be discussed here with the Conference papers because Bennett often referred to these sources "in one breath" (e.g., 1959: 12-13). Shaler's work clearly was a source of inspiration for Bennett, judging by Bennett's continuing references to it, the topics Shaler brought out that Bennett returned to, and by Bennett's priorities in acquiring more exact data in the

areas that Shaler touched upon. As we saw in Chapter 3, Shaler thought of soil science as a very easy and straightforward science, a good nursery for a would-be geologist. Because soil microbiology was still a new area, and because of his prior geological perspective, he would not have known of the bafflingly complex links among physical, chemical, and biological soil processes. In his report he does not discuss the complex processes of erosion—he does not define erosion—but addresses three of the stases pertinent to erosion: the fact *that it exists*, that *it is serious*, and that something *must be done* about it. In calling attention to the estate of the physical body of the soil, geologist Shaler expresses a concern shared even by very few soil scientists; most continued in their more established disciplinary streams, to focus on the chemical aspects of soil fertility so much so that they did not even see the physical changes.

Although he wrote too early to be able to give an accurate picture of the importance of soil microbial life (and is therefore inaccurate on some points), Shaler wrote in the context of the logging fires, deforestation, and severe flooding that resulted in the creation of the Division of Forestry in 1881 and in 1891 the Forest Reserve Act, a mandate to acquire and manage fragile lands for timber and for flood abatement (Heisley 1933; Runte 1991).⁴⁴ Consequently Shaler placed strong emphasis on topics that were critical to recognizing the need to act on erosion. The length of time needed to form soils, the damage done by erosion, the connection between careful tillage and high populations (meaning good markets), the fact that it was preventable, and, particularly, an estimate of the extent of area eroded—all were significant topics emphasized in this work.

Shaler was far ahead of his time in recognizing that tillage as then practiced was not necessary and not in the order of nature, mainly because it results in the formation of a plow pan. “First of all, every husbandman should clearly understand that the process which he follows in obtaining crops from the soil is essentially unnatural” (1891: 334). He laments the practice of shallow plowing, and plowing at the same depth every year, so

that a hardpan develops. He therefore recommends subsoiling, which does not overturn the soil, but loosens it at a lower level than the plow; this statement advocates a treatment approximating "no-till," which is close to natural processes in not overturning soil and in building up organic matter in the A horizon. But then he goes on to recommend spading. He says that spade tillage, were it not so much more expensive than plowing, would be much preferable: "One of the greatest desiderata in connection with our farming is an instrument *which will overturn the earth* in the manner of a spade—that is, without compacting the lower portions of the deposit in order to overturn the upper parts" (1891: 335). Shaler's comment represents the presumption, a culturally-based one, that the soil must be overturned; tellingly, he does not advance any reasons why the soil should be overturned. He is only arguing that spading does not result in a hardpan that plows create as the heel "smears" across the lower portion of the soil, sealing it over and compacting it with the pressure of the plow.

Shaler also placed great emphasis on the extent of erosional damage in farming communities. Shaler calculated, "according to the estimate I have made with some care," that the total area of formerly cultivated fields abandoned in the states of Virginia, Kentucky and Tennessee was between five and six thousand square miles, or one-thirteenth of the total land area of those states (1891: 333). Speaking not of total land area but of formerly cleared, productive farm land, Shaler further estimated that at least five percent of all of the soils in the United States that "have in their time proved fertile under tillage are now unfit to produce anything more valuable than scanty pasturage" (1891: 333). Shaler noted that impoverishment of land was "greater in the South and in the new States of the Mississippi Valley than in the eastern portions of the Union, because careful tillage has long been made possible in the last-named section by the high price of farm products." He identified as particularly badly affected states South of the glacial belt—the above-mentioned states, and with them, Mississippi, South Carolina, and North Carolina, Bennett's home state:

⁴⁴Also USDA *Yearbook* for 1915, pp 54-55.

There is scarcely a county in these States where it is not possible to find a number of areas aggregating from 300 to 500 acres where the true soil has been allowed to wash away, leaving exposed to the air either bare rock or infertile subsoil. Where subsoil as well as the truly fertile layer has been swept away, the field may be regarded as lost to the uses of man, as much so, indeed, as if it had been sunk beneath the sea, for it will in most instances require thousands of years before the surface can be restored to its original estate. (1891: 331-332)

Shaler was highly respected—had a strong scientific ethos—so his words carried weight in the natural science disciplines. Bennett must have derived encouragement, then, from Shaler's explicit and categorical statement of the necessity of taking action *as a society* and the immorality of failing to do so. Shaler amplifies this idea in the concluding statement of his essay:

In [soil] the celestial and terrestrial energies combine their work to lift the mineral elements up to the higher planes of sentient life. From it comes the sustenance of plants and animals, both of sea and land. The frame of man is the product of its forces; his form is indeed but a bit of soil uplifted for a moment to the noblest shape of life, then bidden to return to the garner of earth....When we perceive that civilization rests on the food-giving capacities of the soil, when we perceive that all the future advance of our kind depends upon the preservation and enhancement of its fertility, we are in a position to consider the duty which we owe to it. This obligation bids us nurture and care for this part of the earth with an exceeding tenderness and affection. It bids us ever remember that it is enriched with the dust of our progenitors, and is teeming with the life which is to come.... It is characteristic of our time that men seek to clear away evils by means of law. There is a general discontent with the results which have been obtained by the system of individual ownership of land and a growing disposition to qualify and limit the nature of that possession. In considering the questions as to the ways in which the earth's resources shall be administered, it is clearly necessary to bear in mind the needs of exceeding care in the preservation of the fertility of the earth.

As long as lands are in the state of forest or prairie, the admirably adjusted forces of nature will insure their preservation. When they become tilled, it is imperative that they be peculiarly well-guarded; any legislation concerning the tenure of land should be devised in view of the fact that we need to have not less but more personal interest and sense of responsibility in the management of these problems. It is not proper here to consider the probable effect of the various proposed modifications of the land laws. It seems, however, fit that any such changes as may be made should be planned with a clear understanding of the very serious nature of the needs. When in the future a proper sense of the relations of the soil to the necessities of man have been attained and diffused we may be sure that our successors will look back upon our present administration of this great trust with amazement and disgust; they will see that a state of society in which men took no care of the rights which the generations to come have in the earth lacks one of the most essential elements of a true civilization. (1891: 345)

That Shaler was willing to bring his professional stature to the support of this cause and present his arguments so eloquently and with such commitment must have been a matter of inspiration for Bennett. Shaler's influence is suggested by Bennett's single-mindedness in pursuing data on the extent of erosion, on his own as a surveyor, then supervising other surveyors, and later administering the 1928 allocation. It is further suggested by Bennett's continuing use of some language and images that Shaler also used. Two of these have to do with sheet erosion. Shaler refers (1891: 333) to "a vastly more important though less visible damage [that] has been done by the partial destruction of the nutritive layer, in the course of which it has been thinned and worn to a point where it will no longer pay the cost of tillage." Bennett would refer in his work to sheet erosion as "invisible," "stealthy," and "insidious," and speak of its action as all the more dangerous because unperceived (1927). He would also, almost invariably connect sheet erosion's characteristic, gradual thinning without gullyying, to colors of red, yellow and brown in the runoff, a symptom to which Shaler also forcefully called attention (1891: 330).

A second, and probably greater, influence on Bennett at this juncture was President Roosevelt's conference on natural resources held at the White House in 1908, and particularly Thomas Crowder Chamberlain's presentation. The proceedings of the Governor's Conference became available shortly after the conference, with addresses including those of the President and Secretary of Agriculture Wilson; topics included water resources and transport, geology, mining, and forestry. Participants seem to have been unanimous in pointing to the fact that the era of unlimited expansion was at an end. James J. Hill, founder of the Great Northern railroad, preceded Chamberlain, giving a speech entitled "The Natural Wealth of the Land and its Conservation." He began by expressing appreciation that the conference was being held, and then, in a reference indicating the influence of "the late Professor Shaler," quoted him as saying, "Of all the sinful wasters of man's inheritance on earth, and all are in this regard sinners, the very worst are the people of America." In a presentation dealing chiefly with the importance of protecting coal and iron reserves, Hill detoured into a brief accounting of forest cutting, the rate of logging, and the probable remaining supplies. He forecast that the remaining supplies of some varieties of trees would be gone within 10 to 12 years, and asked "When will we take up in a practical and intelligent way the restoration of our forests?... Not only the economic but the political future is involved. No people ever felt the want of work or the pinch of poverty for a long time without reaching out violent hands against their political institutions...." Hill, like many others at the conference, explicitly connected the supply of natural resources with the health of the society and the stability of government.

In "Soil Wastage," T.C. Chamberlain, Head of the Department of Geology at the University of Chicago, gave an address that is referred to often in Bennett's writings. To Chamberlain, Bennett appears to owe his use of the term "menace" applied to erosion. More importantly, Chamberlain confirmed for Bennett that he was right to be concerned about the soil body (as opposed to mineral nutrients exclusively) and the essential connection between soils and the survival of a people:

...surface wastage is a serious menace to the retention of our soils under present modes of management. Historical evidence enforces this danger. In the Orient there are large tracts almost absolutely bare of soil, on which stand ruins implying former flourishing populations. Other long-tilled lands bear similar testimony. It must be noted that more than loss of fertility is here menaced. It is the loss of the soil-body itself, a loss almost beyond repair. When our soils are gone, we too must go, unless we shall find some way to feed on raw rock or its equivalent.

With a geologist's estimate of 10,000 years to form one foot of soil, Chamberlain further confirmed Bennett's assessment of how great an exigency soil erosion was.

From Bennett's later statements (e.g., 1959), he appears to have felt that he owed to this speech of Chamberlain's a greater sense of dedication to his work of bringing erosion to public attention, because at that venue, before the Governors and the President, Chamberlain gave legitimacy to the issue by giving erosion a serious treatment. Chamberlain placed soil erosion in the company of other issues regarded as worthy of deliberative attention leading toward collective action of some kind, whether it would be action on the county or state level or whether it would be policy and expenditure by the federal government. Chamberlain explained that the science of geology had made it apparent that, contrary to humanity's earlier beliefs, the earth had experienced roughly the same climatic conditions for "a vast period" and that "a vast future of habitability may fairly be predicted." "Whether you are prepared to accept so large a view of the habitable future or not, I trust you will strike hands with me in the conviction that the probabilities of the future are at least so great as to render imperative the serious consideration of our obligations toward it." Again, the explicit link was forged between present use of resources and obligation towards Americans of the future.

While follow-through from this conference was insignificant, statements made at the conference itself had solidly established two important topoi for a soil conservationist. These points were, first, that the soil physical body is important in itself. As we saw

earlier, the Bureau of Soils did not officially adopt that position until 1927 (1920 unofficially). The second critical point established by the very mention of erosion at this venue was that society has an interest in protecting the soil, in the present to prevent flooding and damage to water resources and to soils, and in the future for maintaining political and social stability. Hill and Chamberlain echoed Marsh in calling attention to civilizations that had collapsed owing to their improvident exploitation of natural resources. The soil, in this perspective, is not simply the concern of the farmer who currently resides there. Indeed, often the resident is entirely unable to take responsible care of the land, but this connection between tenancy and erosion had not yet been recognized. The Depression brought out this connection two decades later; at that time tenancy became more widespread, rather than predominantly in the South, where it was only to be expected.

4.1.2.2. The Outcome: Indifference in Congress and in the Bureau of Soils

The 1908 Governor's Conference came at the end of four years of activism to secure and protect the nation's natural resources for future generations, and this had generated considerable conflict with a Congress traditionally supportive of logging, mining, railroad, and other development interests, lagging in its perception of the fact that the end of availability of some resources had been reached or was in clear view. Frank Ellis Smith, in *The Politics of Conservation*, wrote of the aftermath of the Governor's Conference that "Congress wanted no more advice on conservation from T.R." (1966: 109). The recalcitrant Congress, balking at Roosevelt's continuing efforts to prod them into action, refused to support any more of Roosevelt's conservation meetings or committees. One of the most pressing matters to Roosevelt at this time had been the issue of flooding, especially on the Mississippi. Following the reasoning of geologist/hydrologist W.J. McGee, Roosevelt connected steadily worsening flooding and navigation problems on the Mississippi to erosion from farming and clearing in the watersheds (Smith 1966: 89). Roosevelt had pressed for action on waterways planning and legislation, and advised by McGee and in coordination with Nevada representative

Francis Newlands, he established the Inland Waterways Commission for the purpose of planning a coordinated use and development of the river and tributary systems. The Commission proposed to integrate reforestation and soil conservation in tributary watersheds to reduce the speed of runoff and the river's silt load; future planning for inland waterways would improve navigation and take account of the opportunity to provide power and establish upstream reservoirs for irrigation and flood control; this was the approach Newlands and Roosevelt proposed in preference to the "levees only" solution that the Army Corps of Engineers had been carrying out since 1861 (Harrison 1961; Long 1909; Smith 1966).

But the Congressional leadership refused to enact into law the recommendations of the Commission. Congress even failed to provide for the publication of the Commission's report to Congress. For the next 20 years Newlands and the remaining Roosevelt conservationists tried, within the context of ever-shifting Congressional coalitions, to pass legislation to conserve the soil and water of the upper Mississippi River Valley and improve the security of the residents of the lower valley. But Congress throughout this period was not yet willing to accept the two central principles that distinguished the coordinated from the levees-only approach: that the floods, at least to the degree that they had been rising, were an externality, and that people were entitled to protection from damages created by externalities.

The flooding of the Mississippi River in the early years of the century largely represented a "third party effect," now known as an externality. This flooding was not simply a hazard of living on a flood plain, in doing which Louisianians and others were taking their own calculated risk. Except that major "flood events" can be expected at intervals because of unusual weather, an undisturbed watershed has a normal range of runoff and drainage. Records, gradually more quantitative, had been kept of the levels of the Mississippi since its settlement in the early 1700s, when "spring rises" could be controlled with low dirt embankments. But in the mid- to late 1800s, the levees were having to be built higher and

higher and in more and more locations (Bailey and Forsling 1934; Harrison 1961; Smith 1966). Higher crests were an unintended result of upstream economic activity, especially clearing and cultivation, which caused the water to pass off of the land too quickly and the river to carry a heavier load of silt, increasing its volume. Flooding was therefore what Bitzer distinguished as a rhetorical exigency, not entirely an unavoidable disaster. The second principle was that, since this was a third-party effect, the victims should be protected from it, and by the society as a whole, expressing their responsibility through their government. A further point was that where soil is carried off and becomes an offsite externality in one place, it and its fertility value are lost from its point of origin; thus the more complete solution would be to prevent its eroding in the first place. The Corps of Engineers' unwillingness to take this approach perpetuated the loss of rich loess soil on the bluffs of the Mississippi at Missouri and on its tributaries in Illinois, Iowa, and Indiana—the richest soil in the world. At the same time their “money-saving” approach perpetuated flood-damages downstream. Prevention, leading to a better *futurible*, would be the best solution to this problem. Because society rather than an individual or business was perpetrating this externality, society should shoulder the burden of preventing it, and of damages when it occurs.

Congress was not prepared to endorse these principles and the values that would be promoted by Newlands' plan. They preferred the cheaper expedient of raising levees, which at the time were being underwritten by local river authorities. But the Congressional solution of raising levees ever higher and further upstream actually caused the problem to worsen downstream (Harrison 1961; Cassidy and Althaus 1994). The effect of this approach was a constriction of floodwaters, which raised the level of damages that would eventuate when the levees did break, as they would, disastrously, in 1922 and 1927. In the 1927 flood, 250 people were drowned and at least 700,000 had to leave their homes; estimates of property damage ranged from \$364 million to a half of a billion (Smith 1966: 172). On the other hand, the conservationist proposals to finance a multidimensional approach to the flooding would reduce the externalities, and keep the

soil and water as positive values where they were. Two critical aspects of responsibility related to natural resource use had now been aired: responsibility to do no harm to and protect the rights of others *elsewhere* and to others *later*. These principles—that *offsite* and *intertemporal* externalities, respectively, are not the fault of the victims and that either government should prevent them or that the responsibility for them should be borne by the perpetrators—had not been established before this period. A further principle of investment for achievement of positive benefits as well as avoidance of negative costs had also been broached. Now these principles had been recognized for soil, water, timber, and minerals. The concept of externalities is a clear instance of economic theory that resonates with rhetorical import; we can restate the essential content of this concept as seen in this application (flooding damage/relief and prevention) in the terms of our study: Intentional choice and action in the present will prevent probable negative consequences, and at the same time permit the realization of desirable future conditions.

Unfortunately, Congress was not ready to adopt this level of responsibility. Nor was the Bureau of Soils or the soil science community ready to adopt Bennett's view that soil erosion was a serious problem and should be dealt with actively, to prevent perpetrating more loss of opportunity in future generations. How Bennett fared at the Bureau is a measure of the esteem in which he was held. In 1909, he was made Inspector of Soil Surveys for the Southern Division, in which position he was to supervise fieldwork in 18 states. While this was a promotion, it was not much for someone with Bennett's energy, initiative, and commitment (Swain 1963: 147). In the same year, as a measure of the Bureau's view of the perishability of soils, whether by erosion, by chemical or physical degradation, or by these factors in combination, the Bureau of Soils published its Bulletin 55. In this Bulletin, Whitney argued that soil was of inexhaustible and permanent fertility: "The land is the one permanent and immutable asset of the country...." While Whitney no longer censored discussions of erosion out of Bennett's reports, he did, apparently, intend to "cool" Bennett down, by sending him on surveys and projects in Alaska and Cuba, and in South and Central America (Brink 1951: 13; Swain 1963: 147; Simms 1970: 23).

4.1.2.3. Reasons for the Lack of Concern about Erosion in the Bureau of Soils

Earlier the point was made that the personal experience of farming and living with clay soils in the South made a difference in the perspectives of Bennett and McLendon, as a basis upon which they were able to make the discovery of sheet erosion. The negative form of this hypothesis is well supported by a rough survey of the backgrounds of working agriculturalists and soil scientists of their day. None of these men were from land where old leached soils (such as those of the Coastal Plain and southern Piedmont) predominated, and none were interested in the "menace" of erosion. The Secretary of Agriculture, James Wilson, was from Scotland but was raised in Tama County, Iowa; he was known for a boundless optimism about the agricultural potential of the nation. C.G. Hopkins was from Illinois; F.H. King, the soil physicist, was from Wisconsin. Milton Whitney was from Baltimore, and as the son of a lawyer who kept a farm outside the city, he had limited experience working with soil. Whitney's contribution was in visualizing and running the soil survey, but, with little farm experience and less scientific education than many of his subordinates (Miller 1949), Whitney seemed unable to contend with the complexity of soil fertility and degradation issues of the day. Curtis Marbut, Bennett's contemporary and his lifelong friend, who would be in charge of the Soil Survey from 1913 until his death in 1935, was raised in Missouri. On the loess deposits on the Mississippi River, very high rates of erosion were measured in southern Missouri after erosion research began, but there as in many areas in the Midwest, the soils were so deep that erosion did not affect their productivity as much as it did shallow soils over clay subsoils. Comments by Marbut, a Missourian, suggest that he thought erosion not, "in the aggregate," very important (Johnston: 1935: 32). That sheet erosion in Marbut's home territory was easy to overlook is suggested by Swain's comment: "The revelations made by the erosion stations [in 1929-32] lent a convincing aura of scientific validity to the warnings of Bennett and others. Many unsuspecting farmers, learning that sheet erosion was removing their topsoil at an alarming rate, and all unnoticed, began to think in terms of stopping it. The actual measured losses of soil in the Shelby Loam region must have shocked Iowa and Missouri farmers" (1963: 154). In later years, Charles Kellogg and

Secretary Henry Wallace, while supporting the conservation program to some extent, were also often equally concerned to limit the scope and powers of the Soil Conservation Service; Kellogg and Wallace were from Michigan and Iowa respectively. All of these as well as many more in the field of soil science diminished or played down the importance of erosion. Only Hilgard of all the prominent early soils scientists had put significant emphasis on erosion. Hilgard, whose reputation has risen and remains higher than that of anyone else of his era, exemplifies the effect of experience in erosion prone areas; he lived and worked in Mississippi for several years during and after the Civil War, and produced a scathing statement on the erosion there (Jenny 1961). Hilgard moved to California and shifted his focus of study to problems of arid-lands agriculture—salinity and water; what he might have done about erosion had he stayed in Mississippi is a matter of speculation only.

Around 1907 there seemed to be an indication of a shift of opinion about erosion in the Bureau. In that year, a four-page section on erosion appeared in the Bureau's part of the Report of the Secretary of the *Yearbook of Agriculture*. The Bureau's contribution to the Report is usually written by Whitney, judging by the style as compared to others of his articles. The erosion piece is in a different style, much more specific, and because of this and because of its emphasis on the connection between erosion and flooding on the Mississippi, the section is very likely to have been written by W.J. McGee. McGee, as already noted, was the originator of the idea of the Inland Waterways Commission and its proposed objective of developing coordinated and multipurpose development of the Mississippi River. As his expertise and authority were indispensable to Roosevelt and Pinchot, his membership on the Commission also was essential; and so, "to make McGee eligible to be the commission secretary, he was appointed to a job in the Bureau of Soils" (Smith 1966: 106). With such power behind McGee, who was an independent authority in his own right, it would have made little difference what Whitney thought about McGee and his ideas.

While brief, this text covers all of the topics related to erosion that had so far been canvassed (e.g., in Shaler and in the "Washed Soils" Bulletin). Specifically the *Yearbook* section addresses the immense amount of sediment in solution in American rivers, the Mississippi in particular, and the fact that this material originates chiefly in farm fields, where it would have been the best part of the soil. It also discusses cover and organic matter as contributing to infiltration (and reducing runoff), and devotes a page to discussing remedies: deep plowing, "contour" cultivation (possibly the first use of that term), grass-land balks on rolling farms, and forestry on the steep slopes. The writer connects the soil to the welfare of the nation as "the immediate source of our prosperity," asserting that it is "a National duty to see that the remedy is prescribed and applied" (*Yearbook* 1907: 83). In making this assertion, McGee was explicitly connecting erosion to traditional rhetorical territory—law and social action. Smith emphasizes that McGee was the "brains" behind Roosevelt's inland waterways policy, with respect both to scientific and socio-economic aspects: "From the start, McGee linked his ideas of the use of new scientific discoveries with social purposes" (1963: 89). The linking of new scientific knowledge to social purposes made conservation possible.

Finally the author introduces the one point about soil erosion and conservation that would prove most contentious: who is responsible for it. He states that "in extending the principles and applications of American agriculture, three steps are contemplated in the approach of the Bureau—education, individual action, and collective action." The first action, education, involves only the diffusion of information "on a few leading points": the magnitude of soil loss, the "ease" with which it can be checked, and the confirmation that checking erosion saves money and possibly results in profit (as was later proven). The writer is confident that "given this information, American intelligence and enterprise may be counted on to meet the occasion" because, as the author says of individual action, "the public spirit is the strongest incentive to private conduct." Assumed here is the general perception that stopping soil loss is a matter of public spirit, but this view had not

been established, and the Bureau had done little up until this point to help create awareness of the problem let alone instill a commitment to controlling it.

McGee asks how collective action should be carried out. "The keynote to [aid as needed from the State and Federal instrumentalities] sounds in the social principle that in the long run the good of the individual is the good of the community, and in the legal principle that each so use his own as not to injure others..." (*Yearbook* 1907: 85). This passage introduces the problem of third-party effects of erosion, the aspect of the problem that most clearly invites collective action; the writer enumerates some of these effects: "the streams are fouled, water power is jeopardized, navigation is impeded or prevented, and floods are raised." He concludes, "...since in the end the responsibility rests with the men on whose farms the evil starts, the remedy must begin with the farming community and should be guided by the interests of the whole people."

In this brief statement, then, the Bureau gave discursive space, for the first time in the 1900s, to the problem of erosion. But Whitney was very likely not responsible for this new salvo. Up to this time, Whitney seems to have made only one mention of erosion, in the 1897 *Yearbook*, where it occurs in a paragraph of his "Reasons for Cultivating the Soil" (123-130). There Whitney connects it with the too shallow plowing, often with wooden shares, that was the rule at the time, and he attributes the abandonment of thousands of acres of land to it. Thereafter there is no mention of erosion, and Whitney was, by the early 1900s, time deeply embroiled in debates with the soil science community at large over the relative importance of the chemistry versus the texture of soils. McGee, the probable author of the passage in the 1907 *Yearbook*, was a well-recognized, self-taught authority of considerable stature, and must have felt that the *Yearbook* was a suitable venue to further emphasize and disseminate these—his own and the Administration's—views. Other than this bit of text, a 1911 Bulletin by McGee and one other publication by R.O.E. Davis in 1913, there is no indication of interest within the Bureau. Several Bulletins were published between 1900 and the late 1920s by the

USDA, but their authors worked in the Division of Forestry, the Bureau of Plant Industry, the Bureau of Public Roads. Several more bulletins emanated from the State Experiment Stations, but there is no further evidence of interest in erosion in the Bureau until around 1927 (Swain 1963), the year of Whitney's retirement and death, and right before Bennett's 1928 USDA Bulletin.

Other than this aberration in the *Yearbook* and the 1911 and 1913 Bulletins, and earlier Hilgard's concern with erosion in Mississippi, the soil science community was indifferent to the idea of erosion. Excepting Hilgard, and Shaler who was from Kentucky, none of the prominent scientists mentioned earlier (for whom erosion was not an important issue) were from the South or had lived in the South for an extended period of time; all were from the North, North Central or Midwestern states where soil science was dynamic and prestigious (Wilding 1994), while erosion was largely an issue in the South. The distance between the two cultures is accentuated by the reputation of Seaman Knapp. Knapp, from New York and then Iowa, and the originator of the experiment station bill, was regarded as a missionary for venturing in 1886 to Louisiana "to interest the native population in improved methods of agriculture." Knapp was appointed by his friend James Wilson, Secretary of Agriculture, 1896-1912, as a "special agent of the department for the promotion of farming in the Southern states" where he promoted diversified farming, especially to combat the boll weevil.⁴⁵ While in Louisiana, and through his writing for and about conditions in the South, Knapp did much to inform others of these conditions and to help raise the interest in and the potential for improvement of lingering post-war and depression conditions. Two factors explain, then, why the soil science community was not impressed with the seriousness of erosion. First, as we have seen, soil science was relatively quite new in the United States. There were few soil scientists from the South to give consideration to erosion. The South was rural, with lower rural and urban populations, and had twice the rate of illiteracy of the North (Trimble 69). Furthermore, it was just emerging from post-war conditions. The South had not had an adequate pool of

eligible young men to be trained as scientists. Soil scientists in general (non-southerners) may have attributed the rumors of erosion in the South partially to social and economic backwardness. The survey began to make a change in this mutual isolation by putting surveyors from both regions in rural areas throughout the country.

The second main reason that the soil science community showed little interest in erosion was its preoccupation with institutional problems, specifically the conflict between the Bureau and the experiment stations, that revolved around the authority to make policy and take action where there remained fundamental disputes over disciplinary issues. Central to the debate were the two incomplete and conflicting views of the nature of soil, one focusing on chemical loss of nutrients, that is, the chemical degradation of soil, as the main cause of infertility, and the other focusing on textural factors. As we have seen earlier, the first truly disciplinary study of soil fertility began in Germany under Leibig, and its objective was to replace nutrients lost in the removal of crops from the field. Although members of the North American soil science community raised objections to and disproved the major errors in Leibig's claims, notably the source of plant nitrogen, they addressed his claims largely on his own grounds—the presence or absence of nutritive elements in the context of inquiry into crop productivity. They stayed, therefore, within the frame of reference of Leibig's by-then classical soil science. Because of Leibig's prominence, soil scientists who offered other perspectives had been obscured and made inaccessible; such scientists, more recently rediscovered, include soil physicist Wollny, who conducted some of the first quantitative erosion research in studies of raindrop impact, and Way, whose chemical adsorption studies are now seen as forming what Hempel calls "bridge principles" between soil physical and soil chemical properties (Hempel 1965: 72, 100). Whitney is credited with helping to break Leibig's spell, and although his focus on soil texture as the controlling feature of soil fertility was likewise incomplete, his persistence and his position pushed debate so far that a new and much more adequate synthesis was reached. But even in Whitney's case, the basis of the

⁴⁵ From the *Dictionary of American Biography*, vol 10, pp. 452-453.

importance of texture was a crop productivity criterion, the soil's ability to provide nutrients in solution to plants—crop productivity or identification of soils “adapted” to given crops. All of these—Leibig, Whitney and most North American soil scientists—were still working within the set of assumptions appertaining to a fundamentally geological view of soil, one that did not take climate or biological organisms or activity into account, and their goals were agronomic. (For these reasons Marbut would say that soil science had not truly begun until the questions asked had to do with the soils themselves—in the 1920s.) Why Leibig concentrated on chemical loss of nutrients and why North American soil scientists, in general, were unable to *see* and appreciate erosion, I contend, is at least in part the lack of the concept of climate as a variable in soil formation processes and in soils under cultivation. Hilgard, by far the earliest, and for a long time, the only, American soil scientist to see climate as a soil-forming factor, was an exception also in seeing erosion as very serious and important; unfortunately, his work and influence were suppressed outright by Whitney, and indirectly by the Whitney conflict (Jenny 1961; Tandarich and Sprecher 1994). Consideration of climatic factors sheds considerable light on the basic problem of physical degradation. Also, as stated, a second reason for the lack of interest in erosion was the irrelevance to most soil scientists of the implications of the single crop, clean-cultivated farming that prevailed only in the southern agricultural economy.

What difference does climate make? Bennett and McClendon saw and were concerned about erosion because climate affects physical, chemical, and biological properties of soils. All of the basic mechanisms of erosion can occur in southern soils, and they are exacerbated by climate in several ways. Heat raises the level of activity of microorganisms, and speeds up the breakdown of organic matter. Not only is microbial activity more intense in the summer months of the year, when northern soils also support soil microbiotic life, but this activity can go on throughout mild winters. In the South, rains are more intense, generally, than in the North, so that a single intense rain can cause

severe erosion.⁴⁶ If a farmer in central Louisiana were to plant the same crop on the same type of land (with all other variables equal) as a farmer in southern Wisconsin, the Louisiana farmer could expect to lose three times the soil, due solely to the difference in rainfall erosiveness (Trimble 1985: 68).

Heat and humidity over time contribute to the formation of clay soils, and the Cecil clays are the most common soils in the southern US. In hard rains, fine soil particles are dislodged from poorly structured clay soils and this material carried in the turbid water clogs the remaining pores as it infiltrates into the soil; while the first controlled experiments on this phenomenon, "crusting," were carried out by Lowdermilk in 1920, it was familiar to surveyors, as can be seen by the mention of it in Bennett and Smith's 1910 survey of Lauderdale County (Bennett 1939: 204ff; Bennett and Smith 1910). These small dislodged particles also form surface crusts; this crusting, clogging effect, especially on bare soils with inadequate organic matter and/or poor texture, minimizes infiltration and increases runoff. Further, freezings and thawings typical of the middle South, as contrasted to hard freezes and snow cover in the North, contribute to the physical breakdown of soil structure. Glaciation in the geological past of Northern soils, except those of New England, also meant that they were better even before any clearing

Leaching is greater in the South because of its wetter conditions. Rainfall maps show the South receiving the greatest rainfall averages of the country; in Jenny (1941: 82) the southeast region is classed as "wet," and Northern seaboard states as "humid" and North Central states as "sub-humid." Rainfall decreases with distance westward. High rainfall contributes to the loss of highly soluble bivalent basic minerals by leaching, and causes their replacement by hydrogen ions; this in turn causes, not just sub-fertile conditions, but acid conditions, in which aluminum and manganese can become soluble to the point of toxicity, and phosphorous becomes unavailable (Ammons 1997; Blaxter and Robertson

⁴⁶ Recognized as a topic in erosion research as early as the 1930s (Burgess 1936: 89), and continuing as an important topic (e.g., studies in El-Swaify, Moldenhauer, and Lo 1985).

1995). Southern soils also have not had the benefit of the Wisconsin or earlier glaciations, so that the soils are old, and the effects of high rainfall, continuing for long geological eras, has leached the soils and broken their constituents into the smallest textural particles: clays. The southern landscape is also more "dissected and steep," whereas the landscape over broad regions in the North and North Central States (excepting New England, which also has steep topography) reflects the work of glaciers, which left "vast rolling (but usually not steep) plains, especially in the corn belt" (Trimble 1985: 68). A further critical factor is the parent material of the eastern seaboard. The rock underlying the New England states' glacial till is granite, and very poor in basic minerals. The parent material of almost the entire eastern seaboard is granite or gneiss, while the western sloping mountains and the states beyond are underlain by calcic parent material. The tilting of the continental plate that created the Appalachian mountains left acid bearing rock on the eastern slopes, while on the western slopes and in the central part of the country, parent rock, glacial till and loess as well as alluvium derived from all of these substances are naturally basic (Ammons 1997; Buol *et al.* 1980; Buol 1996). All of these factors contribute to the prevailing conditions in cultivated southern soils: precipitated clay, very smooth fine texture, low organic matter, and lack of structure. These conditions imposed themselves upon the imaginations and on the lives of those who lived with them. By contrast, these conditions were not apparent to the majority of soil scientists, and, indeed, were not until the 1970s when surveying for the 1976 *Soil Taxonomy* was being carried out (Guthrie 1996). In addition, preoccupation with chemical fertility—loss of mineral nutrients—was obscuring the physical aspects of the problem for those whose environment did not feature this magnifying effect of the southern climate on physical processes.

Physical and chemical soil processes and soil fertility are the domain of the soil scientist, but social and economic factors also contributed to soil problems in the South. Effects of these factors in the South by comparison to other regions, and the effects of these factors on rural prosperity in the South, if not on the soils *per se*, were noted by those who

observed closely (see, e.g., Knapp, Spillman). Spillman (1910), for example, connected lower population density with lack of markets for livestock and their products. This contrast with New York, New Jersey, and Pennsylvania, where dairying was very important from the early 1800s (Bidwell and Falconer 1941). Livestock raising was practiced relatively little in the southern states until after the Depression; little was done in the way of pasture or herd improvement. This meant less manure for fertilizing and less land kept in grass cover. Also because of relative lack of livestock, and because of a lower level of management generally, crops were not rotated into pastures, and run-out land was abandoned unseeded. Use of legumes was rare but growing. Some observers suggest that farmers, especially among southern Scots and Irish settlers, intentionally farmed wastefully, as though to show that they knew that plenty of land lay beyond the fields they currently cultivated (Trimble 1985: 71; Swain 1963: 144). In the Lauderdale County, MS, survey, however, the categories of farmers were more likely to be divided along racial lines, there being three times as many black as white farm operators. This could account for the occurrence of cowpea cultivation, because G. W. Carver was actively promoting their cultivation through the influential *Tuskegee Messenger*.

Certainly, however, the South in general had less immigration of Central and North Europeans as models of better farming (because they were land-hungry, they were renowned for the care they bestowed on making their settlements permanent). The South also had less immigration and lower population in general, with free artisans and small farmers, for example, being discouraged from immigrating and settling by market and labor structures imposed by the plantation system, in which virtually all consumables were produced, processed or built on the premises. As a result, land prices remained lower, prices of agricultural products also remained low as markets were less accessible, and products other than the cotton, corn, and tobacco had no market at all. The criticisms of the early American prodigality towards the land applies, therefore, even more aptly to the South. The land was more erodible, and there was more of it to be wasted for a longer time. Few soil scientists—having to deal, at any rate, with concerns and problems of soil in

their own regions—would have had the opportunity or inclination to look deeply into the question of whether there were any problems unique to southern soils, let alone undertake measures to improve them. Only because Bennett had the experience and background that enforced this commitment, did he set himself to the task of bringing attention to soil erosion. Working over two decades with little success, he was nevertheless prepared to go on speaking and writing about erosion, so that he himself raised the level of knowledge among those he contacted, and thus contributed to the shift of opinion. Bennett had been working to persuade his colleagues and superiors for many years when other historical events combined in the unique setting in which the first national soil conservation program was created.

4.1.3. Bennett Works Toward his Goal

Between 1908 and 1928 Bennett worked, directly and indirectly, for the recognition of erosion as a serious problem in America. While he is known almost exclusively for his work after 1935, a study of his publications before that period suggests that he had worked very hard to establish scientific credibility, with a view to persuading his superiors to take initiative on the erosion problem. In the pursuit of this goal, he constructed a strong ethos as a hard and willing worker and a reputation as the foremost expert on soil erosion in the US. He also became the Bureau's expert on soils of the southern US, publishing *The Soils of the South* in 1921, and was the individual in the Bureau of Soils with the most experience of soils in South and Central America.

4.1.3.1 Bennett's Contribution to Classification: Distinctions Caused by Erosion

In his single-minded pursuit, for over 20 years, of a means of dealing with erosion, Bennett gained a reputation as a voracious reader of publications on geology and soils, and as a "gargantuan" worker, able to endure hardship and privation, as on his assignments to Alaska and to Honduras and Guatemala. In building his professional ethos, Bennett, although primarily motivated to do something about erosion, nevertheless

contributed to the primary disciplinary focus of the period: a sound basis for classification.

This contribution was partly through his language choices in his soil classification work. Bennett changed some elements of the language he used to be able to express what he saw. For example, in "Soils of the Prairie Regions of Alabama and Mississippi and their Use for Alfalfa" (1911), he still did not distinguish explicitly between geological and what he later called "man-accelerated" erosion: as in "...the Selma chalk formation was lifted above the sea, subjected to erosion and resubmerged" (7), and he also merges the effects of the two kinds of erosion: "Since the emergence of the land, erosion has removed much of this sedimentary material" (note p. 6). Bennett is widely credited with the recognition of "sheet erosion." But in fact he was not the first to recognize it. The 1894 Bulletin, "Washed Soils: How to Prevent and Reclaim Them," discusses sheet erosion, but in different terms. Bennett achieved the overthrow of the perception that erosion was insignificant via his recognition and publicizing of the extent of erosion, his clearer explanation of the action of erosion, and his *characterization* of sheet erosion and the change of name that he eventually introduced.

Bennett's characterization of the quality of erosion with the term *sheet erosion* was important. As we saw in Chapter 1, when Hilgard was proposing the expression "crumb" as opposed to "kernel" for soil aggregates, his focus on the choice of a term was a recognition of the need to match language to the perception of nature and make "an ally" of nature in reaching for greater understanding. This is because language imposes a knowledge and a perspective on the language user. Speaking of Burke's views on language, Foss *et al.* write

Words are important in their own right and not merely as names that convey information or stand for their counterparts in reality. In fact not only does Burke assert that words are not only terms used to name things, but he reverses the equation and asserts that things are 'signs' of words. We cannot look to reality to know what a word means; instead we must look at language to see what reality means. Words impose knowledge on us—they create a reality for us. For example,

calling a police officer a "pig" is not as much a literal definition of the person as it is the placing of a person in a category that emphasizes certain attributes and attitudes that then serve as a motivation for our actions toward the officer. (1991:183)

Applied to soil science, this concept would refer to how a term instructs the audience to perceive a substance or a process. Part of Bennett's achievement in describing the invisible kind of erosion was in the name he gave it. The most common early term for erosion was "washing," which was too neutral, or even positive, to be allowed to persist if the process were to be stopped. Also, it was too general, and Bennett needed to designate a particular type of erosion. McGee, in his 1897 paper on "sheet-flood" erosion for the USGS, had called what was removed in that form of erosion "a layer of slime," the connotations of which term are decidedly negative. That is, a reader would not think it bad if "slime" were taken away. McGee's dyslogistic representation of the loss of soil is therefore not well calculated to convince the reader that this process is to be avoided. Bennett, on the other hand, choosing the term "sheet erosion" arrived at an expression that has no negative connotations and, while not semantically transparent, gives the impression that "a lot of soil" gets taken away. The actual process denoted by the term is that a thin layer of soil is lifted from the soil surface by rainwater and carried away across the slope. The term "sheet erosion" gives positive connotations to the soil that is lost by likening it to a sheet, a covering on the earth's surface that protects it from extremes of temperature, wind and rain; it's the least essential "cover" in a bed—security, comfort. Linguistic and rhetorical analysis are indistinguishable at the point of invention that seeks ways of expressing newly observed or reconsidered phenomena. The observer must find the word that suits the anticipated audience—specific or universal audience. With the opportunity to nominate a term for a process affecting soils Bennett chose a non-technical term with connotations that assert the topsoil to have a beneficial essence and that recommend implicitly that the audience perceive this same topsoil favorably.

Whitney's expectation, encoded in the survey field manuals for several years (Simonson 1978) was that every soil type would have every phase of texture, and this group of related

soils was referred to as a "soil series." The bureau's approach was naturally one of inventorying the soils *that were there*, despite the fact that the natural soil type in many cases had been altered by use. Thus surveyors were encouraged to classify clay soils, for example, as the clay-predominating phase of some typical soil. In such situations, Bennett was seeing not a natural soil, but the subsoil, or an "eroded phase" of a healthy soil, recognizing that the actual natural soil was *not there*. Whitney's "lens" did not permit him to see the soils that were not there, the ones that had been taken away by erosion. He did not acknowledge that the soils were changing more or less "under his very nose." His concept of the soils, as we have seen, was that they were a "permanent, unchanging" asset of the nation, because they were derived entirely from the breaking down of the parent material and were therefore nothing but pulverized rock. The only other forces that affected soils in this view, were movement by water or wind and breaking-up by freezing and thawing, although in fact chemical weathering is more significant than physical weathering.

Bennett made a discovery in recognizing that sheet erosion occurred, because it takes place on uniform or regular slopes, and the erosion does not leave the familiar signs of rills or gullies. Because of the cultural perception he brought with him he saw erosion, so that he saw soil classification not only in terms of what was there, but also in terms of what was not there or what had recently been brought there. Because he had such an experience of it himself, he read a soil profile from, say, a meadow, not in terms of geological-time soil deposition, but with the awareness of man-made erosion. This differentiation stood up to testing by other surveyors and was incorporated into survey practice (*Results of Erosion* 1936). Bennett had helped the new pedology to establish that the only way to gain fundamental knowledge of soils is to reckon with them as natural bodies rather than as substrates incidental to crop production; pedology in the late 1920s began to take account, for the first time since the 1840s, of the importance of aspects of the soil physical body rather than think of exclusively of the nutrients available in soils.

4.1.3.2. Emphasis, Awareness, and Discourse Space for Erosion

Jamieson (1983) and Miller (1984) suggest that looking at genres is fruitful for discoveries about rhetoric in science. When Bennett's soil survey reports are compared with those of other surveyors, it is apparent that he placed more emphasis in the reports on recommendations, that is, on the treatment of the land to build and protect it and to prevent deterioration. While most of the surveys simply end their text when the discussion of the last soil is completed, Bennett typically used this section to draw conclusions about the *quality* of agriculture and the standards of living in a given location. The emphasis on the quality of farming was Bennett's innovation, intended not merely to describe the agriculture encountered, but to shift it to reach a higher potential. Jamieson points out that a change of the genre is bound to draw a reaction, so that it becomes a form of social action, and it may eventually shift the entire ground of discourse (Miller 1984; Faigley 1985: 244). Bennett had clearly been hoping for more of a response from the Bureau than he did get after publication of the surveys of the 19-teens showing tens and hundreds of thousands of acres ruined by erosion.

Bennett had a conviction, from his own experience, of the importance of erosion. The depth and breadth of his experience of the importance of soils and of the susceptibility of soils, clay soils in particular, to erosion, enabled him to attend to and therefore see its effects on land and people in the South. Following the events of 1908, it soon became obvious that there was both ignorance of and indifference to soil erosion in his Bureau (McGee excepted), while soil conservation meant little or nothing to the public, including farmers and others. The term "had scarcely attained dictionary status" Bennett said (1958: 12). Bennett had remarked that at the University of North Carolina, the state land-grant university, no attention was given to the part of land in economics or politics. As Bennett said, speaking of a course in economics, "the story of the devastating effects of erosion on the land [was completely overlooked] in the classic texts on economics," (meaning Adam Smith's *Wealth of Nations* and Mills' *Principles of Political Economy*) and no allusion

made to the importance of land by the instructor (Brink 1951: 42). Bennett noted that his training in the Bureau of Soils had likewise given no emphasis to erosion.

A contemporary textbook can provide an illustration of the lack of emphasis and incompleteness of coverage on erosion that Bennett needed to address. Lyon and Fippin, professors of soil technology at Cornell University in New York, devoted to erosion only six pages (including the three half-page photographs and one engraving) of 500 in their 1913 text entitled *Soils*. As befits staff at a university highly regarded for its agricultural research both in the department proper and at its research station, the authors demonstrate a command of their topic that is very complete for the early date. Their text compares in complexity and completeness to research at the highest level of sophistication for that period, that appearing in the *Journal of Agricultural Research*, the USDA-supported organ for publication of research from the state land-grant colleges and their experiment stations.

Lyon and Fippin cover chemical and physical features of soils, soil organisms, and fertilizers and manures and their effects. They also present extensive information on the formation of soils and mineral nutrients, and on management and machinery for soil management. Their section on erosion begins well, echoing Shaler's pervading term, menace—"Erosion is often a serious menace to the productiveness of the soil." But their coverage does not continue to address in any depth the fundamental relevant topoi: mechanics of erosion, its effect on fertility and productivity, or the extent of affected lands. They only briefly address means of remediation. They write, for example, in their page-and-a-half section on erosion by water: "This type of erosion is a function of flowing water. It therefore occurs almost entirely on sloping land." This statement—no elaboration follows—seems to imply that sloping land is the exception in the arable lands of North America when in fact it is the rule. Even where the land is practically flat, however, erosion can be high, especially, when the rainfall is intense, the slope long and/or the soil highly erodible. Once the erosion research stations had begun to record

their findings, rates of erosion of 40 tons per acre were recorded as taking place on two percent slopes (Bennett 1928). So both by virtue of the actual prevalence of sloping land in the landscape and by the potential of erosion on practically flat land, a greater degree of concern is justified. Lyon and Fippin were not asking questions about erosion or according it discursive space. Without the interest to follow the issue in a spirit of inquiry, they were not finding or giving answers.

Why were they unaware of the prevalence and extent of erosion? The reason for this is not lack of at least bootstrap research. Lyon and Fippin can be expected to have encountered Shaler's work (their book includes extensive geologic background information) where plowing on land of over five degrees of slope is strongly discouraged, and where erosion was estimated to have taken place on approximately 4,000 square miles in the country. Lyon and Fippin might also have been expected to take an interest in the results of the national soil survey, both having been employed by the survey in its first decade, and because also, by that time, several of the counties in New York had been surveyed. Possibly, as New Yorkers, from a state with a tradition of land improvement nearly a century old, with sizeable regions of fairly high native fertility, and with a landscape of pasture and dairying, the idea of widespread, serious erosion may not have resonated with them or matched what Kuhn called "relevant conceptual categories" in their own conceptual matrix. At any rate, it did not attract their attention enough for them to address the question of how serious it was, and as we shall see, they do not make a judgment for action.

Lacking urgency, because unaware of the dimensions of erosion, Lyon and Fippin convey a passive attitude in the tone of their text. Here is a selection from their recommendations for control of water erosion:

Various protective coverings and binding materials may be kept on the soil. The most effective of these are fine-rooted crops, which not only hold the soil together, but protect it against the force of the water. In those sections where it thrives, blue grass is

permitted to occupy those areas of the hillside most subject to erosion. Trees afford a similar protection and are valuable in reclaiming eroded land. It is the general custom to retain in some cover crop those steep areas of land most subject to erosion. (496)

Here, the authors express "passiveness" to some extent with the passive voice in the first sentence and a state-of-being verb in the main clause of the second. But more than that, *they do not actually recommend* the preventive practices that they mention and their language suggests rather a grudging toleration of protective plant cover than an advocacy or promoting of it: "Various... binding materials may be kept on the soil." Bluegrass is "permitted to occupy" the steeper parts of the slopes. "Some" cover crop *is by custom retained* on "those steep areas of land most subject to erosion." This can be contrasted with Shaler's statement, "Wherever the inclination of the slope exceeds about 5° of declivity (or one in twelve), except where the soils are remarkably permeable to water, it may in general be said that *justice to mankind demands* that the field be as far as possible exempted from the influence of the plow" (333).

In the context of such a passive view of erosion as this, Bennett set out to make people aware of the process and its effects. From the time of the Louisa County survey, Bennett went on to find and report on the staggering erosion problems he saw—he saw it everywhere now, and his new understanding moved him from speaking or writing in the "context of discovery" to that of the "context of justification" (Kuhn 1970: 8). His surveys from around 1909 record the nature, extent, and location of eroded conditions, and, in connection with erosion, introduce some new classificatory terms for soil conditions reflecting not the presumption that a soil is a different substance because it does not have the same kind of profile as others near it, but rather that it is a substance changed by some process, geological or "man-accelerated." In 1910, a field party consisting of three members of the Mississippi Geological Survey and two Bureau of Soils surveyors, Bennett and Howard C. Smith, completed a survey of Lauderdale County of that state. This survey identifies an "erosion phase" of Orangeburg sandy loam, designating a soil, not as a distinct soil type, but as a soil that is modified by erosion; they

assert through classification that a soil difference was the result of erosion. "Meadow" in this survey, as in the Louisa survey, is identified as an erosional type, consisting of "material so extremely heterogeneous as to preclude type differentiation" (754).

Bennett *et al.* recommend cottonseed meal and acid phosphate fertilizers for corn on some of the soils of the county, saying that the amount needed varies with the character and condition of the soil. Emphasizing organic matter, they go on to say, "Again, soils rich in humus do not require so much as the droughty lands impoverished through severe usage"(742) and later, "It need not be expected that good yields of oats or any other crop can be secured by seeding on roughly broken land containing little humus" (743). In one section, "Rotation of Crops," the surveyors revert to the importance of incorporating organic matter, and to the importance of growing soil-improving crops rather than corn or cotton only:

Under the system in which cotton and corn have been the important crops, too little attention has been paid to rotation. One of the most serious drawbacks to the cultivation of so large an acreage of these clean-cultivated crops is that the organic-matter content of the soils is in time reduced to a point where the sandy lands become loose and incoherent and very irretentive of moisture, while the heavier soils compact and crust, resulting in increased susceptibility to drought. The necessity for growing more leguminous crops in rotation with the staples, both for the purpose of supplying humus and adding nitrogen, has already been pointed out. Cowpeas, velvet beans, soy beans, bur clover, and vetch should be included in rotations with cotton, corn, oats, rye, vegetables, and melons to a much greater extent. It is encouraging that cowpeas, particularly, are coming more and more into use as a forage and hay crop and soil improver... (751).

In this report, the surveyors discuss the Orangeburg sandy loam as "peculiarly susceptible to ruinous erosion under the conditions of rolling topography..., particularly in the southwestern third of the county." "If the gentler slopes are not terraced and the steep

situations kept in timber, deep gorgelike gullies or 'caves' gradually encroach upon cultivated fields." The gullies, with walls rising 25 to 50 feet high, have in places eaten into the ridges, "advancing from opposite directions...completely cut across high ridges, necessitating the construction of bridges." These bridges are "of very common occurrence along the ridge roads" (759-760). That bridges had to be built suggests that some collective effort had to be expended with respect to erosion; the irony is that the bridges would be only temporary, and that effort lost, because, in loess, the gullies would continue to eat away at the sides of the walls. The point goes to reinforce Bennett's insistence on dealing with erosion itself, not with economic or educational deficiencies in the farmers' environment.

The Lauderdale County report apparently was not edited in Washington, so its extended discussion of erosion effects and recommendations to remedy them were allowed to stand. Bennett carried on with survey work, describing erosion wherever he found it and raising awareness of it in the surveys and in his personal contacts (Swain 1963; Brink 1951: 53). In 1911, as Inspector for the Southern Division of the soil survey, Bennett was responsible for the "classic" report on Fairfield County, SC. There, by the account of his staff, 136,000 acres of formerly cultivated land had been ruined by erosion. The survey includes three photographs taken in the county. The first two show steep hillsides that had been cleared and planted to cotton; these were severely eroded. A third photograph shows a field being reclaimed by the use of "contour cultivation and terracing." This was the first survey to allocate a separate section to "Rough Gullied Land," as to a soil type, designating loss of soil from sloping land; this category alone accounted for 18.7 per cent, 90,000 acres, of the open land in the county (1911: 491). It was also the first survey to give the locations (by distance and direction from the villages) of gullied land within the county. The survey describes a landscape of ridges in the county, the ridges being "the remnants of a former extensive undulating plateau. The region has been subjected to severe erosion and is dissected by a large number of small streams, both large and small, giving it a generally rough topography" (1911: 480). Again the term "Meadow" embraces

the stream bottom land having soils with very varied textures. "Formerly the areas of Meadow were among the most productive soils of the county, but the wash from the uplands, where owing largely to poor management erosion has been severe, has covered the original soils (Congaree) with deposits of variable texture, largely sand, and made many of them worthless for cultivation" (1911: 510). Meadow accounted for the other 46,000 acres, debris from the sloping lands covered the originally fertile soils of lower level fields "It is believed that at present as large an acreage lies idle and abandoned from this cause (washing of eroded material over arable land) as is actually under cultivation" (1911: 487).

Beyond following the structure of the by-now well-defined genre of survey report, the authors detail the features of the individual county's agriculture and economy as it reflects the soil and its condition. In Fairfield County, they found that cotton and some corn occupy 87 per cent of the crop land in the county, and that the exclusion of food crops meant that "the necessities are now purchased at high prices" (1911: 487). They elaborate here on the connection between the cropping systems and the economy of the county:

The financial condition of most of the planters has been and is such as to demand that a ready-money crop be produced. The majority of the planters are renters, either white or colored, only 26.4 per cent of the farms being operated by the owners in 1910. Practically all of the tenant class operate on a credit basis, the owner of the land or some merchant advancing the necessary supplies and taking a lien on the crop, to be paid when the cotton is sold. Such a system is not conducive to the development of the county. Under the present system cotton is a ready-money crop, and the lien system demands that it be grown. It has been grown so long and so continuously to the exclusion of other staples that the labor scarcely knows how to grow any other crop... There is at present a tendency toward better farming on the part of those who operate their own land, although the tenants farm only for the present crop, taking no thought of the future. (487-88)

As suggested, the report describes in detail the conditions of eroded land in the county and the social and some of the cultural conditions as well; it makes a clear connection between erosion and both economic insecurity (tenancy, especially) and the bondage of tenants to short-term horizons.⁴⁷ Via their detailed recommendations, the authors are actually teaching erosion control practices in the survey. By stretching the limits of the genre—what a survey can present to the audience—they produced their own opportunity to emphasize and raise awareness of erosion and how to combat it, and introduced a selection of the pertinent and available topics related to it. The work of Bennett and his colleagues, however, did not arouse interest; indeed, “mere publication of the fact that bad farming in Fairfield County had wrecked 136,000 acres of prime land did not...make a ripple on the surface of public opinion” (Brink 1951: 58). Nevertheless, Bennett was set in his course to persevere until his call to action was taken up, because by the end of the 1920s, he had seen and surveyed eroded lands all over the Southeast and Southwest of the country.

4.1.3.3. Bennett’s Work In The Bureau 1911-1929: *Logos* and *Ethos*

One author of an appreciative article on Bennett and his work, T.S.Buie, divided Bennett’s work into five periods. The first spanned from his birth until his graduation from college in 1903, and the second extended through 1911, when he did his early surveying and identified, with Bill McLendon, the effects of sheet erosion, and also made the “classical” report on Fairfield County South Carolina with its 136,000 acres of formerly cultivated land ruined for use by erosion. That report, which he apparently expected would arouse public opinion, met instead with “massive indifference” (Simms 1970: 8). The 1913 survey of Stewart County Georgia featured a gully of thousands of acres, named “Providence Cave” because it grew by undercutting, that had begun where rain dripped from the eave of a barn roof; this survey was accorded the same reception.

⁴⁷ Cp. S.A.Knapp, 1908. “Causes of Southern Rural Conditions and the Small Farm as an Important Remedy.”

This indifference pervaded the geology and soils professions, with some exceptions, and it applied to the public. From 1911 to 1929, the third period of Bennett's life, as designated by Buie, Bennett undertook to develop "his concept of soil use and care" (2).

As we saw of the period around 1908, Bennett had discovered why soil erosion was hurting his native region so badly—so much of it took the form of sheet erosion, leaving no signs of its work as do rill and gully erosion—and he had seen, after the emphasis put on conservation, including soil conservation, at the level of national policy in 1908, little motion towards implementing any program of conservation—research or applied. McGee had lead a charge on the issue, but the Bureau administration did not follow, as indicated by the absence of discourse space given to erosion after McGee's death in 1912 (Lapham 1949). In order to create motion in that direction, Bennett apparently recognized, he had to foster an interest in erosion among the public and among the members of his discipline. First, to interest his discipline, he had to document and bring to his colleagues' attention the extent and degree of erosion. Bennett was in a good position to pursue this aspect of logos as a soil surveyor, from 1909 on, as Inspector for the Southern Division and subsequently Inspector of the Eastern Division, which encompassed the Southern.⁴⁸ From the vantage points of surveyor and then Inspector, he became knowledgeable about soil types and conditions on soils throughout the entire Eastern Division, North to South.

Not only did surveying permit Bennett to begin to document the extent and degree of erosion, it also gave him the opportunity to observe land use practices and work towards a program of remediation that he would not be able to put to use until the early 1930s. As early as 1910, in the Lauderdale County survey, he had begun to formulate his concept of using land according to its capability (Bennett 1959; Swain 1963), because surveying gave him the opportunity to observe and document conditions of soils well used and those not well used. Land use constituted the second area in which Bennett strove to develop the knowledge and expertise from which to arouse interest in erosion, and he addressed

this issue in his first conference paper, making the point that many areas then cropped or abandoned should be reafforested (Bennett 1921). But because Bennett seems to have been regarded by the Bureau hierarchy as, in Swain's words, "a promising but impetuous" young scientist he was sent away on special assignments, seemingly as far away as possible, after his erosion surveys of 1910 and 1911 and 1913. Brink (1951: 59) states that these assignments were intended to "cool him off." Whether or not he was intentionally sent away to distance him from his "main fight," his publications trail shows him having worked in the Panama Canal Zone in 1909 and 1913, Alaska in 1914 and 1916, and in 1919 in the area bordering warring Honduras and Guatemala, where he served with the international boundary commission. This intended diversion of Bennett's energies had an opposite effect for the Bureau hierarchy, because it gave him entry to a third arena in which to hone his skills for the erosion fight.

The assignments for which Bennett had to travel necessarily entailed comparative/contrastive soils study, and his reports were a timely contribution to the evolving view of soils. After his exposure to tropical soils in Panama in 1909, Bennett's next distant assignment was in Alaska. Simonson, writing on the evolution of soil survey and soil classification, points to Bennett's 1914 report of Alaska as the first use in US surveying of the Russian work on climate as a soil forming factor (Simonson 1987: 8). Only Hilgard in the US and a group of Russian scientists had begun to follow the implications of soil forming factors; but Hilgard's work, was conceptually inaccessible to many of his colleagues, because it resulted from a contrast that not many had experienced. Jenny's statement of why Hilgard's work did not "take" with his colleagues provides an excellent illustration of the function of presumption, or what part in science concepts from environment and culture can play:

The soil scientists on both sides of the Atlantic, though thousands of miles apart, and speaking different languages, had nevertheless one common background: they lived in the humid region. Throughout their lives they were so used to clouds and

⁴⁸ Swain 1963: 147; Brink 1951: 57.

fogs and high rainfall that it appeared as norm; never would they dream that this environment might affect their scientific outlook. But it did. To these people there was but one dominant soil forming process: leaching of weathering products into rivers, lakes, and oceans. Their foundation of pedology was geology, the science of rocks.... (1961: 71)

The genetic view of soils that was being developed by the vanguard of soil scientists of the late 1800s, Hilgard and Dokuchaev, Glinka, and others, was not understood by any but the most thoughtful researchers, until nearly 1920. G. N. Coffey alone at the Bureau of Soils had been trying to introduce Hilgard's work into the survey in about 1912-13, but without success (Simonson 1987: 8-11). Hilgard was doing climate-related soils work at the same time as the Russians, but Hilgard's work remained practically unknown in the eastern United States, partly because of Whitney, who proscribed the use of Hilgard's surveys to the survey staff and restricted access to his work (Tandarich and Sprecher 1994), and partly because it was so advanced.

Except for Coffey, most investigators were getting bits and pieces of the puzzle of the interaction of soil genetic factors, and because they were not synthesized, these bits and pieces were ignored. Bennett was making some connections of his own between climate and soils. Simonson regards elements of both Bennett's Panama and Alaska reports as examples "of lack of impact of ideas offered ahead of their time," ideas that he, Simonson, saw as contributing to the rapid and radical development of pedological understanding in the period. Bennett had been "prospecting" for information about erosion (Brink 1951: 57), and must also have been reading intensively, since his 1905 discovery in Louisa County; at some time in 1914, he must have come upon the Russian soil scientists' argument for climatic factors in soil formation. In Bennett's Alaska survey (with T.D.Rice, published in 1915) he draws some of this argument from K.D.Glinka's 1914 German translation of Dokuchaev's work, two copies of which had arrived in the US before the World War (Simonson 1987: 8). Marbut translated this volume between

1917 and 1919, and the period of the translation marked his conversion to a zonal and genetic view of soils.

While Bennett was still making some analyses on the basis of parent material as late as 1929 (Simonson 1986: 11), at other times, early in the period of adoption of genetic theory, he indicated awareness of the influence of climate on soil formation. In the Panama report, completed in 1912, Bennett and Taylor emphasized as "important and interesting facts" the little accumulation of organic matter on the soil surface with, simultaneously, a "surprisingly" high average organic matter content of the soils, and the "remarkable resistance to erosion offered by the soils" (in Simonson 1986: 8). Judging from his discussion before the Southern Forestry Congress of friable versus refractory clays (1921: 71), Bennett had already determined that the silica to sesquioxide ratio and the percentage of humus that he found in some tropical soils (in Panama) was a critical effect of climate; he reported on these findings after his work in Cuba in 1921. Bennett had found evidence that he articulated in the early 1920s as climate-related soil characteristics, seven years before the official recognition of the climate factor. The sesquioxides, ferrous oxide, and aluminum oxide, as contrasted to calcium, and their relations to climate formed an important part of Marbut's classification system, in which the basis of the central distinction was rainfall. Marbut and Bennett had traveled and worked together on a boundary surveying trip to Guatemala and Honduras in 1919, and in the early 1920s were considering some of the same chemical indicators of climatic effects on soils. It is possible that there was some "synergy" between the soil scientists in the conceptualization of the new genetic basis of classification (Guthrie 1996). The interaction of physical, chemical, and climatic factors seems to be the focus of much of Bennett's work in the early to mid-twenties.

Through the period from about 1910 to 1930, Bennett seems to have been, as Allen Harris says of Noam Chomsky, "hugging tightly to ethos and logos" (1989: 105). Bennett was moved out of the field to Washington in 1918, and at this point began to speak and

write more (Swain 1963). Like Chomsky, he achieved his first opportunity to publish with a reputable organization by presenting and having his paper included in the *Proceedings*; this was with the Southern Forestry Association. Recalling that the Division of Forestry had been established to conserve resources and to reforest land needing vegetative cover, Bennett was "singing to the choir" at the Forestry meeting, when he still could not get a hearing in his own Bureau. But Bennett persisted in working to establish a sound scientific ethos in this period, publishing extensively on soils in the South, conducting detailed technical investigations for his articles on the humid tropics, and, with R.O.E. Davis, contributing suggestions for technical improvement in the textural triangle to simplify soil survey work (1927). During this period he was also working to promote interest in erosion with scientists outside of the Bureau of soils and with the public in general.

Carrying out soil surveys had kept Bennett constantly in agricultural farming districts, and he claimed to have worked in every county of the United States but three. During his survey work, Bennett preferred to sleep outdoors or, in winter, to stay with the farmers nearest his work site. In so doing, Bennett took the opportunity to get an intimate knowledge of farmers' situations—comfortable or spartan. It gave him also an opportunity to "preach" conservation, but only after he had captivated his hosts with tales of his wide travels (Brink 1951). Bennett is reported to have buttonholed and talked erosion to virtually everyone he met. According to Swain, Bennett had decided as early as 1916 to do what he could personally, and so he "first attempted to arrange demonstrations of proper terracing methods in South Carolina." But these exhibitions "attracted scant attention," so he then learned to construct terraces himself, and offered publicly to help anyone who wanted to build one. Swain says that Bennett learned a valuable lesson by this experience, that "to preach theories to farmers was a waste of time. If a person 'puts on his old clothes and boots and gets into the practical side of it, he gets results'" (1963: 147). This lesson was converted into the farm demonstrations of the 1930s and the "farm face-lifts" that went on almost to 1950.

Biographers comment that while Bennett was not being advanced in the Bureau hierarchy, he was, outside of his organization, gaining a substantial reputation as an expert on erosion and on the soils of the southern US and of South and Central America. Bennett had many contacts among the public, made through surveying, with whom he promoted erosion control informally. He also had many contacts among the state experiment station researchers, because the surveys were carried out in cooperation with the states. He kept in communication with researchers in the State Experiment Stations who were interested in erosion, and information was exchanged through this network. The Bureau of Soils administrators were not traveling the states and seeing the condition of land and the conditions of farming in various parts of the country, and erosion was not taken up as a significant issue by the Bureau administration. But because of the survey, there had developed a large group of surveyors whose experience contradicted the official view. These surveyors saw and measured conditions on the land. As a matter of required survey data, the survey reports gave data on the prices of land, and land prices in the southeastern states were invariably low compared to those in northern states, causing survey writers occasionally to report with surprise the low value of land in some states as compared to those in others. Station researchers, like soil surveyors, were receiving no support from their hierarchy in Washington in their work on soil erosion, but such work had begun in several states. Research into erosion had begun as early as 1908 in North Carolina under the aegis of the State Geological Survey (Ashe 1908; 1909) and by 1914 by the Forest Service in Utah, where severe flooding occurred in short, steep, formerly wooded watersheds (Zon 1912; Forsling 1931). During the early years of the survey, a strong esprit de corps developed among soil surveyors (Lapham 1949; Miller 1949).

From around 1916 several state experiment stations began to do erosion study, work being done on terracing in Alabama, Texas, and Mississippi, and on experimental plots in

Missouri, Illinois, Indiana, and Iowa.⁴⁹ There began to be some communication among researchers sharing this concern in the late teens and early 1920s. While these stations were starting to publish bulletins on erosion, at the same time Bennett began publishing in the popular and farm magazines, such as *American Game*, *Country Gentleman*, and *Nature Magazine* (1927a; 1927b; 1928), and he contributed a monthly article to *Farm Journal* from late 1925 through 1926. In total, he published at least 40 items between 1920 and 1930, including his book and technical and popular articles, despite having had several assignments in South and Central America and the Caribbean during that period. His best known publication from this period was his 1928 Circular 33, *Soil Erosion, a National Menace*, written with the Forest Service's W.R. Chapline. This circular was regarded as more effective than any of the preceding erosion bulletins; the bulletin is not technical and it does not discuss treatment or measures to take to avoid erosion, as did all previous bulletins. Instead, coming shortly after the great Mississippi flood of 1927, the costliest in lives and property the nation had ever seen, Bennett and Chapline's bulletin emphasizes only the damages caused by erosion and the need for action to stop it.

Soon after the publication of this circular, Bennett finally saw some federal funding approved for erosion research. This came about through his connection to A.B. Conner, director of the Texas Experiment Station. According to a prearranged plan, Conner was to discuss erosion with Congressman Buchanan of that district. When the congressman maintained, as they expected he would, that federal money was to be spent for defense, Conner would bring up expenditure for battleships, and then argue that protecting the soil that supports the citizenship protects the nation. This "devious" arrangement worked, and as a result, Bennett was soon asked to testify before Buchanan's subcommittee, and an amendment for a sum of \$160, 000 over four years was attached to the 1929 appropriation for the Department of Agriculture. With this money, over a period of two years, Bennett established a network of ten erosion stations, on different erosion problem

⁴⁹ In the southern states, Mississippi: Carpenter and Gross (1918); North Carolina: Lynde (1918), Texas: Olsen (1919), Conner, Dickson, and Scoates (1930); Kentucky: McHargue and Peter (1921). In the

areas in the country: Guthrie, OK, Hayes KS, Temple, TX, Tyler, TX, Bethany, MO, Pullman, WA, Clarinda, IA, Statesville, NC, La Crosse, WI, and Zanesville, OH. Of these ten, those at Bethany and Guthrie were already in existence, but experiments at these stations were adjusted to match a model, so that all of the stations were controlling and testing the same variables. With this approach, for the first time, commensurable data on soil erosion from different locations in the US became available (Middleton 1930; Middleton *et al.* 1934). Bennett provided findings from these stations in "The National Program of Soil and Water Conservation" in 1930 at the annual convention of the American Society of Agronomy, the largest, longest established, and most prestigious body he had addressed up to that point (Bennett 1931).

4.2. SITUATIONAL FACTORS: THE DUST BOWL, THE END OF THE FRONTIER, AND THE DEPRESSION

As Henry C. Wallace observed, the government did almost nothing for farmers throughout the 1920s, which were years of severe depression in the agricultural sector. In this period the results of wartime expansion of production were making themselves felt. Grain production was expanded drastically in the US in WWI, and the war years were the tail end of expansion and good economic times for the farm sector; this period leading up to the war was the basis for the concept of parity—the economic standing of the farm population relative to that of the rest of the population. From about 1910, because the agricultural economy was good and because no more government land was available, the price of land had begun to rise. In the wartime expansion, farmers in general, and Midwestern grain farmers in particular, invested heavily in land and in machinery, such as threshers. Forty million additional acres were plowed up to meet the war-time demand, and there was a price to pay when this acreage was not reduced (Cox and Jackson 1937: 5). The collapse of the overseas markets after the war brought the collapse of prices, and farmers met—or attempted to meet—the pressure to pay debt for capital expansion by continuing to grow more crops, producing more but getting lower per-unit prices.

Midwest, Indiana: Fisher (1919); Illinois: Mosier and Gustafson (1918).

Two major outcomes resulted from the continuing high production in a reduced market. The first was the inevitable farm sector depression, from which the farm economy was just showing signs of emerging when the stock market crashed (USDA *Yearbook* 1928). By the mid 1920s, sociologists and farm economists were considering what to do about the "farm problem" and were also seriously considering various forms of land-use planning (Wehrwein 1936; Wilson 1937). This was the first point at which thought had been given, in any considerable degree, to the government's taking responsibility in preventing overproduction in agricultural commodities and to restricting uses of land according to their characteristics. In an address to the American Farm Economic Association, F.F. Elliott of the Agricultural Adjustment Administration said that

...farmers as a group have frequently found themselves in an extremely unfavorable position as a result of increasing their efficiency of production, particularly since industry has not lowered its prices proportionately with its increased efficiency. And it is notorious that our extreme laissez faire policy toward land acquisition and settlement has resulted in important examples of unwise settlement and over-expansion. (1936: 18)

The second outcome, also inevitable, was the degradation of both rich and marginal lands being farmed. The wartime expansion had already led to cultivation of land that should not have been broken up, and after the war it was being worked even harder to get the farmer out of debt.⁵⁰ This occurred as the rich loessal bluffs of eastern Missouri, southern Illinois, Indiana, and Iowa, now lacking cover from continual cultivation, physically degraded from hard use, and hammered by the hard rains of the North American continent, dissolved into the Mississippi. In the context of this effort—to push the soil to make wartime production and then to produce solvency in the post-war agricultural depression—that the indifference of a large part of the soil science community gave way to

an acute concern. The recognition that erosion was a problem in this area under these circumstances is reflected by the erosion research and extension work begun in those states, in Illinois in 1918 (Mosier and Gustafson), in Indiana in 1919 (Fisher), in Iowa in 1920 (Hoffman and Turner), and in Missouri in 1923 (Duley and Miller).⁵¹

In the marginal lands in the West, land was deteriorating as well. In the Oklahoma land rush, April 22, 1889, from twelve to forty thousand settlers entered the virgin territory of Oklahoma, a settlement made possible by the largesse of a laissez-faire government perspective. A few weeks later the population of the state stood at sixty thousand. The very next year brought a drought so serious that Congress was forced to vote for aid for these settlers. The state's population rose to 130,000 in 1892, and 389,000 in 1900 (Hyams 1952: 144-45), having risen again quickly after the decade of drought (1886-1896). "Rain will follow the plow," speculators told the homesteaders (Reisner 1987; Smith 1966: 60-64; Sears 1971). Congress heard testimony from geologist and explorer John Wesley Powell that it "would be almost a criminal act to go on as we are doing now and allow thousands and hundreds of thousands of people to establish homes where they cannot maintain themselves" (Smith 1966: 65). The region beyond the 100th parallel had already proven that it was a very fragile environment, but Congress ignored scientific testimony that cultivated land in the short-grass region was liable to "blow out" in time of drought. Bound by cultural presumptions of a humid area, the Eastern-dominated Congress refused to grant larger blocks of land for family settlements, when larger blocks would have allowed for extensive farming (grazing) rather than cropping (Hurt 1985; Sears 1971: 4-5). Oklahoma was at the very heart of the dust storms to come.

⁵⁰ Barger and Landsberg show a precipitous drop in pasture/hay production from over 13 million short tonnes in 1908 to less than 2 in 1936. Production levels of other commodities were rising at the time (1942, Figure p. 23, Table p. 336).

⁵¹ Research in the Midwest grew quickly at this point. It is further represented also by Lehmann and Duley (1920), Miller and Krusekopf (1932), Duley and Hays (1932), and by Duley (1931) in an article in *Science* for the general public.

In the 1920s and early 1930s, the majority of Congressional representatives were from farm districts (Batie 1985: 12). The Depression brought home to them the destruction of community and national prosperity that would result if a government does not take responsibility for some of the economic and market forces affecting its people. The greatest lessons that were learned were to do with finance and agricultural markets. The recognition of this past neglect was represented in the avalanche of legislation addressing the state of the economy and the state of the land: the establishment of the Reconstruction Finance Corporation (1932), the Agricultural Adjustment Act to provide payments in return for curtailed production (1933), the Taylor Grazing Act and the establishment of the Natural Resources Commission (1934), the Securities and Exchange Commission (1934), the Soil Conservation and Domestic Allotment Act and the Flood Control Act (1936), Federal Crop Insurance (1939), and the list goes on. Sandra Batie, writing about the convergence of the Depression and other factors leading to the establishment of the Soil Conservation Service, points to government's recognition of its role in preventing financial and resource irresponsibility as the characterizing feature of the New Deal, and the farm sector's difficulties as key to this recognition. As well it should have been; in 1935 farm numbers were at a peak, the highest—6.8 million—that they had or have been in the history of the country, and the farm population had been in economic distress since 1920. Between 1929 and 1932, the already depressed net income of agriculture fell 70 per cent and the index of prices received by farmers fell 56 per cent (Batie 1985: 5-6).

As we saw of the record of the handling of the Mississippi River project appropriations (Smith 1966; Swain 1963) this assumption of responsibility was long in coming. Contrasting with every president since the earlier Roosevelt, President Franklin Roosevelt was committed to conservation. Even before his term as governor of New York, conservation issues "had a reality for Roosevelt far beyond the newspaper headlines of the Progressive Era" (Smith 1963: 241). He had been chairman of the New York State Senate Forest, Fish, and Game Committee before his governorship, at in both positions he had been involved in reforestation for the state, to reclaim both state land as well as his

own Hyde Park property (Little 1987; Owen 1983), and his statements about the multiple dimensions of conservation demonstrate that he was well aware of the long-term positive effects, both agronomic and economic, to be gained by the intentional effort to implement conservation practices, as well as the externalities to be avoided. Roosevelt reportedly held the controversial view that the Great Depression resulted from the farm depression of the 1920s, the eventual effect of reduced participation of a very large proportion of the population in the industrial market throughout that period. Just as a healthy farm sector had helped fuel the boom years up to and including World War I, the collapse of this sector eventually brought the collapse of the financial sector (Held and Clawson 1965).

Guided by this perspective, Roosevelt was ready to meet the challenge of the Depression by institutionalizing the concept of responsibility. Roosevelt's election also coincided with the recognition of agricultural economists that land use and production levels should be planned if great financial loss was to be avoided (Elliott 1937). Another simultaneous recognition was that landowners under conditions of financial duress were powerless to make improvements, especially to make long-term investments such as erosion-control structures. It was also being recognized that tenants were liable to be able to contribute even less to the capital improvements of the nation's farm sector (Hardin 1951; Schickele 1937). Indeed, tenancy had come to be regarded as closely connected to erosion, and during the agricultural depression and the general Depression, tenancy rates rose to alarming levels; rates were high not just in the southern states where they had been very high since the Civil War, but even in the high-producing states of the Wheatbelt, Illinois, Indiana, and Iowa. Just as tenure security was connected to responsible land use practices, land degradation was beginning to be recognized as fitting into a pattern of financial insecurity and rootlessness.⁵²

⁵² The dynamic between tenure to land ethic has been recognized from about the turn of the century; see, e.g., Woods 1908; Knapp 1908; Lowdermilk 1937; Bunce 1942; Maris 1950; Hardin 1951; Schickele 1937; Vaughn 1996; Wehrwein 1936; Wilson 1937, Parsons 1991.

As part of the New Deal agenda of the "Relief, Recovery, and Reform" (Batie 1985: 6), Roosevelt's administration encouraged the Congress to establish a Civilian Conservation Corps (CCC). The Corps was envisioned as being able to reduce unemployment and also to make a positive contribution to the future state of the nation's resources and instill in the young men who participated a sense of a stake in that future (Owen 1983: 14, 82-83; Swain 1963). By the mid-1930s, Congress had begun to accept both the principle of responsibility and the connections among complexes of physical, social, and economic causes. Accordingly, when Congress enacted the National Industrial Recovery Act of June 16, 1933, the way was opened for publicly financed conservation projects. The CCC was established under this authority "to carry out reforestation and other conservation projects in the national forests and national parks" (Rasmussen 1982: 6). A five-million dollar appropriation was to be made available for erosion control on private and public lands, with work to be administered by the Bureau of Agricultural Engineering to provide both conservation works and relief labor. Bennett approached his superior, Assistant Secretary of Agriculture Rexford Tugwell, who represented the USDA on the Board of Public Works, about how the job was to be carried out, fearing that it would consist of terracing only, when an integrated approach was really needed. Bennett by this time had finally attained some degree of acknowledgment within the Department of Agriculture; Tugwell, a new administrator with Roosevelt's administration, greeted Bennett as the nation's erosion expert. Thus with Tugwell's support and Bennett's planning, the Soil Erosion Service (SES) was established, in the Department of the Interior, in September 1933. Other submissions had been made to Congress for the use of this appropriation, but they would, in fact, have applied the funds for terracing only. Consistent with his position from as much as 25 years earlier, Bennett argued successfully then that an integrated approach was needed. A true conservation effort would establish not just terraces, but strip cropping, water management, crop rotations, pasture improvement and management, reforestation, and farm planning on the affected properties (Helms, in press). Bennett called for a multidisciplinary approach, requiring the integration of many kinds of technical expertise into plans for each site; the contributions of agronomists, engineers,

foresters, soil scientists, wildlife biologists, and social scientists would all be needed. The very values that had been something of a professional liability to Bennett in the Bureau of Soils—his commitment to action in an organization whose mandate was for research, and his lifelong experience with all aspects of farming—led him to approach soil management and conservation as a multidisciplinary task (Headley 1984).

Upon receiving the SES appropriation, Bennett lost no time in undertaking work that he had been trying to get done for decades. By mid-October, there were twelve people on the payroll, and the erosion reconnaissance survey was started immediately. One of the first administrative actions affecting the SES was the transfer to it from the Forest Service of over 150 Civilian Conservation Corps (CCC) camps (Simms 1970:17). Over the next two years, Bennett set up approximately 40 large demonstration projects, applying a wide array of soil conserving and restoring practices at the demonstration sites; the CCC enrollees were engaged in doing the actual work on the demonstration projects—planting trees, building erosion control structures, planting cover crops. This work was being well received by farming communities and the general public and attracted a great deal of favorable attention, suggesting that many people thought it was the right thing to do. In December, Henry C. Wallace, Secretary of the Department of Agriculture, sent Harold Ickes, Head of the Department of Interior, a memo requesting the transferal of the SES to Agriculture, a move reputed to have demonstrated the success and popularity of the SES research and demonstrations. This work had been underway only a few years when nature gave a verdict on the land-use practices of the western states.

In March of 1934, following a dry winter, dust storms blew up in the short-grass prairies, just as the scientists of the state colleges of the region had predicted (Sears 1971). The clouds swept to and through the states of the Eastern seaboard, dissipating as much as two hundred miles out over the Atlantic. Early in 1935, Congress was considering the status of the SES, and Bennett was to testify on the question of continuing the appropriations. By this time, through the work of the erosion reconnaissance survey and the research

stations, Bennett had accumulated a great deal of data on types and extent of erosion, and his testimony in the report of the Hearing is massive (Bennett 1935). Bennett had a particular reason for wanting to dilate upon the importance of erosion: the hearing is reported to have been scheduled on a day that he expected a dust storm to arrive (Simms 1970: 15-16). On March 6,

...the SCS chief was again recounting the nation's soil erosion problems to Congress, this time before a subcommittee of the Senate Committee on Public Lands. The subcommittee, under the chairmanship of Senator Carl A. Hatch, was reviewing a proposal to establish a permanent soil conservation agency. Bennett had been told by his aides that another big dust storm was building up and moving eastward. He kept one eye on the window as he reminded the senators of the conditions that were resulting in widespread wind erosion damage. He painted a vivid word picture of the 1934 dust storm, in which, he said, no less than 300 million tons of soil had been blown off the Great Plains lands. During Bennett's testimony, the sky darkened and for the second time in twelve months yellow soil from the far-off Dust Bowl began to settle on the streets of Washington.

Following this testimony and the submission of subcommittee recommendations to the House, John Dempsey of New Mexico introduced H.R. 7054 committing the federal government to a policy of soil conservation and establishing the SES as a permanent body to carry out this policy (Brink 1951: 16). The bill passed both houses without a dissenting vote. In thus realizing at last the goal of a means of acting against erosion, after 20 years of advocacy, Bennett brought to its logical conclusion the reasoning of the first Roosevelt, who connected the guardianship of natural resources with a society's responsibility for its future generations.

A statement found frequently in articles about soil conservation runs something to the effect that "the Soil Conservation Service was authorized by Congress in 1935 and H.H. Bennett, a soil surveyor, was selected to run it," suggesting that Bennett was just a soil scientist with reasonable qualifications who was in the right place at the right time. But

Bennett had been pushing the issue with great energy, total commitment, and growing competence as a public figure for many years. In his *Rhetoric of Motives*, Kenneth Burke wrote that "there may be fortunate moments in history when both kinds of motives work together," when powerful actors' temperaments allow them to act in accordance with the requirements set them by the scenic conditions; in such circumstances they can act with appropriate boldness or adventurousness and caution, as needed. "The scenic conditions require a certain kind of act; and the ruler may happen to have exactly the kind of temperament and character that leads him into this same kind of act. Given such a lucky coincidence, the perfect manifestation of the scene-act ratio is one with the perfect manifestation of the agent-act ratio" (1969:163). Bennett certainly was at the right time for the circumstances, but, because he had already been at work for decades helping to establish a sounder understanding of soils and, especially, promoting conservation, the circumstances included elements of his own making: a raised awareness of erosion and some hard data on its prevalence and distribution. Thus the rhetor had helped to create the rhetorical situation by making a difference between passive receptors of information and a true "rhetorical audience."

CHAPTER 5

SCIENTIFIC KNOWLEDGE, SITUATIONAL CONTEXT, AND POSSIBLE ACTION

For the farmer, the chief tool is the soil itself, or rather it is partly tool, partly material, a unique combination in the whole scheme of production. It is unique in that it is both a variable factor, affected by each period of use, and at the same time a constant factor, which cannot be replaced.

N. Gorgescu-Roegen

5.1 REVIEW OF THE STUDY

In the course of this study, we have undertaken to examine the nature of rhetorical situations leading to and involved in the adoption of soil conservation policy. Of particular interest within the rhetorical situation have been the roles of the rhetors, that is mainly, soil scientists in their disciplines and historical settings, and the relationship of their invention to the rhetorical situations. The rhetorical situations examined, from the beginning of soil science studies to the establishment of the US Soil Conservation Service, have been seen to vary greatly, with this variation reflecting factors ranging from knowledge about fundamental soil processes, to the kind of audience addressed, to climatic conditions, to political leadership. This examination strengthens the post-modern contention that science develops in context and is not the entirely objective artifact of a method, but also reflects the participants' contextual presumptions and their commitments to social and community goals.

In the early days of soil science in America, although soil erosion had been progressing for more than a century, issues of conservation were not even considered. This was partly because soil studies were adopted from England and Northern Europe, and the as-yet unquestioned presumptions about climate and management were adopted with them. Cool-temperate climates with soft rainfall predominated in these areas, and so soil studies reflected no need for concern with erosion, and logically gave it no discourse space. Denser settlement patterns also had an influence on the perspective of European soil science: labor was more available, fertilizing materials were more highly valued, and there were markets for a great variety of farm products. The perception of conserving soil that developed under European conditions was one of preserving the nutrient levels in the soil, mineral nutrients being of most concern in the dominant soil study paradigm in Germany, and organic matter and texture in addition in soil management in England. Social and scientific presumptions about the nature of soils accompanied English and North European settlers to the Americas.

Not long into the period of settlement of colonial America, a new set of issues emerged as fundamental to soil conservation. Because of the immensity of the new land, farmers as a rule did not take care of the soil; the emerging pattern of exhaust and abandon was reported by many European travelers to the colonies. Under natural and agronomic circumstances in New England and in the southern coastal states, maintaining the soil was impossible even for many who tried. Presidents Washington and Jefferson both acknowledged that they were unable to maintain their own lands in Virginia. Labor was scarce, fertilizing materials were essentially unavailable, and there was new land to turn to when the old was exhausted. In some areas and classes, farmers would reputedly brag about how many farms they had worn out, as though it were a measure of achievement. When soil science had its tentative beginnings in America in the mid-1800s, English and European assumptions came with them, assumptions established in crowded, land-hungry societies and in climates with mild rains and temperature regimes favoring the maintenance rather than the breaking down of soil physical attributes. The predominance

of softer rainfalls is regarded as one of the chief differences in these unrecognized perspectives, but, as we have seen, the factors of low population and pioneer conditions were equally important.

The conditions so severely affecting soils in New England and the southern states required decades to be articulated into their causes, effects, and implications. The difficulty in discovering the reasons for infertility had partly to do with what topics were already given authority by received soil science, and partly to do with the complexity of the causes of acidity (in New England), and with the complexity of clays and their part in creating acid conditions (in the southeastern states). The relative solubility of minerals, for example, was a very important key to understanding the poorest soils of the eastern states, because high levels of the residual (because less-soluble) aluminum and manganese reached toxic levels when other nutrients had been extracted. Equally important as a cause of erosion, however, was the presence of close-textured old, leached clay subsoils underlying shallow topsoils; once cleared and cultivated, these soils were highly susceptible to erosion. Difficult issues in soil chemistry were only resolved theoretically in the late 1920s, but this need not have been so, and the lack of such resolution need not have stalled work and promotion of soil management as it did. That is because, first, fundamental studies leading the way to understanding the relevant chemistry questions had been done in the 1800s, and were being overlooked only because of the internal struggles of the federal Bureau, and, second, these issues were already being addressed competently in an empirical fashion by the state experiment stations whose focus, unlike that of the Bureau of Soils, was more practical, willing to "tolerate theoretical confusion" while achieving control by proceeding on and accepting the results of work on an empirical basis.

Difficulties in comprehending problems in soil fertility in this period reflected a lack of scientific leadership in the USDA Bureau of Soils, which was exacerbated by conflicts among personalities and across institutions for the first decade of the twentieth century.

Individuals interested in soils in this period had a welter of often conflicting theories, messages, and influences to contend with in the process of attempting to understand a substance as complex as any on earth. Lack of disciplinary leadership in a science that was at its best when problem-oriented meant that practical recommendations were being delayed in a context in which they were badly needed. By the early 1900s, all of the arguments that later proved central for soil fertility maintenance and soil conservation had been found, rediscovered, or voiced, but the external climate did not permit their becoming the basis of action.

In the first two decades of the 1900s, people within the federal Bureau of Soils, and researchers in other organizations—the Forest Service, the Bureau of Plant Industry, and the State Experiment Stations—were attempting to meet this lack of leadership in the development and application of sound management practices. To those working in the southeastern states, to western researchers encountering overgrazing and deforestation and subsequent floods, and to researchers in the Midwest working on fertile but highly erodible soils, erosion was clearly a serious problem, so much so that they worked on this problem despite lack of approval and even active disapproval from their institutional superiors. In this process, researchers of whatever affiliation who shared an interest in active soil maintenance and soil conservation built a network of communication and a shared spirit of purpose. The importance of erosion and the strength of the subsequent trend of land abandonment were also becoming apparent to the majority-rural members of Congress as a problem affecting their constituents and their political bases, because it had become clear that tenant farmers made for an unstable electorate and were poor community members. As an outgrowth of the communication and cooperation among these groups—Congressional representatives and soil researchers of various organizations—of the individual efforts of Bennett, who continued to push for research funding, and of the willingness of the Roosevelt administration to take responsibility for action to protect natural resources on private land, the Soil Conservation Service (SCS) was established.

For several years after its inception, the SCS grew and was assigned responsibility for more funds and programs, for example, in connection with the Agricultural Adjustment Act. One of the major SCS activities was conducting farm demonstrations, in which all the applicable soil conservation practices were put in place on a farm or on a watershed. In 1936, the SCS carried out 55 demonstrations in the Great Plains alone (Helms 1992: 142). The SCS also carried out farm "face-lifts," completing over 100 in 1947 and 1948, and just under 100 in 1949 (Brink 1951: 125-6). In these activities, every aspect of a farm property and operation would be considered; they were not aimed solely at preventing the movement of soil but at making possible a system of sustainable production. Thus these demonstrations required the participation and planning input of scientists and technicians from a wide range of professions. The depth and correctness of the insight and practice of this SCS work seem to have had a strong resonance among a great number of people: numerous books on conservation were published at the time, conservation services were established in other countries—many in Africa and in South America, and there were many awards to Bennett, including nomination for the Nobel Prize.

Through all this time, however, there was a simmering undercurrent of conflict with the state Extension Services and Experiment Stations, which erupted in 1953 under Eisenhower. SCS did its last demonstrations and farm face-lifts in 1949, and no more after the new administration commenced and Bennett retired (Parks 1952; Headley 1985). Beginning in 1953, as the Eisenhower administration began to implement its policy positions, changes were in the wind for the SCS. All though the early years of the SCS and before its establishment, opposition to a national soil conservation program had existed. The source of this opposition was primarily the state experiment stations, the Missouri Station being one of the chief stations to object to the program. In many states an alliance had been built up between the Farm Bureau Federation and the experiment stations, with the effect that in states where both institutions were strong, there was strong opposition to the SCS. Missouri was one of these states. Eisenhower and his brother Milton were Missourians, and Milton, who took an active role in the Eisenhower

administration of agricultural programs, had been connected with the Missouri Experiment Station. Despite the fact that the experiment station and the SCS seemed to have had excellent relations in the early days of cooperative work, this state of relations did not continue, and by Milton's time there was considerable resentment in Missouri of the SCS.

Bennett retired in 1952 (his term of employment having been extended one year past retirement age by President Truman), and was no longer able to protect his programs. Under the Eisenhower Administration, the research activities of SCS were transferred to the Agricultural Research Service, the regional offices were abolished, and responsibility for a number of programs was transferred out of SCS. Speaking and writing in a very active retirement, Bennett lamented that the excisions being made by the Eisenhower Administration would severely hamper the Service's ability to put conservation on the land. The depredations visited on the service after Bennett's retirement support the statement frequently made about him that he persuaded via the strength of his personality. At any rate the early cooperation between the Soil Conservation Service and the state experiment stations that had been kept alive through the long-term friendships of Bennett and station researchers was at this point disrupted by dissatisfaction, also of long-standing, over the distribution of funding and responsibilities between the systems.

5.2. CRITICISMS OF BENNETT AND THE SOIL CONSERVATION MOVEMENT

Bennett succeeded in bringing attention and action to the issue of soil erosion. His success in this, a rhetorical goal, was made possible through *emphasis*, regarded by Weaver as the essential characteristic of rhetoric. Nevertheless, emphasis on one aspect of a situation must inevitably displace other aspects of the situation to the background. Bennett is thus criticized for giving legislators and the general public a distorted perception of the erosion situation by overgeneralizing about the extent of erosion and the permanence of its effects, and also by de-emphasizing other soil-degrading factors (when

in the early years of the SCS he tended to speak of erosion as though erosion were the sole agent of damage to soil). In the context, as at present, of widespread and assumed use of fertilizers, the possibility of erosion ever becoming serious again seems remote; indeed to some commentators, it seems that the present low rates of erosion suggest that erosion was always over-stated. Once fertilizer use was widely adopted, it was found to reduce the severity of erosion and to help to reclaim eroded land. That is because, by replacing mineral nutrients lost with erosion (other soil properties cannot be replaced), fertilizers 1) promoted vigorous root growth and penetration, which 2) improved the texture and composition of the soil and promoted the production of plant body-foilage-to protect the soil surface from the impact of raindrops (Buol 1996; Joffe 1949; McVicker 1952).

Thus Bennett's soil science critics are correct to criticize him for foregrounding erosion as the most important soils problem in the country. There are four main bases of these criticisms. First, Bennett exaggerated the extent of erosion throughout the country, basing his estimate of its seriousness on criteria relevant to his own region. Second, he is criticized for continuing to claim that vast areas of land were or would, at the present rates of erosion, be permanently lost to agricultural use, again on the basis of his experience of southern soils. The faults in Bennett's position were twofold: many soils, especially midwestern soils, are so deep that even relatively high rates of erosion would cause little impact on productivity for centuries, and, on the second point, a great many soils that he determined to be so badly damaged as to be permanently lost to productive use were reclaimed much more quickly than Bennett thought possible (Larson 1995; Crossson and Stout 1986).

A third criticism that was leveled at him was that he attributed to erosion, the physical removal of the soil body, problems that were actually caused by more complex processes of soil degradation. It is clear in retrospect that crop yields were stalled between the 1870s and the 1940s. Many critics maintain that this had more to do with declining soil fertility than with erosion, because lack of fertility was the first cause of the erosion (*Soil Survey*

1951: 261). In Faulkner's *Plowman's Folly* (1943), this is what he means to indicate with his provocative expression, "soil does not erode"; he means that if a substance is in a condition to erode, it is not longer fully a soil. Soils in a natural state maintain mineral nutrients in their topsoils through biocycling; this is why the settlers found majestic trees in Virginia, Georgia, and the Carolinas on soil that would soon, nevertheless, wear out. The perspective is that these soils would not have worn out and then eroded had the fertility levels been maintained, but circumstances in colonial times and after made it unprofitable to maintain them. Bennett is accused of attributing wholesale to erosion virtually any kind of soil problem. By making a symbol of erosion, Bennett was representing it as a first cause, when soil degradation resulting from cultivation without adequate management was often more accurately the first cause. This criticism is well founded, in that lack of fertility can be the first cause of erosion. But this is not entirely the case, because while mineral depletion often plays a role in erosion, there are many soils that erode quite easily once plant cover is removed, and climate and cultivation practices exacerbate this susceptibility. Again, this criticism can be countered by the argument that Bennett always advised an integrated approach, of which all the elements would lead to improved fertility.

It has been recognized that the early claims about soil erosion had been inaccurate and overgeneralized. Pierre Crosson and Anthony Stout, in their 1983 study for Resources for the Future, *Productivity Effects of Cropland Erosion in the United States*, conclude, "It is clear that in the 1930s when the modern soil conservation movement took shape in federal, state, and local institutions, knowledge about erosion amounts and productivity effects was anecdotal, highly fragmented, and quite inconclusive. Nevertheless, sweeping statements were made indicating that erosion had permanently ruined tens of millions of acres and threatened even more if it were not brought quickly in check." Studies with this emphasis tend to originate in the Midwest, in areas with deep soils, where productivity is not so quickly affected by erosion. They do not contradict the fact that eroded material is higher in plant nutrients than remaining materials or the fact that erosion is still extremely

serious in regions with shallow topsoils or soils that are less fertile even in their original state.

The fourth criticism leveled at Bennett was that he was largely a publicist. He was renowned as a forceful and persuasive speaker, and was very successful, eventually, in achieving his goals. Douglas Helms points out that although Bennett "may best be remembered for his emotional appeals... it should be remembered that he first called for research" (1992:16). Bennett and those he worked with were only able to achieve their goals within a favorable rhetorical situation, consisting of many other factors. One of the most important of these factors was the growing body of fact on erosion, especially its extent and degree, to which they had contributed themselves through the soil survey and through individual formal or informal research. When he did receive funding for erosion work, much of the initial effort went to developing more data on the processes of erosion.

What Bennett did that was most effective was to make a symbol of erosion. Where soil scientists had previously been unable to obtain any considerable collective action to control erosion because their target had been fertility, Bennett was able to achieve that goal by designating it as an enemy of sorts. Gullies could be seen, mineral nutrients could not. Symbols evoke emotion in ways that are not understood, and emotion is necessary for action (McKeon 1987 [1955]; Wallace 1974). By focusing on erosion and making it symbolic of the entire complex of soil problems, Bennett was able to coalesce serious concerns about soil conditions in general and about the lack of official attention to their management. This approach got greater results than previous approaches. He brought greater infusions of money, manpower, and attention to soil problems than ever before. Bennett's critics claim that this was not appropriate for a scientist. But while he was criticized for overemphasis on erosion, his practice in carrying out conservation work showed that, as from his earliest soil survey reports, he regarded erosion in the context of long-term fertility and management practices of whole farms and whole communities, not as an isolated phenomenon.

5.3. RHETORIC OF SCIENCE IN THE RHETORICAL SITUATION OF SOIL CONSERVATION POLICY

In the course of exploring the contexts in which soil conservation was adopted as policy we have found support for conclusions on several points. The first point is that soil science is indeed highly rhetorical. What is "true" about soils has varied with situation; in different geographic locations, different soil facts were found and deemed "true," not just because the soils themselves have varied, but also because management practices affected the assumptions of the researchers about soils, their processes, and responses. To an extent, in the early days of the science, study techniques also varied with location. Soil science has varied over time mainly because of the development of equipment and techniques for study. Both in time and place, personalities and institutions have also affected the content of soil study, including suitable topics of study and what could be said about them. Invention or discovery about soils is therefore affected by this local and time-bound essence of soils as well as by the culture-related constraints of investigation.

Further findings, also about soils, are that the assessment and valuation of soil conditions are also situational. We found that soil erosion and soil degradation are more objectionable to larger units of society and when there are many people on the land. When there are few people on the land, as especially in the 1800s and before in the South, these processes can cause the absolute dissolution of communities. Several conditions result from this situation. First, as communities disperse, their culture and their values are destroyed; our study showed that this happened in the South, and that in the awakened conservation perspective of the 1930s and 1940s, a number of studies were written pointing to the fact that this had happened repeatedly in antiquity. Thus this loss can be recognized, but because it happens bit by bit, in dispersed locations, and over time to particular landowners, witnesses to the facts about the former communities and about what happened to the soil are few, and their reports are qualitative and anecdotal.

But the nature of the proof did not change the condition of soils, so that, at a later time when there were more people, the topsoil and its fertility were missed, and the kind of treatment that was the cause only of dislocation and impoverishment of a relative few became regarded (or recognized) and condemned as a practice that was detrimental to society. This latter view represents another finding. Just as Shaler foretold, soil degradation becomes recognized in retrospect, and in the view of a more populous nation, as a threat to the well-being of these later generations. While soil degradation made a relatively small impact in the early 1800s and before, it did, according to Bagley, Hall, and many other researchers, contribute to the constraints that led to the Civil War, and also to the inability of the South to prosecute the war successfully. In other words, most certainly, a degraded natural resource reduces individual choices and freedom; it represents the inverse of the ideal of Jeffersonian democracy, in which citizens are smallholders and can maintain some freedom and independence of will by supplying their basic needs from their own industry on the land. Thus a degraded natural resource impacts the ability of a community—and by extension, a nation—to govern itself. The 1930s saw dislocation and bread lines, the mobs in the street, some people displaced from the land because of 10 years of agricultural depression, and others displaced from the Dust Bowl region as a direct result of soil degradation.

In the course of this study we have also seen that, in the context of growing population and more stable settlement patterns brought about by the closing of the frontier, the growth of socially-sponsored scientific and technical education opportunities and activities helped bring the turnaround in land management. The notoriously poor treatment of the soil resource in America before the 1800s constituted an exigency for only a few. By the early 1900s more voices were being raised about this issue, partly because of the science education offered by the land-grant colleges. The growth of science, of the Department of Agriculture, of the Experiment Stations, and of the Soil Survey in the late 1800s and early 1900s helped to bring about more awareness of the conditions of soils around the country. In this respect we saw that the purposeful direction

of resources by a society to understanding and developing its resources paid off, if sometimes indirectly, in encouraging the development of soil knowledge, logos, and a body of individuals able to read accurately what signs they saw on the landscape.

We also saw that this was true especially of those best prepared by experience as well as training to understand soil processes. Ethos and pathos played a part in soil science, especially among those whose family and community connections contributed to their selection of topics and goals in the study of soil science and the development of its logos. Ethics told them that this was something that would cause even greater damage and hardship in the future for others if this problem were not dealt with at its root, and the 1927 flood, along with many others, served as confirmation of this concern. Sympathy told them that the perpetrators of this evil were themselves so constrained as to be exhausting the soil out of their own desperation.

We also saw that soil erosion is an external, objective fact, not a negotiated one. Bennett, as an advocate for the legislation to bring about resource-saving action, did so as an individual who had had personal experience with the costs of erosion to family, community, and region. Did Bennett more nearly resemble Bitzer's, Vatz's or Consigny's model of the rhetor? Did he create the "reality" in the context of which his legislation was adopted? It may be objected to Vatz's model that many people saw and experienced loss from erosion, and were ready to "translate" reality virtually the same way that Bennett was: Soil, the lifegiving substance, was being lost. As Chamberlain said, "...the loss of the soil-body itself [is] a loss almost beyond repair. When our soils are gone, we too must go, unless we shall find some way to feed on raw rock or its equivalent." When people felt their way of life and basic survival threatened, they were willing to take concerted action because of what they perceived. If "intersubjectivity" names the convergence of opinion on the nature of a scientific fact, then the effects of the Depression, possibly the most significant factor of the rhetorical situation, helped legislators attend to Bennett's subject of emphasis, and see what he saw. The conditions of this rhetorical situation

brought the willingness of legislators to see what soil scientists saw, and to become activists on the part of their constituents.

Also, the development over more than a century of the rhetorical situation for soil conservation attests to the reality of "cultural lag," and means that for a potentially serious problem, damages may exceed society's ability to control them in time. When, after WWI, American farming mined the soil capital to a greater extent than ever before, there were well-informed observers who saw this as a rhetorical exigency—people who were conceptually and experientially prepared to interpret correctly what they saw. Bennett was the foremost among the mainly scientific observers who were concerned and who wanted to deal with this exigency, but only when the March 1935 dust storm put the evidence right before the eyes of the legislators was a firm commitment to the policy of conservation made. Looking at the enactment of soil conservation policy, it is inescapable that, in this event, the implementation of rhetorical recommendations followed rather than prevented crises, that is, much damage was already done when action was taken on the basis advocated.

In this study we have examined a rhetorical situation that was possibly a century in the making. Rhetors themselves were responding to their higher level of awareness of damages, and had been urging action from earlier in conservation exigencies when much greater damage could have been prevented. Conviction in the rhetorical audience, however, came only after greater destruction. Despite decades of work to convince Congressional colleagues of the value of preventive spending on the Mississippi, Newlands' recommendations were given serious consideration only after the disastrous 1927 flood, and many of the works promoted by Newlands were then finally implemented through the Soil Conservation Service by way of afforestation, and through the Small

Watersheds Protection Act in 1936. Bennett and Chapline's 1928 Bulletin emphasizing the offsite effects of erosion may have owed its warm reception to this circumstance more than to the strength of its reasoning and presentation, that is, the audience was moved by the fact of damage, not the exhortation to avoid future damage.

In "Empire of Dust," Paul Sears (1971) recorded how, from the opening of the western states up until the Dust Bowl years, scientists in Nebraska and Oklahoma protested forcefully and ineffectually to an Eastern-dominated Congress that the size of homestead allotments they set for the states west of the 100th parallel was, contrary to their presumption, far too small for sustainable farming. They had confidently warned of more frequent and more widespread "blow-outs," as did, in fact, inevitably occur. Legislation to address the man/land imbalance in the western states, although it too had been urged for decades, came only after the "ocular proof" of the dust storm during the Congressional hearing; it could certainly be argued that that legislation was no rhetorical success at all, but rather a scientific demonstration. Deliberative action came only after damage was done, despite years of scientifically sound conservation advocacy. The all-too-clear evidence of delay in addressing problems long recognized by the scientists in that field should concern us when we realize that there are not centuries but decades at most to react to some of the natural resource threats of the present, like DDT, chloroflourocarbons, and nuclear waste. Added to the speed with which these introduced threats may work is the fact that we don't even know what unintended effects many of them will have (Meadows *et al* 1972).

Erosion may soon be entirely a thing of the past, in the US, with no-till being increasingly implemented fully or in part and reducing the conditions contributing to erosion. Because of the availability of this, a *technical* solution to a problem that arose in part because of social conditions, many of the features that made erosion a rhetorical exigency no longer exist. Farms are now far larger and fewer, and their reliance on machinery is much greater than in the 1930s. Because of these factors, society is now able to assign to the farms the

responsibility for preventing erosion, and maintaining the soil base, as it has done through the 1972 Clean Water Act, and more recently with Cross Compliance. A wholesale shift in the demographic character of the US and a revolutionary change in the nature of farming has helped to resolve what was once a rhetorical situation. But this current balance depends on current farm ownership patterns and mechanical and technical supports. As we become a nation of fewer and fewer farmers, more of them corporate, we get farther away from the Jeffersonian ideal of the independent landowner, who has the freedom of action to be independent of will, farther from a society with roots and experience in nature, and closer to complete reliance on the systems that process and distribute farm products. It was in the era of many small farmers, subsistence farmers, that the worst erosion occurred, but it was also that era in which most people had some knowledge of farming, and in which the political will to correct erosion—and many other social ills—was aroused. New farm ownership patterns and new mechanical and technical supports may bring with them new problems, problems whose nature is as yet unknown. In part their rhetorical character will be determined by the legacy of Bennett and others described in this study, so we would do well to attend to the resources, natural and rhetorical, that they worked to secure.

BIBLIOGRAPHY

BIBLIOGRAPHY

- Adams, Fred, and Robert W. Pearson. (1967) "Crop Response to Lime in the Southern United States and Puerto Rico." Pp. 141-205 in *Soil Acidity and Liming*, Robert W. Pearson and Fred Adams, eds. No. 12, Agronomy Series. Madison, Wisconsin: American Society of Agronomy.
- Ammons, John T. (1993) Personal Communication. Plant Sciences, College of Agriculture, University of Tennessee, Knoxville TN, 14 January.
- Arakeri, H. R., and Roy Donahue. (1984) *Principles of Soil Conservation and Water Management*. Totowa, NJ: Rowan and Allanheld.
- Arieti, Silvano. (1976) *Creativity, The Magic Synthesis*. New York: Basic Books.
- Aristotle. (1984) *The Rhetoric and the Poetics of Aristotle*. W. Rhys Roberts, trans. New York: Modern Library.
- Ashe, W.W. (1908) *Terracing of Farm Lands*. North Carolina Geological and Economic Survey Bul. No. 17.
- . (1909) "The Waste From Soil Erosion in the South." *Review of Reviews*, 39: 439-443.
- Ayres, Quincy Claude. (1936) *Soil Erosion and Its Control*. New York: McGraw Hill Book Co.
- Bagley, William Chandler, Jr. (1942) *Soil Exhaustion and the Civil War*. Washington, DC: American Council on Public Affairs.
- Bailey, Reed W., and C.L. Forsling. (1934) *Floods and Accelerated Erosion in Northern Utah*. USDA Misc. Pub. No. 196. Washington DC: Government Printing Office.
- Barger, Harold, and Hans H. Landsberg. (1942) *American Agriculture, 1899-1939: A Study of Outputs, Employment and Productivity*. New York: National Bureau of Economic Research, Inc.

- Batie, Sandra. (1983) *Soil Erosion Crisis in America's Croplands?* Washington DC: The Conservation Foundation.
- . (1985) "Soil Conservation in the 1980s: A Historical Perspective." Pp. 5-21 in Douglas Helms and Susan Flader, eds. *The History of Soil and Water Conservation*. Washington: The Agricultural History Society.
- Baver, L.D. (1940) *Soil Physics*. New York: John Wiley and Sons.
- Bazerman, Charles. (1983) "Scientific Writing as a Social Act: A Review of the Literature of the Sociology of Science." *New Essays in Technical and Scientific Communication*, P. V. Anderson, R. J. Brockman, and C. R. Miller, eds. Farmingdale NY: Baywood.
- . (1991) "Second Stage in Writing Across the Curriculum." Review. *College English*, 53.2:209-212.
- . (1988) *Shaping Written Knowledge: The Genre and Activity of the Experimental Article in Science*. Madison, University of Wisconsin Press.
- Bazerman, Charles, and James Paradis, eds. (1991) *Textual Dynamics and the Professions: Historical and Contemporary Studies of Writing in Professional Communities*. Madison: University of Wisconsin Press.
- Bennett, Hugh Hammond. (1925) "Agriculture in Central America." *Jour. American Society of Agronomy*, 16.7: 318-326.
- . (1921) "The Classification of Forest and Farm Lands in the Southern States." Pp. 69-114 in the Proceedings of the 3rd Southern Forestry Congress. Atlanta, GA July 20-21.
- . (1942) "Conserving Soil and Water with Stubble Mulch." *Agricultural Engineering*, 23.2 (Feb.): 37-38.
- . (1955 [1947]) *Elements of Soil Conservation*. 2nd ed. New York: McGraw Hill.
- . (1926a) "Judging the Value of the Land." *Farm Journal*, (February): 34, 76-77.

- . (1959) *The Hugh Bennett Lectures*. Lectures delivered at North Carolina State College, January 1958-February 1959. Raleigh, NC: The Agricultural Foundation, Inc., North Carolina State College.⁷
- . (1927) "The Problem of Soil Erosion in the United States." Pp. 748-757 in *Proceedings of the First International Congress of Soil Science*, Vol 4.
- . (1926b) "The Real Reason for "Worn Out" Soil." *Farm Journal*, (September): 10, 46-47.
- . (1924) "The Relation of the Soil Survey to the Utilization of Southern Soils." *J. Am. Soc. Agron.*, 16.7 (July): 421-428.
- . (1928) *Soil Erosion a National Menace*. USDA Circular No. 33. Washington DC: Government Printing Office.
- . (1921) *Soils and Agriculture of the Southern States*. New York: The Macmillan Company.
- . (1924) "The Soils of Central America and Northern South America." Report to the Fifth Annual Meeting of the American Soil Survey Association, Chicago, Illinois. November 20-21. Vol 1.4: 69-81.
- . (1911) "Soils of the Prairie Regions of Alabama and Mississippi and their Use for Alfalfa." Part I: Houston Clay and Associated Soils. USDA Bureau of Soils and Bureau of Plant Industry Cooperating, Rpt. No. 96. Washington DC: Government Printing Office.
- . (1926) "Some Comparisons of the Properties of Humid-Tropical and Humid-Temperate American Soils; With Special Reference to Indicated Relations Between Chemical Composition and Physical Properties." *Soil Science*, 21.5: 349-374.
- . (1922) "Some Neglected Soil Investigations." Report of the Second Ann. Meeting of the Am. Soil Survey Assoc.

- . (1925) "Using the Soil Correctly." *Farm Journal*, (February): 10-50.
- Bennett, H.H., and W.E. McLendon. (1905) "Soil Survey of Louisa County, Virginia." Pp. 191-212 in the *Field Operations of the Bureau of Soils*, 1905. Washington DC: Government Printing Office.
- Bennett, H.H., and William Clayton Pryor. (1942) *This Land We Defend*. New York: Longmans, Green and Co.
- Bennett, H.H., and T. D. Rice. (1914) "Soil Reconnaissance of Alaska, with an Estimate of Agricultural Possibilities." USDA Bureau of Soils. Washington DC: Government Printing Office.
- Bennett, Hugh Hammond, Howard C. Smith, W.M. Spann, E.M. Jones, and A.L. Goodman. (1910) "Soil Survey of Lauderdale County, Mississippi." Pp. 733-784 in the *Field Operations of the Bureau of Soils*, 1910. Washington DC: Government Printing Office.
- Bidwell, Percy W., and John I Falconer. (1941) *History of Agriculture in the Northern United States: 1620-1860*. New York: Peter Smith.
- Bitzer, Lloyd F. (1968) "The Rhetorical Situation." *Philosophy and Rhetoric*, 1: 1-14.
- Blaxter, Kenneth, and Noel Robertson. (1995) *From Dearth to Plenty: The Modern Revolution in Food Production*. Cambridge: Cambridge University Press.
- Board, Paul. (1994) "The Aristocrat who Revolutionised Chemistry [Lavoisier]." *New Scientist*, Vol. 142, No. 1924 (7 May): 33-35.
- Böhme, Gernot, Wolfgang van den Daele, and Wolfgang Krohn. (1976) "Finalization in Science." *Sociologie de la Science*, 15 (2/3): 307-330.
- Booth, Wayne C. (1974) *Modern Dogma and the Rhetoric of Assent*. Chicago: University of Chicago Press.
- . (1963) "The Rhetorical Stance." *College Composition and Communication*, 14: 139-145.

- Brady, Nyle C. (1984) *The Nature and Properties of Soils*. New York: MacMillan Publishing Co.
- Bradley, John. (1993) Director, West Tennessee Agricultural Experiment Station, Milan TN. Personal Communication.
- Brink, Wellington. (1951) *Big Hugh, The Father of Soil Conservation*. New York: The MacMillan Co.
- Bruffee, Kenneth A. (1992) "Science in a Postmodern World." *Change*, 24.5 (Sept./Oct.): 18-25.
- . (1986) "Social Construction, Language, and the Authority of Knowledge: A Bibliographical Essay." *College English*, 48.8 (Dec.): 773-790.
- Buie, T.S. (1961) "Influence of Dr. H. H. Bennett on Soil Conservation in the U.S.A." SCS mimeo of talk given at Southeastern Section Meeting, Soil Conservation Society of America, during the Annual Meeting of Assn. of Southern Agricultural Workers, Jackson MS, February 16, 1961.
- Bunce, Arthur C. (1942) *The Economics of Soil Conservation*. Ames, Iowa: Iowa State College Press.
- Buol, Stanley W. (1996) William Neal Reynolds Professor of Soil Science and Graduate Alumni Distinguished Professor. North Carolina State University. Personal Communication.
- Buol, Stanley W., F.D. Hole, and R.J. McCracken. (1980) *Soil Genesis and Classification*. Ames: Iowa State University Press.
- Burges, Austin Earl. (1936) *Soil Erosion Control*. Atlanta: Turner E. Smith & Co.
- Burke, Kenneth. (1969) *A Grammar of Motives*. [1945] Berkeley: University of California Press.
- . (1969) *A Rhetoric of Motives*. Berkeley: University of California Press.

- Cameron, Frank K., and James M. Bell (1905) "The Mineral Constituent of the Soil Solution" USDA Bureau of Soils Bul. No. 30. Washington DC: Government Printing Office.
- Campbell, Charles P. (1990) "What Can Discourse Theory Offer the Professional Who Writes?" *IEEE Transactions on Professional Communications*, 33.4: 156-161.
- Capra, Fritjof. (1982) *The Turning Point*. London: Fontana.
- Carpenter, John W., Jr., and E.R. Gross. (1918) "The Terrace in Mississippi." Mississippi Agricultural and Mechanical College, Ext. Bul. No. 9.
- Carr, Earl, F.S. Welsh, G.A. Crabb, Risdin T. Allen, and W.C. Byers. (1911) "Soil Survey of Fairfield County, South Carolina." Pp. 479-511 in the *Field Operations of the Bureau of Soils*, 1911. Washington DC: Government Printing Office.
- Carter, Vernon Gill. (1949) *Man on the Landscape: The Fundamentals of Plant Conservation*. Washington, DC: National Wildlife Federation.
- Carter, Vernon Gill, and Tom Dale. (1974) *Topsoil and Civilization*. [1955] Rev. ed. Norman: University of Oklahoma Press.
- Cassidy, Dan and Rickert Althaus. (1994) "The Flood of 1993: The Economic Aftermath." *Choices*, First Quarter 1994: 29-32.
- Chamberlain, Thomas Crowder. (1909) "Soil Wastage." Pp 75-83 in Proceedings of a Conference of Governors in the White House, Washington D.C. May 13-15, 1908, W.J.McGee, ed. Washington DC: Government Printing Office.
- Chapman, Paul W., Frank W. Fitch, Jr., and Curry Lafayette Veatch. (1950) *Conserving Soil Resources: A Guide To Better Living*. Atlanta: Turner E. Smith & Co.
- Coleman, N.T., and Grant W. Thomas. (1967) "The Basic Chemistry of Soil Acidity." Pp. 1-39 in *Soil Acidity and Liming*, Robert W. Pearson and Fred Adams, eds. No. 12, Agronomy Series. Madison, Wisconsin: American Society of Agronomy.

- Conner, A.B., R.E. Dickson, and D. Scoates. (1930) *Erosion as a Factor Influencing Runoff and Soil Erosion*. TX. Exp. Sta. Bul. No. 411.
- Consigny, Scott. (1974) "Rhetoric and Its Situations." *Philosophy and Rhetoric* 7: 175-186.
- Cox, Joseph F., and Lyman E. Jackson. (1937) *Crop Management and Soil Conservation*. New York: John Wiley and Sons.
- Crosson, Pierre, and Anthony Stout. (1983) *Productivity Effects of Cropland Erosion in the United States*. Washington DC: Resources for the Future.
- Cutter, W.P. (1895) "A Pioneer in Agricultural Science." Pp. 493-502 in USDA *Yearbook of Agriculture*. Washington DC: Government Printing Office.
- Dana, Samuel T. (1916) "Farms, Forests and Erosion." Pp. 107-134 in USDA *Yearbook of Agriculture*. Washington DC: Government Printing Office.
- Davis, R.O.E. (1913) "Economic Waste From Soil Erosion." Pp. 207-220 in USDA *Yearbook of Agriculture*. Washington: Government Printing Office.
- de Man, Paul. (1978) "The Epistemology of Metaphor." Pp. 11-28 in *On Metaphor*, Sheldon Sacks, ed. Chicago: University of Chicago Press.
- Dictionary Catalogue of the National Agricultural Library*. ([current]) Washington DC: Government Printing Office.
- Duley, F.L. (1931) "Erosion as a Factor in Soil Deterioration." *Science* n.s., 73 no. 1882: 79-83.
- Duley, F.L., and O.E. Hays. (1932) "The Effect of the Degree of Slope on Run-off and Soil Erosion." *Journal of Agricultural Research*, 45.6 (Sept.): 349-360.
- Duley, F.L., and M.F. Miller. (1923) *Erosion and Surface Run-off Under Different Soil Conditions*. Mo. Agr. Expt. Sta. Research Bul. 63.
- Ede, Lisa S., and Andrea A. Lunsford. (1984) "On Distinctions Between Classical and Modern Rhetoric." Pp. 37-49 in *Essays on Classical Rhetoric and Modern*

Discourse, Robert J. Connors, Lisa S. Ede, and Andrea A. Lunsford, eds.
Carbondale: Southern Illinois University Press.

Elliott, F.F. (1937) "Economic Implications of the Agricultural Conservation Program."
Journal of Farm Economics, 19.1 (February): 13-27.

Fahnestock, Jeanne, and Marie Secor. (1985) "Toward a Modern Version of Stasis." Pp.
217-226 in *Oldspeak/Newspeak: Rhetorical Transformations*. Charles Kneupper,
ed. Arlington TX: Rhetoric Society of America.

Faigley, Lester. (1985) "Nonacademic Writing: The Social Perspective." In *Writing in
Nonacademic Settings*, Lee Odell and Dixie Goswami, eds. New York: Guilford
Press.

Faulkner, Edward H. (1943) *Plowman's Folly*. Norman OK: University of Oklahoma
Press.

---. (1947) *A Second Look*. Norman: University of Oklahoma Press.

Fisher, M.L. (1919) "The Washed Lands of Indiana: A Preliminary Study." Purdue Ag.
Exp. Sta. Circular No. 90. Lafayette, IN.

Fisher, Walter R. (1978) "Toward a Logic of Good Reasons." *Quarterly Journal of
Speech*, 64: 376-384.

Forsling, C.L. (1931) *A Study of the Influence of Herbaceous Plant Cover on Surface
Run-off and Soil Erosion in Relation to Grazing in the Wasatch Plateau in Utah*.
USDA Tech. Bul. 220. Washington DC: Government Printing Office.

Foss, Sonja K., Karen A. Foss, and Robert Trapp. (1991) *Contemporary Perspectives on
Rhetoric*. 2nd ed. Prospect Heights, IL: Waveland Press Inc.

Frawley, William. (1987) *Text and Epistemology*. Advances in Discourse Processes
Series. Norwood NJ: Ablex Publishing Corporation.

Frederickson, A.F. (1952) *Problems of Clay and Laterite Genesis*. Symposium at the
Ann. Mtg. of the Am. Inst. of Mining and Metallurgical Engineers, St. Louis, MO,

- Feb. 19-23, 1951. New York: American Institute of Mining and Metallurgical Engineers.
- Free, E.E. (1911) *The Movement of Soil Material by Wind*. USDA Bureau of Soils Bul. No. 68. Washington DC: Government Printing Office.
- Fuller, Steve. (1993) *Philosophy, Rhetoric, and the End of Knowledge: The Coming of Science and Technology Studies*. Madison: University of Wisconsin Press.
- Garin, Alexis N. and G. W. Forster. (1940) *Effect of Soil Erosion on the Costs of Public Water Supply in the North Carolina Piedmont*. Washington, DC: USDA SCS Economic Research Division, July.
- Gibbons, Boyd. (1984) "Do We Treat Our Soil Like Dirt?" *National Geographic*, 166.3 (Sept): 351-389.
- Graham, Edward H. (1944) *Natural Principles of Land Use*. London: Oxford University Press.
- Grant, Kenneth E. (1975) "Erosion in 1973-74: The Record and the Challenge." *Journal of Soil and Water Conservation*, 30.1 (Jan/Feb): 29-32.
- Grene, Marjorie. (1984) *Knower and the Known*. Washington DC: Center for Advanced Research in Phenomenology.
- Grimaldi, William M. A. (1972) *Studies in the Philosophy of Aristotle's Rhetoric*. Wiesbaden: Franz Steiner Verlag GMBH.
- . (1978) "Rhetoric and Truth: A Note on Aristotle." *Philosophy and Rhetoric*, 11.3 (Summer): 173-177.
- Gross, Alan G. (1990) *The Rhetoric of Science*. Cambridge MA: Harvard University Press.
- Guthrie, Richard. (1996) Dean of the Agricultural Experiment Station and Professor, Soils, Auburn University, Auburn AL. Personal Communication.

- Hall, A.D. and E. J. Russell. (1911) "Soil Surveys and Soil Analyses." *Journal of Agricultural Science*, 4.2 (October): 182-223.
- Hall, Arthur R. (1937) *Early American Erosion Control Practices in Virginia*. US Department of Agriculture Miscellaneous Pub. No. 256. Washington: Government Printing Office.
- Hallsworth, E.G. (1987) *Anatomy, Physiology and Psychology of Erosion*. International Federation of Institutes of Advanced Study Monograph No. 1. Chichester: John Wiley and Sons
- Halloran, S. Michael. (1978) "Technical Writing and the Rhetoric of Science." *Journal of Technical Writing and Communication*, 8 (2): 77-88.
- Hardin, Charles M. (1951) "Land or People?" *Land Economics*, 27: 133-142.
- Harris, R. A. (1990) "Assent, Dissent, and Rhetoric in Science." *Rhetoric Society Quarterly*, 20.1 (Winter): 13-37.
- . (1989) "Argumentation in Chomsky's Syntactic Structures: An Exercise in Rhetoric of Science." *Rhetoric Society Quarterly*, 19.2 (Spring): 105-130.
- . (1991) "Rhetoric of Science." *College English*, 53.3:282-307.
- Harrison, Robert W. (1961) *Alluvial Empire*. Little Rock AR: Pioneer Press.
- Haw, R.C. (1959) *The Conservation of Natural Resources*. London: Faber and Faber.
- Headley, J. C. (1985) "SCS and Cooperative Extension." Pp. 188-204 in Douglas Helms and Susan Flader, eds., *The History of Soil and Water Conservation*. Washington: The Agricultural History Society.
- Heisley, Marie. (1933) *Our Forests*. United States Department of Agriculture Misc. Pub. 162. Washington: GPO.
- Held, R. Burnell and Marion Clawson. (1965) *Soil Conservation in Perspective*. Baltimore: The Johns Hopkins Press for Resources for the Future.

- Helms, Douglas. (1996) National Historian, Economics and Social Sciences Division, Soil Conservation Service, USDA. Personal Communication.
- . (1992) *Readings in the History of the Soil Conservation Service*. Historical Notes No. 1, Economics and Social Sciences Division, USDA-SCS. Washington DC: Government Printing Office.
- Helms, Douglas, and Susan Flader. (1985) *The History of Soil and Water Conservation*. Washington: The Agricultural History Society.
- Helms, Douglas, and Ralph McCracken. (1994) *The Literature of Soil Science*. Ithaca NY: Cornell University Press.
- Hempel, Carl G. (1965) *Aspects of Scientific Explanation, and Other Essays in the Philosophy of Science*. New York: The Free Press.
- Hervey, Nigel. (1970) *A History of Farm Buildings in England and Wales*. Newton Abbot, Devon: David and Charles (Publishers) Ltd.
- Hilgard, E.W. (1904) "Chemistry of Soils as Related to Crop Production--Bureau of Soils Bulletin No. 22." Pp. 117-121 in Proc. 17th Annual Convention, Assoc. Am. Agric. Colleges and Exp. Stns., 1903. USDA Office of Exp Stns. Bull. 142. Washington DC: Government Printing Office.
- . (1906) *Soils, Their Formation, Properties, Composition, and Relations to Climate and Plant Growth*. New York: The Macmillan Co.
- Hill, James J. (1909) "The Natural Wealth of the Land and its Conservation." Pp 63-75 in Proceedings of a Conference of Governors in the White House, Washington D.C. May 13-15, 1908, W.J.McGee, ed. Washington DC: Government Printing Office.
- Hillel, Daniel. (1992) *Out of the Earth: Civilization and the Life of the Soil*. Berkeley CA: University of California Press.

- Hoffman, M.H., and A.W. Turner. (1920) *The Prevention of Soil Erosion*. "II. Checking Overfalls"; "III. Filling the Large Ditch"; "IV. The Adams Dam." Iowa State College of Agriculture and Mechanic Arts Extension Buls. No. 75-78.
- . (1921) *The Prevention of Soil Erosion* "V. The Use of Concrete." Iowa State College of Agriculture and Mechanic Arts Extension Bul. No. 80.
- Holmquest, Anne. (1983) "Rhetorical Argument in Science: The Function of Presumption." Pp 251-271 in *Argument in Transition: Proceedings of the Third Summer Conference on Argumentation*, D. Zarefsky, M. O. Sillars, and J. Rhodes, eds. Annandale VA: Speech Communication Association.
- Hopkins, C.G. (1904) "The Present State of Soil Investigations." Pp 95-104 in Proc. 17th Annual Convention, Assoc. Am. Agric. Colleges and Exp. Stns., 1903. USDA Office of Exp Stns. Bull. 142. Washington DC: Government Printing Office.
- . (1910) *Soil Fertility and Permanent Agriculture*. Boston: Ginn and Company.
- . (1912) *The Story of the Soil*. Boston: Badger Press.
- Horner, Winifred Bryan, ed. (1980) *The Present State of Scholarship in Historical and Contemporary Rhetoric*. Columbia: University of Missouri Press.
- Hurt, R.Douglas. (1985) "The National Grasslands: Origin and Development in the Dust Bowl." Pp. 144-157 in *The History of Soil and Water Conservation*, Douglas Helms and Susan Flader, eds. Washington: The Agricultural History Society.
- Hyams, Edward. (1952) *Soil and Civilization*. London and New York: Thames and Hudson.
- Ihde, A. (1969) "An Inquiry into the Origins of Hybrid Sciences: Astrophysics and Biochemistry." *J. of Chem. Ed.*, 46: 193-196.
- Jacks, G.V. (1962) "Man: the Fertility-Maker." *Journal of Soil and Water Conservation*, 17.4 (July-Aug):147-148, 176.
- . (1954) *Soil*. London: Thomas Nelson and Sons.

- Jacks, G. V., and R. O. Whyte. (1972) *Vanishing Lands: A World Survey of Soil Erosion*. New York: Doubleday, Doran & Co. Reprint. New York: Arno Press. [first published in London under the title of *The Rape of the Earth*.]
- Jacks, G.V., W.D. Brind, and Robert Smith. (1955) *Mulching*. Farnham Royal, Bucks England: Commonwealth Agricultural Bureaux.
- Jamieson, Kathleen M. Hall. (1983) "Generic Constraints and the Rhetorical Situation." *Philosophy and Rhetoric*, 6:162-170.
- Jenny, Hans. (1941) *Factors of Soil Formation: A System of Quantitative Pedology*. New York: McGraw-Hill.
- . (1961) *E. W. Hilgard and the Birth of Modern Soil Science*. Pisa: Colonna Della Rivista "Agrochimica."
- . (1980) *The Soil Resource: Origin and Behavior*. New York: Springer-Verlag.
- Joffe, Jacob S. (1949) *The ABC of Soils*. New Brunswick, NJ: Rutgers Pedology Publications.
- Johnson, Glenn L. (1986) *Research Methodology for Economists: Philosophy and Practice*. New York: Macmillan.
- Johnston, Elmer H. (ND [1935]) "Professor Dr. Curtis Fletcher Marbut." Pp. 29-35 in *The Life and Work of C.F. Marbut, Soil Scientist, A Memorial Volume*, H.H. Krusekopf, ed. Columbia MO: Soil Science Society of America.
- Johnstone, Henry W. (1980) "Rhetoric and Death." Pp. 61-70 in *Rhetoric in Transition: Studies in the Nature and Uses of Rhetoric*, Eugene White, ed. University Park, PN: Pennsylvania State University Press.
- Journet, Debra. (1990) "Writing, Rhetoric, and the Social Construction of Scientific Knowledge." *IEEE Transactions on Professional Communication*, 33.4:162-167.

- Kay, Jeanne. (1985) "Preconditions of Natural Resource Conservation." Pp. 22-33 in Douglas Helms and Susan Flader, eds. *The History of Soil and Water Conservation*. Washington: The Agricultural History Society.
- Kellogg, Charles E. (1941) *The Soils That Support Us: An Introduction to the Study of Soils and their Use by Men*. New York: The Macmillan Company.
- . (1957) "We Seek; We Learn." Pp. 1-10 in USDA *Yearbook of Agriculture*. Washington: Government Printing Office.
- Keller, Evelyn Fox. (1985) *Reflections on Gender and Science*. New Haven: Yale University Press.
- Kelley, Walter P. (1948) *Cation Exchange in Soils*. New York: Reinhold Publishing Co.
- Kelly, Lawrence C. (1985) "Anthropology in the Soil Conservation Service." Pp. 34-45 in *The History of Soil and Water Conservation*, Douglas Helms and Susan Flader, eds. Washington: The Agricultural History Society.
- Kennedy, George A. (1980) *Classic Rhetoric and its Christian and Secular Tradition from Ancient to Modern Times*. Chapel Hill: University of North Carolina Press.
- Kerridge, Eric. (1973) *The Farmers of Old England*. Totowa NJ: Rowman and Littlefield.
- King, F. H. (1911) "Productivity of Soils." *Science*, n.s., 33 No. 851(21 April): 614-619.
- . (1908) "Toxicity as a Factor in the Productive Capacity of Soils." *Science*, n.s., 28 No. 694 (17 April): 626-635.
- Kline, Ronald. (1995) "Construing 'Technology' as 'Applied Science': Public Rhetoric of Scientists and Engineers in the United States, 1880-1945." *Isis*, 86: 194-221.
- Klingbiel, A.A. (1991) "Development of Soil Survey Interpretations." *Soil Survey Horizons*, 32: 53-66.
- Knapp, Seaman Asahael. (1908) "Causes of Southern Rural Conditions and the Small Farm as an Important Remedy." Pp. 311-320 in USDA *Yearbook of Agriculture*. Washington: Government Printing Office.

- Knoblauch, H. C., E. M. Law, and W. P. Meyer. (1962) *State Agricultural Experiment Stations: A History of Research Policy and Procedure*. US Department of Agriculture Miscellaneous Publication 904. Washington, DC: Government Printing Office.
- Krohn, Wolfgang, and Wolf Schäfer. (1971) "The Origins and Structure of Agricultural Chemistry." Pp. 27-51 in G. Lemaine, R. McLeod, M. Mulkay, and P. Weingart, eds., *Perspectives on the Emergence of Scientific Disciplines*. The Hague: Mouton.
- Krupenikov, Igor A. (1992) *The History of Soil Science: From its Inception to the Present*. New Delhi: Amerind Pub. Co.
- Kuhn, Thomas. (1970) *The Structure of Scientific Revolutions*. 2nd ed. Chicago: University of Chicago Press.
- Lapham, Macy H. (1949) *Crisscross Trails: Narrative of a Soil Surveyor*. Berkeley CA: Willis E. Berg, Pub.
- Larson, W.E., F.J. Pierce, and R.H. Dowdy. (1985) "Loss in Long-Term Productivity from Soil Erosion in the United States." Pp. 262-271 in *Soil Erosion and Conservation*, S.A. El-Swaify, W.C. Moldenhauer, and Andrew Lo, eds. Ankeny IO: Soil Conservation Society of America.
- Lauer, Janice M. "Issues in Rhetorical Invention." Pp. 127-149 in *Essays on Classical Rhetoric and Modern Discourse*, Robert Connors, Lisa Ede, and Andrea Lunsford, eds. Carbondale: Southern Illinois University Press, 1984.
- Lefebvre, Karen Burke. (1987) *Invention as a Social Act*. Carbondale: Southern Illinois University Press.
- Leff, Michael C. (1983) "The Topics of Argumentative Invention in Latin Rhetorical Theory from Cicero to Boethius." *Rhetorica*, 1:23-44.

- Lehmann, E.W., and F.L. Duley. (1920) "The Mangum Terrace." Circular 98 of the Ag. Exp. Sta. of the Univ. of Missouri, Columbia.
- von Leibig, Justus. (1842) *Organic Chemistry in Its Application to Agriculture and Physiology*. London: Taylor and Walton.
- Lemaine, Gerard, Roy MacLeod, Michael Mulkay, and Peter Weingart. (1987) *Perspectives on the Emergence of Scientific Disciplines*. The Hague: Mouton.
- Little, Charles E. (1987) *Green Fields Forever: The Conservation Tillage Revolution in America*. Washington: Island Press.
- Long, Edward. (1993) "Raising the Status of Farmers." *Farmers Weekly*, 118.2 (8 January): 54-55.
- Long, R.A. (1909) "Forest Conservation." Pp. 83-95 in in *Proceedings of a Conference of Governors in the White House*, Washington D.C., May 13-15, 1908, W.J. McGee, ed. Washington DC: Government Printing Office.
- Lord, Russell. (1938) *To Hold This Soil*. USDA Miscellaneous Publication No. 321. Washington, DC: US Department of Agriculture.
- Lowdermilk, W.C. (1953) *Conquest of the Land Through 7,000 Years*. USDA Information Bul. No. 99. Washington, DC: US Department of Agriculture.
- . (1930) "Influence of Forest Litter on Run-Off, Percolation, and Erosion. *Journal of Forestry*, 28: 474-491.
- .(1937) "Soil Conservation Service: Discussion." *Journal of Farm Economics*, 19.1 (February): 40-45.
- Lynde, H.M. (1918) *Report on Runoff Investigations on Third Creek, Iredell County*. North Carolina Agr. Exp. Sta. 41st Ann. Rpt.
- Lyon, T. Littleton, and Elmer O. Fippin. (1913) *The Principles of Soil Management*. New York and London: The Macmillan Company.

- McCloskey, Donald N. (1989) "The Dismal Science and Mr. Burke: Economics as a Critical Theory." *The Legacy of Kenneth Burke*, Herbert W. Simons, ed. Madison: University of Wisconsin Press. 99-114.
- . (1987) "The Rhetoric of Economic Development." *Cato Journal*, 7.1: 249-254.
- . (1985) *The Rhetoric of Economics*. Madison: University of Wisconsin Press.
- . (1983) "The Rhetoric of Economics." *Journal of Economic Literature*, 21(June): 481-517.
- McGee, Michael C., and John S. Nelson. (1985) "*Homo Narrans*, Narrative Reason in Public Argument." *Journal of Communication*, 35 (Autumn): 139-155.
- McGee, W.J. (1897) "Sheetflood Erosion." *Bulletin of the Geological Society of America*, 8 (February): 87-112.
- . (1911) *Soil Erosion*. Bureau of Soils Bulletin No. 71. Washington: Government Printing Office.
- McHargue, J.S., and A.M. Peter. (1921) *The Removal of Mineral Plant Food by Natural Drainage Waters*. Kentucky Exp. Sta. Bul. 237.
- McHenry, Robert and Charles Van Doren, eds. (1972) *A Documentary History of Conservation in America*. New York: Praeger Publishers.
- McKeon, Richard. (1987) "The Development and the Significance of the Concept of Responsibility." [1957] Pp. 62-87 in *Freedom and History and Other Essays*, Zahava K. McKeon, ed. Chicago: University of Chicago Press.
- . (1987) "Philosophic Semantics and Philosophic Inquiry." [1966] Pp. 242-256 in *Freedom and History and Other Essays*, Zahava K. McKeon, ed. Chicago: University of Chicago Press.
- . (1987) "Philosophy and History in the Development of Human Rights." [1970] Pp. 37-61 in *Freedom and History and Other Essays*, Zahava K. McKeon, ed. Chicago: University of Chicago Press.

- . (1987) "A Philosopher Meditates on Discovery." [1952] Pp. 194-220 in *Rhetoric: Essays in Invention and Discovery*. Mark Backman, ed. Woodbridge CT: Ox Bow Press.
- . (1987) "Rhetoric in the Middle Ages." [1942] Pp. 121-166 in *Rhetoric: Essays in Invention and Discovery*. Mark Backman, ed. Woodbridge CT: Ox Bow Press.
- . (1987) "Symbolism Myths, and Arguments." [1955] Pp. 66-94 in *Rhetoric: Essays in Invention and Discovery*. Mark Backman, ed. Woodbridge CT: Ox Bow Press.
- . (1987) "The Uses of Rhetoric in a Technological Age: Architectonic Productive Arts." [1971] Pp. 1-24 in *Rhetoric: Essays in Invention and Discovery*. Mark Backman, ed. Woodbridge CT: Ox Bow Press.
- McVickar, Malcolm H. (1970) *Using Commercial Fertilizers: Commercial Fertilizers and Crop Production*. Danville IL: The Interstate Printers and Publishers, Inc.
- Marsh, George Perkins. (1865) *Man and Nature; or Physical Geography as Modified by Human Action*. New York: Charles Scribner. Rpt. Harvard University Press, 1965, David Lowenthal, ed.
- Maris, Paul V. (1950) "*the land is mine*": *From Tenancy to Family Farm Ownership*. Farmers Home Administration. USDA Agr. Mon. No. 8. Washington DC: Government Printing Office.
- Martin, Sanford. (1959) "And History is Already Shining on Him." *Better Crops with Plant Food*. Washington: American Potash Institute.
- Meadows, D.H., D.L. Meadows, Jørgen Randers, and William W. Behrens III. (1972) *The Limits to Growth: A Report for the Club of Rome's Project on the Predicament of Mankind*. London: Pan Books Ltd.
- Meyer, L. Donald, and William C. Moldenhauer. (1985) "Soil Erosion by Water: The Research Experience." Pp. 90-102 in Douglas Helms and Susan Flader, eds., *The*

History of Soil and Water Conservation. Washington: The Agricultural History Society.

Mickey, Karl B. (1945) *Man and the Soil: A Brief Introduction to the Study of Soil Conservation*. Chicago: International Harvester Company.

Middleton, H.E. (1930) *Properties of Soils Which Influence Soil Erosion*. USDA Tech. Bul. No. 178. Washington DC: Government Printing Office.

Middleton, H.E., C.S. Slater, and H.G. Byers. (1934) *The Physical and Chemical Characteristics of the Soils from the Erosion Experiment Stations*. USDA Tech. Bul. No. 430.

Miller, Carolyn R. (1980) "The *Ethos* of Science and the *Ethos* of Technology." Proceedings of the Technical Communications Sessions, 31st Conference on College Composition.

---. (1983) "Fields of Argument and Special *Topoi*." *Argument in Transition: Proceedings of the Third Summer Conference on Argumentation*, D. Zarefsky, M. O. Sillars, and J. Rhodes, eds. Annandale VA: Speech Communication Association. [pp]

---. (1984) "Genre as Social Action." *Quarterly Journal of Speech*, 70: 157-178.

---. (1979) "A Humanistic Rationale for Technical Writing." *College English*, 40.6: 610-617.

---. (1992) "Kairos in the Rhetoric of Science." Pp 310-327 in *A Rhetoric of Doing: Essays on Written Discourse in Honour of James L. Kinneavy*. Stephen Witte, Neil Nokadate, and Roger D. Cherry, eds. Carbondale: Southern Illinois University Press.

Miller, Carolyn, and Jack Selzer. (1985) "Special Topics of Argument in Engineering Reports." *Writing in Nonacademic Settings*, Lee Odell and Dixie Goswami, eds. New York: The Guilford Press. 309-341.

- Miller, M.F. (1949) "Progress of the Soil Survey of the United States Since 1899." *Soil Science Proceedings*, vol no: 1-4.
- Miller, M.F., and H.H. Krusekopf. (1932) *The Influence of Systems of Cropping and Methods of Culture on Surface Runoff and Soil Erosion*. Mo. Agr. Exp. Sta. Bul. No. 177.
- Mitchell, John. (1882) "The Soil of the State." Speech before the North Carolina Agricultural Society 1822. Rpt. in North Carolina Department of Agriculture Monthly Bulletin No 15.
- Mosier, J.G., and A.F. Gustafson. (1918) *Washing of Soils and Methods of Prevention*. University of Illinois Ag. Exp. Sta. Bul. No. 207.
- Muecke, Stephen. (1987) "Linguistics and Economics." Pp. 29-40 in *Papers in the History of Linguistics*. Hans Aarsleff, Louis G. Kelly, and Hans-Josef Niederehe, eds. Proceedings of the Third International Conference on the History of the Language Sciences. Princeton, NJ, 19-23 August 1984. Amsterdam: John Benjamins Publishing Company.
- Myers, Greg. (1986) "Writing Research and the Sociology of Scientific Knowledge: A Review of Three New Books." *College English*, 48.6: 595-610.
- Nelson, John, Allen Megill, and Donald McCloskey. (1985) *The Rhetoric of the Human Sciences*. Madison: University of Wisconsin Press.
- Nye, P.H., and P.B. Tinker. (1977) *Solute Movement in the Soil-Root System*. Oxford: Blackwell Scientific Publications.
- Odell, Lee, Dixie Goswami, A. Herrington, and D. Quick. (1983) "Studying Writing in Nonacademic Settings." Pp 17-40 in *New Essays in Technical and Scientific Communication: Research, Theory, Practice*, P.V. Anderson, R. J. Brockmann, and C. R. Miller, eds. Farmingdale NY: Baywood Publishing.

- Olsen, J.C. (1919) *Terracing in Texas*. Bul. No. B-51 of the Ext. Svc. of the Agr. and Mech. College of Texas, College Station, Texas.
- Ong, Walter, S.J. (1958) *Ramus, Method, and the Decay of Dialogue: From the Art of Discourse to the Art of Reason*. Cambridge MA: Harvard University Press.
- Overington, Michael A. (1977) "The Scientific Community as Audience: Toward a Rhetorical Analysis of Science." *Philosophy and Rhetoric*, 10.3 (Summer): 143-164.
- Owen, A.L. Riesch. (1983) *Conservation Under F.D.R.* New York: Praeger Publishers.
- Parks, W. Robert. (1952) *Soil Conservation Districts in Action*. Ames: The Iowa State College Press.
- Parsons, Kenneth H. (1991) "Henry Charles Taylor, 1873-1969, Organizer and First Head of USDA's BAE." *Choices*, 6.2:28-31.
- Paul, J.H., and F.S. Baker. (1925) *The Floods of 1923 in Northern Utah*. Utah Univ. Bul. 15 No. 3.
- Pera, Marcello. (1994) *The Discourses of Science*. Chicago: University of Chicago Press.
- Perelman, Chaim. (1979) "Authority, Ideology and Violence." [1969] Pp. 138-145 in *The New Rhetoric and the Humanities: Essays on Rhetoric and its Applications*. Dordrecht, Holland: D. Reidel Publishing Co.
- . (1979) "Dialectic and Dialogue." [1970] Pp. 73-81 in *The New Rhetoric and the Humanities: Essays on Rhetoric and its Applications*. Dordrecht, Holland: D. Reidel Publishing Co.
- . (1979) "The Philosophy of Pluralism." [1977a] Pp. 62-71 in *The New Rhetoric and the Humanities: Essays on Rhetoric and its Applications*. Dordrecht, Holland: D. Reidel Publishing Co.

- . (1979) "The Rational and the Reasonable." [1977b] Pp. 117-123 in *The New Rhetoric and the Humanities: Essays on Rhetoric and its Applications*. Dordrecht, Holland: D. Reidel Publishing Co.
- . (1968) "Rhetoric and Philosophy." *Philosophy and Rhetoric*, 1: 15-24.
- . (1982) *The Realm of Rhetoric*. William Kluback, trans. Notre Dame IN: University of Notre Dame Press.
- Person, H. S. (1936) *Little Waters: A Study of Headwater Streams and Other Little Waters, Their Use and Relations to the Land*. Rev. ed. Washington, DC: Rural Electrification Administration.
- Pfeiffer, Ehrenfried. (1947) *The Earth's Face and Human Destiny*. Emmaus, PA: Rodale Press.
- Prelli, Lawrence J. (1989) *A Rhetoric of Science: Inventing Scientific Discourse*. Columbia: University of South Carolina Press.
- . (1989) "The Rhetorical Construction of Scientific Ethos." *Rhetoric in the Human Sciences*, Herbert W. Simons, ed. London: Sage Publications. 48-68.
- "Prof Milton Whitney, Soil Scientist, Dead." (1927) *The Official Record*, United States Department of Agriculture, 6. 46 (November): 1, 5.
- Rasmussen, Wayne D. (1982) "History of Soil Conservation, Institutions and Incentives." Pp. 3-18 in *Soil Conservation Policies, Institutions, and Incentives*, Harold G. Halcrow, Earl O. Heady, and Melvin L. Cotner, eds. Ankeny, Iowa: Soil Conservation Society of America.
- Reisner, Marc. (1987) *Cadillac Desert: The American West and Its Disappearing Water*. New York: Penguin Books.
- Results of Erosion*. (1936) Agricultural Education Vol 12 Nos. 6-9, (Feb-May). School of Voc. Ed. Clemson: Clemson Agricultural College.

- Richards, Ivor A. (1971) *The Philosophy of Rhetoric*. [1936] Oxford: Oxford University Press.
- Ronan, Colin A. (1983) *The Cambridge Illustrated History of the World's Science*. Cambridge: Cambridge University Press.
- Rossiter, Margaret W. (1975) *The Emergence of Agricultural Science: Justus Leibig and the Americans, 1840-1880*. New Haven: Yale University Press.
- Roth, Filibert. (1901 "Grazing in the Forest Reserves." Pp. 333-348 in *USDA Yearbook of Agriculture*. Washington: Government Printing Office.
- Runte, Alfred. (1991) "National Forests, National Identity." *American Forests*, 97.5&6 (May/June): 22-24, 64-66.
- Russell, E. John, Sir. (1913) *The Fertility Of the Soil*. London: Cambridge University Press.
- . (1905) "The Recent Work in the American Soil Bureau." *Journal of Agricultural Science*, 1: 327-346.
- . (1957) *The World of the Soil*. London and Glasgow: Collins, The Fontana Library.
- Sachs, Carolyn E. (1983) *The Invisible Farmers: Women in Agricultural Production*. Totowa, NJ: Rowman and Allenheld.
- Sampson, A.W., and L.H. (1918) Weyl. *Range Restoration and its Relation to Erosion Control on Western Grazing Lands*. USDA Forest Service Bul. No. 675.
- Schickele, Rainer. (1937) "Tenure Problems and Research Needs in the Middle West." *Journal of Farm Economics*, 19.1 (February): 112-127.
- Schlieffer, Ronald. (1991) "Analogy and Example: Heisenberg, Negation, and the Language of Quantum Physics." *Criticism*, 23.3 (Summer): 285:307.
- Schultz, Theodore W. (1945) *Agriculture in an Unstable Economy*. New York: McGraw-Hill.

- Scott, Robert L. (1967) "On Viewing Rhetoric as Epistemic." *Central States Speech Journal*, 18 (February): 9-17.
- Sears, Paul B. (1935) *Deserts on the March*. Norman: University of Oklahoma Press.
(rev. ed., 1959).
- . (1969) *Lands Beyond the Forest*. Englewood Cliffs, NJ: Prentice Hall.
- . (1971) "An Empire of Dust." Pp. 2-15 in *Patient Earth*, John Harte and Robert H. Socolow, eds. New York: Holt, Rinehart and Winston.
- Shank, Michael H. And David Vampola. (1992) "Negating Positivism: Language and the Practice of Science" In *The Philosophy of Discourse: The Rhetorical Turn in Twentieth-Century Thought*, Chip Sills and George Jensen, eds. Portsmouth NH: Boynton-Cook. Pp. 22-52.
- Shaler, Nathaniel Southgate. (1891) *The Origin and Nature of Soils*. US Geological Survey Annual Report, 12, Part 1. Washington: Government Publishing Office.
- Sills, Chip and George H. Jensen. (1992) *The Philosophy of Discourse: The Rhetorical Turn in Twentieth-Century Thought*. Portsmouth NH: Boynton-Cook.
- Simms, D. Harper. (1970) *The Soil Conservation Service*. New York: Praeger Publishers.
- Simonson, R.W. (1986) "Historical Aspects of Soil Survey and Classification, Part I, 1899-1910." *Soil Survey Horizons*, 27: 3-11.
- Singer, Michael J., and Donald N. Munns. (1987) *Soils, An Introduction*. New York: Macmillan Publishing Co.
- Smith, Frank Ellis. (1966) *The Politics of Conservation*. New York: Pantheon.
- Smith, J.R. (1914) "Soil Erosion and Its Remedy by Terracing and Tree Planting." *Science* n.s. 39: 858-862.
- Soil Survey Staff, Bureau of Plant Industry. (1951) *Soil Survey Manual*. USDA Agriculture Handbook 18 [Rev. of USDA Misc Pub 274, Sept. 1937].

Agricultural Research Administration. Washington DC: Government Printing Office.

Spillman, W.J. (1910) *Soil Conservation*. USDA Farmers' Bulletin 406. Washington DC: Government Printing Office.

Stallings, J.H. (1957) *Soil Use and Improvement*. Englewood Cliffs NJ: Prentice-Hall, Inc.

Swain, Donald C. (1963) *Federal Conservation Policy—1921-1933*. University of California Publications in History Vol. 76. Berkeley: University of California Press.

Tandarich, John P. And Stephen W. Sprecher. (1994) The Intellectual Background for the Factors of Soil Formation. Pp 1-15 in *The Factors of Soil Formation: A Fiftieth Anniversary Retrospective.*, SSSA Special Publication No. 33. Madison, Wisconsin: Soil Science Society of America. 1-15.

Tanner, C. B., and R. W. Simonson. (1993) "Franklin Hiram King—Pioneer Scientist." *Soil Science Society of America Journal*, 57: 286-292.

Thomas, G. W. (1977) "Historical Developments in Soil Chemistry: Ion Exchange." *Soil Sci Soc Am J.*, 41:230-238.

Thompson, Louis M. (1952) *Soils and Soil Fertility*. New York: McGraw-Hill.

Torrens, Hugh. (1992) "When Did the Dinosaur Get its Name?" *New Scientist*, No 1815, 4 April: 40-44.

Toulmin, Stephen. (1990) *Cosmopolis: The Hidden Agenda of Modernity*. New York: Free Press.

---. (1972) *Human Understanding: The Collective Use and Evolution of Concepts*. Princeton NJ: Princeton University Press.

---. (1976) *Knowing and Acting: An Invitation to Philosophy*. New York: Macmillan Co.

- . (1988) "The Recovery of Practical Philosophy." *American Scholar*, 57. 3 (Summer): 337-352.
- . (1958) *The Uses of Argument*. Cambridge: Cambridge University Press.
- Toulmin, Stephen E., and June Goodfield. (1965) *The Discovery of Time*. New York: Harper and Row.
- Trimble, Stanley W. (1985) "Perspectives in the History of Soil Erosion Control in the Eastern United States." Pp. 60-78 in *The History of Soil and Water Conservation*, Douglas Helms and Susan Flader, eds. Washington: The Agricultural History Society.
- True, A.C. (1929) *A History of Agricultural Education and Research in the United States, 1785-1925, Including a History of the United States Department of Agriculture*. USDA Misc. Pub. No. 251. Washington: Government Printing Office.
- Turner, Frederick. (1990) "John Muir in the Appalachians" *Wilderness*, 55 (Fall): 28-40, 70-76.
- USDA: *Its Structure and Functions*. (1940) USDA Misc. Pub. No. 88. Washington: Government Printing Office.
- Vatz, Richard E. (1973) "The Myth of the Rhetorical Situation." *Philosophy and Rhetoric* 6: 154-161.
- Vaughn, Gerald F. (1996) "Leonard A. Salter, Jr.: Who Cares Whether America Has a Sound Land Policy?" *Choices*, Second Quarter: 34-36.
- Viets, Frank G., Jr. (1977) "A Perspective on Two Centuries of Progress in Soil Fertility and Plant Nutrition." *Soil Sci Soc Am J.*, 41: 242-250.
- Waddell, Craig. (1990) "The Role of *Pathos* in the Decision-Making Process: A Study in the Rhetoric of Science Policy." *Quarterly Journal of Speech*, 76: 381-400.
- Waksman, Selman A. (1938) *Humus: Origin, Chemical Composition, and Importance in Nature*. Baltimore: The Williams & Wilkins Co.

---. (1966) *Jacob G. Lipman: Agricultural Scientist, Humanitarian*. New Brunswick, NJ: Rutgers University Press.

Wallace, Karl R. (1974) "Bacon, Rhetoric, and Ornament of Words." Pp. 49-65 in *Rhetoric: A Tradition in Transition*, Walter R. Fisher, ed. Ann Arbor: Michigan State University.

---. (1963) "The Substance of Rhetoric: Good Reasons." *Quarterly Journal of Speech*, 49.3 (October): 239-249.

Warkentin, B.P. (1992) "Soil Science for Environmental Quality--How Do We Know What We Know?" *Journal of Environmental Quality*, 21: 163-166.

Washed Soils: How to Prevent and Reclaim Them. (1894) USDA Farmers' Bulletin No. 20. Washington: Government Printing Office.

Weaver, Richard M. (1970) "The Power of the Word." [1948] Pp. 33-55 in *Language is Sermonic: Richard M. Weaver on the Nature of Rhetoric*, Richard L. Johannesen, Rennard Strickland, and Ralph T. Eubanks, eds. Baton Rouge LA: Louisiana state University Press.

---. (1970) "Relativism and the Use of Language." [1961] Pp. 115-136 in *Language is Sermonic: Richard M. Weaver on the Nature of Rhetoric*, Richard L. Johannesen, Rennard Strickland, and Ralph T. Eubanks, eds. Baton Rouge LA: Louisiana state University Press.

Weber, Gustavus A. (1928) *The Bureau of Chemistry and Soils: Its History, Activities, and Organization*. Baltimore: The Johns Hopkins Press.

Wehrwein, George S. (1936) "Public and Private Aspects of Soil Conservation." *Soil Science Society of America, Proceedings*, 1: 447-452.

Whatley, Thomas J. (1994) *A History of the Tennessee Agricultural Experiment Station*. Knoxville TN: University of Tennessee Agricultural Experiment Station.

- Whitney, Milton. (1922) "The Depletion of Soils by Chemical Denudation." *Science* n.s., 56 No. 1443 (25 August): 216-218.
- . (1901) "Exhaustion and Abandonment of Soils." Testimony before the US Industrial Commission, March 12, 1901. USDA Rpt. No. 70. Washington DC: Government Printing Office.
- . (1904) "Extension and Practical Application of Soil Surveys." Pp 111-117 in Proc. 17th Annual Convention, Assoc. Am. Agric. Colleges and Exp. Stns., 1903. Exp Sta. Bul. 142. Washington DC: Government Printing Office.
- . (1921) "The Origin of Soil Colloids and the Reason for the Existence of this State of Matters." *Science* n.s., 54 No. 1409 (30 December): 653-658.
- . (1906) *Soil Fertility, An Address Delivered before the Rich Neck Farmers' Club, of Queen Anne County, Maryland*. USDA Farmers Bulletin 257. Washington DC: Government Printing Office.
- . (1892) *Soil Investigations*. Maryland Agr. Exp. Sta. Ann. Rpt. 1891: 249-296.
- . (1897) "Reasons for Cultivating the Soil" Pp. 123-130 in USDA *Yearbook of Agriculture*. Washington: Government Printing Office.
- . (1925) *Soil and Civilization: A Modern Concept of the Soil and the Historical Development of Agriculture*. London: Chapman and Hall Ltd.
- . (1899) "Soil Investigations in the United States." Pp. 335-346 in in USDA *Yearbook of Agriculture*. Washington: Government Printing Office.
- . (1909) *Soils of the United States, Based upon the Work of the Bureau of Soils to January 1, 1908*. USDA Bureau of Soils Bul. No. 55. Washington DC: Government Printing Office.
- Whitney, Milton, and Frank K. Cameron. (1903) *The Chemistry of the Soil as Related to Crop Production*. USDA Bureau of Soils Bul. 22. Washington DC: Government Printing Office.

- Wilding, L.D. (1994) "Factors of Soil Formation: Contributions to Pedology." Pp 15-30 in *The Factors of Soil Formation: A Fiftieth Anniversary Retrospective.*, SSSA Special Publication No. 33. Madison, Wisconsin: Soil Science Society of America.
- Wiley, H. W. (1899) "The Relation of Chemistry to the Progress of Agriculture." Pp. 201-258 in *USDA Yearbook of Agriculture*. Washington: Government Printing Office.
- . (1904) "Soil Fertility." Pp 142-146 in *Proc. 17th Annual Convention, Assoc. Am. Agric. Colleges and Exp. Stns.*, 1903. Exp. Sta. Bul. 142. Washington DC: Government Printing Office.
- Wilson, M.L. (1937) "Agricultural Conservation—An Aspect of Land Utilization." *Journal of Farm Economics*, 19.1 (February): 3-12.
- Woodward, J. (1699) Thoughts and experiments on vegetation. *Phil. Trans*, 21: 382-392.
- Woods, A.F. (1908) "The Wastes of the Farm." Pp. 195-216 in *USDA Yearbook of Agriculture*, Washington: Government Printing Office.
- Worboys, Michael. (1971) "The Emergence of Tropical Medicine: a Study in the Establishment of a Scientific Specialty. Pp. 75-97 in *Perspectives on the Emergence of Scientific Disciplines*, Gerard Lemaine, Roy MacLeod, Michael Mulkay, and Peter Weingart, eds. The Hague: Mouton.
- Yates, P. Lamartine. (1940) *Food Production in Western Europe*. London: Longmans, Green and Co.
- Yearbook of Agriculture* ([various years]) USDA. Washington: Government Printing Office.
- Zagacki, Kenneth S. and William Smith. (1992) "Rhetoric, Topoi, and Scientific Revolutions." *Philosophy and Rhetoric*, 25.1: 59-78.

Zon, Raphael. (1927) "Silviculture as a Factor in Maintaining the Fertility of Forest Soils." Pp. 575-582 in Proceedings of the First International Congress of Soil Science, Vol 4.

VITA

Elizabeth Lyman Stouse Weatherford grew up in Winston-Salem, North Carolina, and attended Salem Academy, adjacent to historical Old Salem, a community created by Moravians. Moravians are mentioned in this dissertation for their success in establishing a permanent community with a good land ethic, and strong, religious, ethical, and aesthetic traditions, all of which were impressed in the mind and heart of the Salem student. Her summers were spent in the North Carolina mountains, where she learned to love the land and the trees and wildlife in North Carolina. She graduated from Newcomb College in New Orleans Louisiana, with a BA in Political Science, and then began studying English Literature at Wake Forest University in Winston-Salem. She interrupted this pursuit to work on the field staff for Jimmy Carter's 1976 presidential campaign; she met her husband, James William Weatherford while working in Kentucky on the campaign. Two years, from 1978 to 1980, were spent as a Peace Corps Volunteer on Mindanao in the Philippines. Upon returning from the Philippines, Ms Weatherford continued study whilst market-gardening with her husband on a few acres of rich loess and alluvial soil in Western Kentucky, working as a tutor for Upward Bound, and caring for her two children born in Kentucky. It was at this time, growing intensive garden crops, living downhill from a careless row-crop farmer, and having seen horrendous erosion in the Philippines, that Ms Weatherford began to think about the ethics of landcare and their implications for sustainable communities. She continued her study, *in absentia*, eventually finishing a Master's Thesis on Chaucer's Clerk's Tale in 1986. She and her husband were in Lewis County Tennessee from 1984 to 1988. In 1987 she worked for the Gore presidential campaign. In 1988, she moved to Knoxville with their (then) three children to commence work on their PhDs. Elizabeth took classes mostly in non-literary areas in the English Department: in composition, rhetoric, teaching English as a Second Language (ESL), technical writing and editing, and linguistics. She also taught freshman composition, and ESL, and worked in the Writing Center. In addition, she served as technical editor for the Southern Journal of Agricultural Economics, and undertook free-lance editing work on numerous academic projects. In 1992 Ms Weatherford completed her comprehensive examinations and joined her husband in Ruston Louisiana, where she worked as a writing

consultant to the teaching staff of the Louisiana Center for the Blind, taught ESL at Louisiana Tech University, and organized and ran a telephone bank for Clinton and Gore in the 1992 presidential campaign. In 1993, having chosen as a dissertation project the connections between soil conservation and rhetoric, Ms Weatherford moved to Australia with her family. She then pursued her research and writing, again *in absentia*, and expanded her teaching experience into several new areas, namely teaching adult literacy, and teaching writing and communication for long-term unemployed youth. She set up a Writing Centre at Muresk Institute of Agriculture and ran the center for three years. Ms Weatherford's research went to some depth in soil science as well as historical aspects of the study of soil science as part of the background of the adoption of conservation policy. She completed her dissertation in July of 1997, returning to the US to complete the final work. Ms Weatherford expects to work in technical writing and editing, on documents dealing with natural resources, on her return to Australia.