DISSERTATION

ENERGY, STRUCTURE, SOIL AND SELF-REGULATION IN PLANT/SOIL SYSTEMS: A CONCEPTUAL MODEL

Submitted by

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WE HEREBY RECOMMEND THAT THE DISSERTATION PREPARED UNDER OUR SUPERVISION BY BRYCE F. PAYNE JR. ENTITLED ENERGY, STRUCTURE, SOIL, AND SELF-REGULATION IN PLANT-SOIL SYSTEMS: A CONCEPTUAL MODEL BE ACCEPTED AS FULFILLING IN PART REQUIREMENTS FOR THE DEGREE OF DOCTOR OF PHILOSOPHY.

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ABSTRACT OF DISSERTATION

ENERGY, STRUCTURE, SOIL, AND SELF-REGULATION IN PLANT/SOIL SYSTEMS: A CONCEPTUAL MODEL

A new concept is presented which suggests that in stable plant/soil systems, plants control the soil environmental factors that affect plant growth and the interactions among those factors by controlling system structure. The concept is based on the plant-control hypothesis and rhizocentric model of soil structural development. The plant-control hypothesis declares that in plant/soil systems energy is the primary resource, and structure an essential regulator of energy flows. The rhizocentric model of soil structural development in grass-dominated plant/soil systems describes the process which results in plant-control of soil structure, and, consequently, of energy and nutrient flows for such systems. In conjunction, the plant-control hypothesis and rhizocentric model form a conceptual model of control in plant/soil systems. The conceptual model may help explain the self-regulatory capabilities of stable plant/soil systems, and the causes of instability in some agricultural plant/soil systems. Examination of published data from various sources has revealed no case in which application of the conceptual control model did not result in logically consistent, reliable prediction of experimental outcomes, plausible interpretation of previously uninterpretable results, and often, formulation of testable new hypotheses. It is concluded that the control model -- and the plant-control hypothesis and rhizocentric model which it implies --

has enough credibility to merit further critical examination as a potentially useful conceptual tool for soil and agricultural science, biology, and ecology.

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No contribution to the completion of a major scientific or academic effort seems to be so often overlooked as the contribution made by one's spouse. No one has contributed more to the completion of this dissertation than has my wife, Angela. We came to Colorado State University prepared to accept the financial and emotional consequences of 2, or at most 4, years of post-graduate study. It has now been almost 10 years. During those years she has borne us two handsome, vibrantly healthy sons. She has seen to it that they and their older sister have received the parental support and loving supervision that every child needs, but that, sadly, too few receive. At immeasurable emotional cost to both of us she lived with a sacrifice of personal time so that I might not become a part-time father -- something that can happen all too easily when a man's attention is focused on his work or he becomes involved in the conflicts that his work may arouse. She has contributed in every material way to the completion of this effort, but more importantly to me, and though no one -- I think not even her -- has known, she provided assurance at the most critical moments of the defensibility of the ideas involved in and underlying this piece of work. She has stood by me year after year with no assurance that the effort would ever be completed, or, if completed, fruitful. If the test of time should show that the ideas presented in this dissertation are

indeed a "significant contribution", then it should be recognized as the contribution of that human whole known as Bryce and Angela Payne.

Without my children, Sigrid, Yani, and Lukas, I would never have started this race or been able to stay the course. Constant contact with them has helped sustain in me a faith in human potential and, often with their direct but unknowing assistance, in the powerful clarity of vision reserved to the innocent. I often fear for their future; that they might find themselves in a world where they cannot breathe clean air or find adequate supplies of wholesome food and water. They are for me the reason why the consideration and presentation of the ideas in this dissertation had to be unrelentingly pursued despite more obstacles and higher costs than I ever imagined one might encounter.

I must acknowledge the personal and financial support provided unquestioningly and continuously by my parents-in-law, Jose Torres and Nilza Lemos Menezes, despite the death of my father-in-law, and by my parents, Bryce and Mary Payne, on whom I have come to lean more and more heavily.

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this dissertation would not have been initiated or completed without the encouragement, counsel, professional courage and personally faithful support of Dr. Fred A. Norstadt.

I would like to thank my committee members whose patience I know I have tried and whose intellectual mettle I like to think I might have tested, at least a little -- for I know they have tested mine.

The Soil Plant Nutrient Research unit of the Agricultural Research Service of the U.S. Department of Agriculture provided financial support and its personnel gave freely of their time and the use of their equipment in support of my efforts.

Finally I am indebted to all those previous scientists and thinkers upon whose efforts mine are totally dependent, and some of whose concepts I have, at least by implication, attacked. If those attacks have been successful it is only because their efforts provided not only good targets but good ammunition as well.

DEDICATION

To the memory of my father-in-law, Jose Torres de Menezes, the first agronomist, and one of the finest and most important men I have had the honor and privilege of knowing.

SOIL, STRUCTURE, AND SELF-REGULATION IN PLANT/SOIL SYSTEMS: A CONCEPTUAL MODEL

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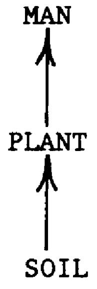
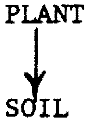
Chapter 1. INTRODUCTION

There are two sensorally obvious fundamental agricultural resources: plants and soils. The emergence within agronomy of crop science and soil science as distinct subdisciplines has developed along lines defined by the obvious separability of plants and soils. Within soil science this apparent separability has been the basis of even further specialization.

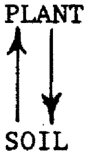
If a soil scientist is involved in production agriculture, then his orientation is toward study of the short-term physicochemical behavior of soils as it affects plant growth. The soil is regarded as a source of, or means for delivering, chemical and physical support to growing plants; functionally the soil is regarded as a bed of randomly arranged mineral, organic, and biological soil materials, which may be manipulated as necessary to meet production objectives. In the words of Buol et al. (1980), "Agriculturalists and industrialists may describe the soil as a machine, whose principal parts are aggregates and roots and which manufactures crops and livestock." This perception of plant/soil systems may be represented as:

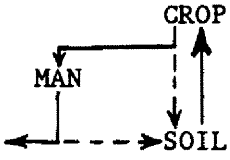


If, on the other hand, the soil scientist's primary concern is soil genesis, then his orientation is toward study of the physicochemical characteristics of soil that become apparent in the long term (decades to millenia) as the effects of biological and other soil forming factors. "Soil is a coincidence of materials and arrangements related to the 'factors of soil formation.'" (Buol et al., 1980). The perception of the plant/soil system from this perspective might be represented as:



Though both perceptions are valid and essential to the theoretical foundation of soil science, both imply that soils are "random" or "coincidental" arrangements of matter. Perhaps this underlying bias explains why there has been no concept capable of supporting a realistic description of the material function of either natural or agricultural plant/soil systems; i.e. none that has adequately considered these perceptions:

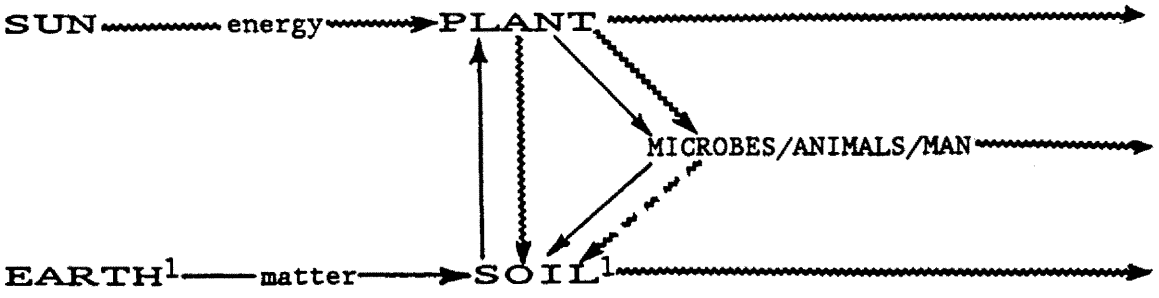




It is the objective of this dissertation to present such a concept.

The concept to be presented is an attempt to enable agricultural science to address plant/soil systems as wholistic entities, that is, as entities the functional parts of which are so physically numerous or functionally variable, and the relationships among the parts of which are so flexible and complex, that even apparently exhaustive enumeration of the parts and description of the relationships among them will not enable scientists to reliably predict plant/soil system behavior.

The concept is based on five fundamental premises and an assumption. (1) Plant/soil systems appear to be adaptive, i.e., plant/soil systems seem to be able to respond internally to external events in such a manner as to assure the continued existence of plant/soil systems. (2) Adaptiveness is an organismic property, i.e., a property of life. (3) Life requires <u>matter</u> which can be organized into living material, <u>energy</u> which can be used to accomplish such organization, and <u>knowledge</u> of at least some organizations of matter which, when accomplished, will have the attribute called life. (4) Among the life forms in plant/soil systems only plants are capable of accessing solar energy to support the biological transformation (organization) of matter -- essentially all other life forms in plant/soil systems are dependent upon the energy provided by plants. (5) For plant/soil systems, soil (the earth in a longer-term sense) can be considered the source of matter, the sun the source of energy, but the source of life-enabling knowledge is not so readily apparent. And the assumption: plant behavior is the best source of reliable knowledge about which organizations of matter, that is, which plant and plant/soil structures, are most effective at assuring their own continued existence, and, consequently, the existence of all forms of life energetically dependent on the sun and materially dependent on the soil. The development of ideas which comprises this dissertation is an effort to identify the logical consequences of these premises and assumption, that is, of this perception of plant/soil systems: and to determine whether or not those consequences are in accord with observations of the structure and function of real plant/soil systems; that is, whether or not the ideas presented merit further consideration as potentially useful conceptual tools for scientific study of plant/soil systems and whether the perception supporting them might serve as a unified conceptual foundation for soil and agricultural science.



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[1] This dissertation is an attempt to present a (set of) concept(s) which has interpretable implications for all the factors suggested by this diagrammatic representation, but the reader should not expect to find in the following brief presentation attempts to discuss all such implications. For example, in the earth environment the atmosphere serves as an avenue for the transfer of matter to plants, and its presence and role should not be ignored. However, the atmosphere is not included in this diagram and not specifically discussed in the following text for two reasons. First, the concepts to be presented below consider a plant/soil system as a localized, biological phenomenon existing with a biologically sensible structure and on a biologically sensible scale, while the atmosphere is considered a global phenomenon neither the structure nor scale of which need be biologically sensible. Any given plant/soil system is considered as adapted to, or in the

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process of adapting to, a relatively fixed range of recurring atmospheric phenomena, i.e. a certain climate. That is, given the existence of a stable plant/soil system, it is implicit that an atmosphere exists within which a certain climate occurs (on the site of the given plant/soil system). Second, the atmosphere is a much more fluid medium than soil; its physical structure is altered by non-biological phenomena on far shorter time scales and over a much wider range of spatial scales than is the physical structure of a soil or plant/soil system. Whether or not the structure of the atmosphere is biologically modifiable or controllable cannot be directly considered from the level of the concepts presented in this dissertation, the level of the individual plant/soil system. On the other hand, the composition of the atmosphere is biologically modifiable, perhaps controllable. Although biological modification of the composition of the atmosphere is not discussed in this dissertation, plant-control of soil structure could, according to the concepts presented, enable plants to control, for extended periods and over extended areas, the exposure of a major portion of the soil microbiota to air and energy-providing substrates, hence, to control the activity of the soil as a source and sink for many gases and, consequently, the composition of the atmosphere. Similarly, geological events of pedological interest, those defining parent material and topography for instance, occur on spatiotemporal scales beyond sensible consideration from the level of the individual plant/soil system. Consequently, although the concepts presented have implications for the within-system transformations of parent material and topography, the implications are not explicitly discussed in the following presentation.

Chapter 2. BACKGROUND AND PROCEDURE

The objective of this dissertation is to present a model[2] which might serve as a unified conceptual foundation for soil science. The objective of the model (as presented in this dissertation) is to replace a dichotomy of concepts of the plant/soil relationship, with a single, wholistic concept. The concept is based upon a systematic and, in so far as humanly possible, objective consideration of observations of the material behavior of soils and plant/soil systems. The generality required of the model, by precluding development of sufficient "new", "hard", experimental data to validate the model within the time frame of even an extended post-graduate study, made it necessary and appropriate to develop and test the model through analysis of the observations of other researchers.

Initially it was felt that the procedure used was a type of systems analysis. In practice the procedure was more generative than analytical. An investigation of system-analytical methods and general system theory revealed this disparity between the procedure used and systems-analytical approaches, as well as some inherent theoretical

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[2] More precisely, a hierarchy of three mutually dependent models, where, as throughout this dissertation, the term "model" should be understood to mean a descriptive conceptual framework, "a tentative ideational structure used as a testing device". It should also be stated that each such conceptual model, when valid, enables prediction of certain, specific behaviors of the modeled system, but not simulation of the behavior of the modeled system as a whole.

inadequacies in "systems" approaches. Further investigation into the principles of and relationships among language, logic, cognition, and science led to the conclusion that the procedure used was more clearly conceptually associated with Lesniewski's mereology, the logic of wholes and parts. (Some of the results of the investigation into the philosophical and logical principles mentioned in this paragraph and used throughout this chapter are considered in the Appendix. A critical overview of general system theory.) The principles encountered during that investigation reveal that any communicable model that is to be fully general with respect to a specific field of inquiry can be only qualitatively descriptive with respect to that field, and that qualitative models have certain characteristics. Some of the characteristics of qualitative models contrast sharply with those of the more traditionally scientifically acceptable quantitative models.

Qualitative models may be considered analog representations and quantitative models digital or numerical representations. Quantitative, numerical, or digital models are reducible, and consequently can be partially valid. Such models are called reducible because they contain compatible submodels; alteration, replacement, or removal of which may change the numerical value of predictions (output) the model produces, but will not cause a functional or logical collapse of the model as a whole. For example, one version of a given model might accurately (quantitatively) predict the behavior of some real system in 90% of the studied cases, while a reduced version, say, missing a submodel, in 60%, and both versions will be considered representations of the same real system.

Qualitative models, in contrast, are wholistic, not reducible, conceptual black boxes if you will, either wholly valid or wholly invalid. To clarify, valid qualitative models are not reducible because, even though they may contain submodels, no submodel can be removed or altered without causing a logical, hence functional, collapse of the model as a whole. This is so because qualitative predictions can be assigned only the logical values of "true" or "false"; that is, a qualitative model makes predictions that are either 100% accurate (qualitatively) or are simply wrong. There can be no partially accurate qualitative predictions, and consequently no partially valid qualitative models. In practice, whenever a qualitative model fails to accurately predict the qualitative behavior of a real system to which it is applicable, that qualitative model must be rejected as a whole, though valid submodels may be used to formulate other new testable models.

Valid qualitative models are essential to science, though their place is not generally recognized. A qualitative model developmentally precedes and functionally encompasses every quantitative model (again, the reader is referred to the Appendix for a consideration of the linguistic, logical, and cognitive principles supporting this statement). The development of valid qualitative models is difficult and slow (relative to the development of quantitative models in which partial validity can be tolerated). They generally appear suddenly, and remain forever likely to disappear just as suddenly as a consequence of a single predictive failure. The procedure used to develop the model presented in this dissertation focuses on these characteristics of qualitative models.

Procedure

The procedure used was in principle identical with a traditional scientific method: observe, attempt to explain what was observed, test the explanation, if the explanation fails the test reformulate it, or if it passes, formulate an explanation for other observations. However, the objective of the effort behind this dissertation was to develop a general, qualitative representation of soil structure and later, by implication, control in plant/soil systems.

Because of its qualitative objective the procedural application of the scientific method was subject to two constraints not encountered, or at least not dealt with, in quantitative scientific studies. First, generality was a required characteristic of the model under study. That generality precluded experimental generation of sufficient data to clearly support or refute the model. Consequently, it was necessary to turn to the literature for sufficient data from sufficiently different situations so that testing of the model could be considered to demonstrate a general validity. Second, a qualitative model can be refuted by a single, unexplainable, valid observation of a real system to which the model is applicable. Therefore, in order to assure valid testing of the model, it was necessary to select from the literature only data or observations from or about studies to which the model was retrospectively applicable.

A three-step selection process was used in order to safeguard against selecting for consideration only those studies the results of which would support the model, and at the same time to assure that the model would not be improperly refuted by an attempt to explain observations of a study to which the model was not applicable. The first step was to (i) determine whether the model had any general applicability to the study under consideration as a potential data source. For example, the model would not be considered applicable to a study of the differences in inter-varietal effects of a foliar application of a pesticide on corn yields, while it might or might not be considered applicable to a study of the relationship of soil type to the effects of pesticide residues on corn yields. Next, when general applicability was apparent, as for example, to a study of long-term effects of different tillage practices on crop yields, (ii) applicability to a specific study was determined on the basis of whether the report of the study allowed an adequate evaluation of the variables required by the model. If the model was considered applicable to a particular study on the basis of the general and specific subjects of that study, then (iii) the procedure and data reported were examined to assure their technical validity. No study which was found to meet these selection criteria could be eliminated from consideration because its data did not support the model.

The procedure used may be summarized, then, as follows:

1. A conceptual model is proposed.
2. Each study to be used for testing the model is selected on the basis of:
    - (i) General relevance of the model to the subject of study.
    - (ii) Compatibility of the study with the data requirements of the model.
    - (iii) Technical reliability of the data reported.
3. The model is given the experimental conditions and any other relevant, reliable information available.
4. The predictions of the model are compared to the results reported for the study.

No procedure is offered for the original formulation of the models presented, since the means by which they became apparent to the author are unknown except to the extent that they are described in the discussion of philosophic and logical principles presented in the Appendix. Those principles suggest that the orderliness (and beauty) of the natural universe, and the phenomena that occur within it, is not a measurable object, but a perceivable quality; that perception of this quality is a primitive, biological, adaptively advantageous, cognitive function; and that there is no substitute for patient and humble cogitation in the pursuit of an understanding of the laws of nature and how they might be wisely applied to the benefit of humankind.

A word regarding the organization and content of the following chapters. Self-regulatory control in plant/soil systems is modeled (chapter 5) as the result of the simultaneous application of the model of biological control of energy use in plant/soil systems (chapter 3, the plant-control hypothesis) and a model of soil structural development (chapter 4, the rhizocentric model). The order of presentation is not intended as a representation of the chronological or developmental order of the models, but only as a communicatively effective organization of the concepts. There is a hierarchical functional relationship among the models, the soil structure model within the biological control model which is within the composite control (self-regulation) model. Adequate input to a lower level model(s) produces specific qualitative predictions about higher level(s), while input to a higher level can produce only general predictions with respect to a lower level. Each model can be used to produce quantitative predictions within the same level, given appropriate quantitative input.

Chapter 3. ENERGY, STABILITY, AND CONTROL IN PLANT/SOIL SYSTEMS: THE PLANT-CONTROL HYPOTHESIS

INTRODUCTION

G. V. Jacks (1963) wrote, "It is commonly agreed that the so-called climax plant association with its associated fauna, in equilibrium with the climate, is the social organism which makes the fullest use of the environment; the plants and animals have made the best possible living conditions for themselves; and the productivity of the soil is then the highest possible under the prevailing conditions."

Jacks used the terms <u>fertility</u> and <u>productivity</u> interchangeably, concluding "...soil fertility is a biophysical rather than a physicochemical phenomenon." Following Jacks' lead, Cooke (1967, p. xi) wrote, "The fertility of soil undisturbed by man is its capacity to support the climax population of plants and animals above ground and the associated flora and fauna below ground. When taken over for agriculture, the fertility of the soil becomes its capacity to produce the crops desired...The inevitable result of farming is always to diminish natural fertility, because portions of the total supply of plant nutrients, and of the organic compounds made with the aid of the energy of sunshine, are removed. In undisturbed communities these would be returned to the soil, to be used again as food for plants and animals, so maintaining or increasing fertility."

Production agriculture has had little or no concern with an ecological definition of soil fertility/productivity until recently. As long as arable land or fertilizer and fuel were plentiful and cheap, a loss in "natural fertility" had no immediate practical importance. Now the situation has changed: More than 10% of the earth's land surface is presently under cultivation, and most of the remaining potentially arable land is marginal (Cox and Atkins, 1979, p. 12-18; Larson, 1986). Fuel and fertilizer supplies are unreliable in many areas of the world. The need to develop and adopt farming practices that do not inevitably diminish soil productivity -- the need to develop a "sustainable agriculture" -- is now widely recognized (USDA-ARS, 1983). Efforts to develop a "sustainable agriculture" will likely be benefited by every insight into how stable ecosystems maintain fertility/productivity. This chapter presents the "plant-control" hypothesis which suggests that plants induce biological maintenance of fertility/productivity by biophysically controlling production <u>and</u> <u>decomposition</u> processes.

SOIL FERTILITY/PRODUCTIVITY: ECOLOGICAL CONSIDERATIONS

Several writers remark about a lack of the knowledge necessary to develop a practical ecological definition of soil fertility/productivity. One predicted that the secret of soil fertility will be revealed only when the ways of life of many kinds of bacteria and other microscopic organisms of the soil are known. "Crops, soil, and soil microorganisms must need be investigated simultaneously: a great task fraught with great issues for the welfare of mankind" (Keeble, 1932, p. 145-146). Cox and Atkins (1979, p. 219) stated that soil is a...portion of a terrestrial ecosystem...an ecosystem in its own right...the complexity and variability of which are extraordinary. Since many of its important functions take place among microscopic organisms within a dense, opaque matrix, it is one of the most difficult of ecosystems to study, and as yet we have but meager knowledge of its structure and dynamics. Both writers mention "microscopic organisms", reflecting the essential role of soil microbes in soil fertility/ productivity.

ENERGY AND CONTROL OF NUTRIENTS IN STABLE ECOSYSTEMS

I begin this ecologically oriented discussion of soils and plant/soil ecosystem fertility by considering certain differences and similarities, and the nature of a possible biological "common thread" between two ecosystems that often appear, especially for agricultural purposes, to present two environmental extremes: the tropical rain forest and the temperate semiarid grassland.

The Rain Forest: An Ecosystem in One Environment

In the rain forest most biological activity occurs outside the mineral soil. This limited role of the mineral soil facilitates discussion of three ideas: (i) Biomass and detritus are the most biologically effective means of retaining nutrients in an ecosystem. (ii) The ability of the soil to support the plant community, i.e., its fertility, is its ability to <u>retain</u>, not just contain, effectively plant-available nutrients. This ability increases proportionally as the detrital food web extends into the soil. (iii) Successful retention of nutrients by an ecosystem depends on the coordination of production and decomposition processes. The plant community controls energy and, consequently, decomposition in and nutrient release from the detrital food web.

Nutrient Retention in Biomass and Detritus

Although recent studies have shown nutrients may be lost by volatilization (Morgan et al., 1985; O'Deen and Porter, 1986) and leaching from living plants, these losses are, especially in stable natural systems, much smaller than might be expected if the nutrients were in the soil (Vervelde, 1978). Tropical rain forest ecosystems depend on plant/microbial symbioses that intercept nutrients prior to release from detritus. These relations assure that the nutrient supply is essentially all in plant biomass, detritus, or in the microbial biomass of microorganisms under energetic control of the plants.

Productivity and Soil Fertility

Just how much the fertility of the soil ecosystem affects the productivity of the entire host terrestrial ecosystem is variable, particularly when there is a question of what constitutes the soil. Generally, soil is defined as the uppermost portion of the crust of the earth on and within which resides the terrestrial plant and animal populations of an ecosystem. Often, especially in an agricultural context, surface layers of accumulated organic litter and detrital materials are not considered a functional part of the soil. This can be a troublesome exclusion (van Wambeke, 1978).

For example, try to reconcile ecological descriptions of soil fertility/productivity with the above soil definition. Cox and Atkins (1979, p. 247) say that the fertility of the soil ecosystem is determined by how well it retains water and nutrients in forms readily available to plants. Cooke (1967, p. xi) holds that the fertility of soil undisturbed by man is its capacity to support the climax population of plants with the associated fauna and microflora. In tropical rain forests the underlying mineral soils do, indeed, support the climax population. Many studies have shown, however, that when the litter layer is removed, these soils fail to retain nutrients and/or water to support a climax plant population (Herrera et al., 1978; van Wambeke, 1978). Rather, the plant-available nutrients are stored within the standing plant biomass and the detritus and decomposer biomass which occur outside the mineral soil. Plant-fungal symbioses (mycorrhizae) are very common and the host plant exploits directly, via the decomposing activities of the symbiont fungus, the nutrients in recently fallen litter. Such relationships protect the nutrient resources essential to the long-term survival of the tropical rain forest ecosystem by preventing release of nutrients to the mineral soil (Herrera et al., 1978; Stark, 1971; van Wambeke, 1978).

The Detrital Food Web and Soil Fertility

Retention and recycling of nutrients within an ecosystem are essential to its survival. That the detrital food web is effective for nutrient retention and recycling is well-established (Thompson, 1952, p. 42-49; Vervelde, 1978). Though the rain forest ecosystem is productive, the soil is not fertile, because the detrital food web is outside the mineral soil. The latter, consequently, has minimal retentive capacity for plant-available forms of nutrients and moisture (van Wambeke, 1978). In the rain forest ecosystem, the most important role of the mineral soil is not to store nutrients or shelter sensitive organisms, but provide a physical foundation that can accept and drain away large amounts of rainfall without incurring destabilizing soil losses (de Mooy, 1981, personal communication; Unger and McCalla, 1980).

Coordination of Production and Decomposition

All biological activity in stable terrestrial ecosystems depends on the energy provided by plant production (Cox and Atkins, 1979, p. 40; Jacks, 1963; Thompson, 1952, p. 42). On the other hand, the nutrients that permit production depend on decomposition by microbes. Coordination of these complementary processes, production and decomposition, is inherent in the rain forest ecosystem for two reasons. First, but perhaps less importantly, producers and decomposers share a common environment, outside the soil. When environmental conditions limit production they likely limit decomposition as well. Second, the primary decomposers and producers are directly physiologically linked in mycorrhizal and other symbiotic associations which dominate plant-microbe relations in the rain forest. Because of the symbiotic connections, plant activity throttles microbial activity; hence, production and decomposition are coordinated.

The biota of tropical rain forest ecosystems meet the constraints imposed by a constantly warm, high rainfall environment. Analogously, the biotas of other ecosystems must meet the constraints of their particular environments, some where moisture and temperature vary widely with weather and season.

The Temperate Grassland: An Ecosystem in Two Environments

The temperate grasslands are among the most agriculturally important ecosystems (Jackson, 1984). Temperate grassland soils, unlike those of tropical rain forests, support a biota adapted to an environment where both cold and lack of moisture constrain plant activity for extended periods. The ecosystem's need for a stable foundation is coupled with a need for shelter. As in the rain forest, stability requires coordination of production and decomposition. However, the factors permitting passive coordination in the rain forest are not effective in the grassland ecosystem.

Soil and the Need for Shelter

In grasslands many decomposer microorganisms depend on the soil for protection, their activity above-ground being severely limited (Woodmansee, 1984). In fact, most microorganisms survive in grassland ecosystems within the dense, opaque matrix of the mineral soil, partially protected from lethal effects of the wet/dry, freeze/thaw cycles characteristic of the above-ground environment. Perennial plants survive similarly protected within the soil body. Such plants invest relatively large amounts of energy to develop and maintain extensive, durable root systems that can survive winter cold or extended drought. Also, such root systems conserve nitrogen (N) by retaining and recycling N within the living plant biomass, and they permit rapid, effective response to major or minor rainfall events (Clark, 1977; Woodmansee, 1984). Even many of the consumers of the grassland ecosystems, various rodents and insects, seek refuge from the "elements" by residing within the soil. The detrital food web, then, is mostly limited to operate within the confines of the protective soil environment.

Coordination of Production and Decomposition

Not a result of environmental coincidences -- The producers provide inputs of new available energy when environmental conditions both above and within the soil are favorable for plant growth. However, microbial activities in most of the detrital food web are determined only by the environmental conditions within the soil. Further, soil microbes are not so fastidious in their environmental requirements as plant roots. Microbial decomposition of plant residues has been found to occur at moisture levels well below those at which root activity ceases (Bartholomew and Norman, 1946). Frequently, then, below-ground conditions permit microbial activity when environmental conditions prevent plant activity. Decomposition at such times releases nutrients that may accumulate and become subject to leaching or volatilization. When such losses exceed nutrient inputs, productivity is reduced, eventually threatening the survival of the biota and the ecosystem.

In laboratory studies microbial communities decompose and release the nutrients from most of the organic materials found in grassland soils rather rapidly. However, field studies show that volatilization and leaching losses are minimal for grassland ecosystems (Clark, 1977; Woodmansee, 1978). Organic matter of intact soil, then, must release nutrients at much slower rates than predicted from results of laboratory studies. Such information and the decrease of organic matter in soils under cultivation, have led to the generalization that decomposition is relatively slow in intact grassland soils.

It has been suggested that slowed but prolonged decomposition, induced by relatively cool soil temperatures and higher moisture levels caused by vegetative and litter cover, reduce losses of released nutrients (Woodmansee, 1984). Transpiration, however, always reduces soil moisture, and at least one study has shown that decomposition in grasslands is more closely related to moisture availability than temperature (de Jong, 1981). The results of a carbon-14 (^{14}C)

laboratory study of soil respiration under blue grama (Bouteloua gracilis (H.B.K.) Lag.) also indicated temperature was not as restrictive of microbial activity as might be expected (Dormaar and Sauerbeck, 1983) (Table 3.1). Evolution of ^{14}C-labelled carbon dioxide ($^{14}CO_2$) was measurable throughout a simulated winter even though the soil eventually froze.

Table 3.1. Redistribution over three simulated seasons of ^{14}C activity photosynthetically fixed during the first simulated summer and translocated to roots by blue grama [Bouteloua gracilis (H.B.K.) Lag.] plants . Data from Dormaar and Sauerbeck (1983).

Location of fixed carbon	SIMULATED SEASON FIRST SUMMER[1]	 FALL/WINTER	 SECOND SUMMER[2]
	(% of ^{14}C activity)		
Roots	41	16	16
Soil	27	48	44 to <48
CO_2 evolved from soil	31	4	0 to <4

[1] All ^{14}C labelling occurred during the first summer through photosynthetic fixation of ^{14}C from $^{14}CO_2$ in the above-ground atmosphere.

[2] Ranges are reported for soil and CO_2 because of the unknown distribution of ^{14}C activity retained in shoots over winter and translocated below-ground during the second summer.

Further, mass balances on carbon (C) suggest decomposition in grasslands is not slow. One such mass balance was prepared for a Canadian grassland soil (data from van Veen and Paul, 1981) by assuming grassland soils are in a steady state with respect to organic C and using an estimate of the rate of loss of organic carbon from cultivated Canadian grassland soils (de Jong, 1981). Decomposition of root residues in native prairie (1300 kg-C ha^{-1} yr^{-1}) was only 64 kg-C ha^{-1} yr^{-1} less than total decomposition apparent for a parallel cultivated soil (calculated as 1104 kg-C ha^{-1} yr^{-1} crop residues plus 260 kg-C ha^{-1} yr^{-1} in organic matter losses -- 260 kg-C ha^{-1} yr^{-1} is likely an overestimate of losses due to decomposition since some of this is probably erosion loss). If only 6.4% of the grassland above-ground residue (1000 kg-C ha^{-1} yr^{-1}) were considered as input to the soil, then the decomposition rate would equal that of the cultivated soil. In fact, it was estimated that 50% of the grassland above-ground residue was input to the soil. Clearly in this case, decomposition in grassland soil was <u>not</u> <u>less</u> than in cultivated soil.

Additionally, a crop-fallow management system causes higher soil temperatures in periods of adequate moisture (no transpiration losses during fallow) than occur in grassland soil. The decomposition rate during the less frequent periods of activity in the grassland must have exceeded the rates in the presumably more-favored cultivated soil. Otherwise, annual decomposition in the grassland could not equal or exceed that of the cultivated soil. Again, it follows that microbial activity (decomposition) is higher in the presence of active plants than when conditions appear to favor microbial activity but plants are inactive or absent. Thus, examination of the environmental conditions required by plants and microbes and the two environments in grassland ecosystems reveals, but does not explain, the coordination of production and decomposition.

<u>Not a Result of Symbiotic Dependencies</u> -- Most plant and microbial activities are symbiotically linked in rain forest ecosystems. Mycorrhizae, essential in the rain forest detrital food web, apparently are less important in grassland soils. Mycorrhizal infection may be an adaptation to environmental constraints, but once the fungal symbiont is

established, supporting it apparently is obligate for the host plant. The symbiosis is an energy expense that is not always compensated by greater nutrient availability for the host plant (Alexander, 1977).

In a tropical rain forest, where photosynthetic energy supplies to roots are less constrained, spending energy to improve nutrient availability would be advantageous. But, mycorrhizae apparently do not improve nutrient availability to plants in relatively fertile soils, and plant activity in temperate grasslands suffers lengthy interruptions. During such interruptions, supporting the fungus would reduce energy reserves for root maintenance and initiation of new shoots when environmental conditions improve. These factors may explain the questionable importance of mycorrhizae in productive grasslands and other fertile soils (Alexander, 1977, p. 71; Focht and Martin, 1979). Legumes and the rhizobial symbiosis are often of minor importance in grassland ecosystems (Jackson, 1984). Thus, symbiotic plant/microbe relationships cannot adequately explain the coordination of plant and microbial activity in stable, productive grasslands.

Energy and Plant-Control of Microbial Activity -- The tight coupling of plant and microbial activity in the detrital food web of grassland ecosystems appears an enigma. Often, conditions favor microbial rather than plant activity yet decomposition rates indicated by nutrient losses seem low during these periods. Decomposition rates, however, must be quite high when plants are active to achieve the required high annual turnover. There is no direct physiological link between the microbial decomposers and plants, nor a sharing by these two groups of the above-ground environment in grasslands -- the linking factors apparently important in rain forests.

The inadequacy of the "competition" hypothesis that explains the coupling of plant and microbial activity in grasslands as due to the simultaneous occurrence of plant-favorable and microbe-favorable environmental conditions, has already been discussed. (The reasons for so naming this hypothesis will become clear.) The problem with this hypothesis is not obvious theoretical inadequacies, but an assumption that experimental results obtained from agriculturally or experimentally disturbed soils can be reasonably extrapolated to undisturbed soils. Plants are left out of most studies of soil microbial activities, and when plants have been present, it has almost always been in disturbed soils: plowed, or sieved for greenhouse or laboratory studies. Interpretation of results from such experiments gave rise to the adage, "Microbes are first to the table" with respect to uptake of nutrients. That is, soil organisms usually extract their nutrient quota first and higher plants must subsist on what remains available (Brady, 1974, p. 132). Competition for limited nutrients is the central concept -- a correct interpretation when adequate available energy substrate makes soil microbes much better competitors for nutrients than plants. Competition, however, requires that the competitors be functionally independent. The functional independence of plants and soil microbes, correct for disturbed soils, should not be presumed correct for undisturbed soils, especially those of climax ecosystems.

Consider again the tropical rain forest where plants exercise much control over microbial activities through a direct microbial dependence on them for energy. The key to the remarkably-effective nutrient conservation of this ecosystem is the dependence of microbial decomposers, the only organisms capable of increasing the supply of plant-available nurtrients, on the plant producers. The nature of the cooperative plant and microbial commmunities (mycorrhizae being one example) that have developed in rain forests is the result of adaptation to an environment where light, temperature, and moisture rarely limit plant activity.

Grassland ecosystems also efficiently conserve nutrients. If soil microbes and plants independently competed for nutrients, then intense microbial and plant activity would not coincide nor would low microbial activity occur during microbe-favorable/plant-unfavorable periods. Neither grassland nor rain forest ecosystems seem to have the independence of microbial and plant activities necessary for competition. However, in the grassland, the detrital food web is mostly underground and plant-microbe symbioses are less common. Further, microbially available nutrients are plentiful as is organic C. Despite plentiful nutrients and substrate for microbes, their activity is disproportionately low during periods of reduced plant activity. Plant activity releases energy as organic C into the soil, stimulating microbial activity (Alexander, 1977, p. 427-429; Foster et al., 1983; Merckx et al., 1985). Thus, a "plant-control hypothesis" is proposed.

THE PLANT-CONTROL HYPOTHESIS

The hypothesis is that in stable grasslands, as well as in stable forest ecosystems, plants control microbial decomposition of organic matter and the associated transformations of nutrients by controlling the supply of energy. The "plant-control" hypothesis can be valid only if three conditions exist in grassland soils. First, microbial activity must be energy-limited when plants are inactive. Second, since organic C (energy substrate) is plentiful in grassland soils, even when plants are inactive, energy limitation must depend on some abiotic soil factor(s). Third, plant activity must effectively control the soil factor(s) limiting energy.

Soil Microbes Are Starved

The lack of microbially available energy in soils is well recognized (Alexander, 1977). Generally, there are adequate inorganic nutrients but little readily utilizable organic nutrients. Evolution of CO_2 from soil will increase upon addition of a simple organic compound, but not when inorganic nutrients are added. Soil fractions produced more striking results in the same manner when supplemented with soluble C substrate vs. inorganic nutrients (Payne, 1985). Also, the "rhizosphere effect" or microbe population increase adjacent to active plant roots is thought a response to root-derived, readily utilizable energy substrate (Alexander, 1977, p. 427-429; Merckx et al., 1985). No such response occurs beyond the rhizosphere though concentrations of inorganic nutrients likely are higher there. Thus, soil microbes lack available energy substrate when or where plants are inactive.

Factors Limiting the Microbial Availability of Energy

Three factors are thought to limit microbial access to the energy in soil organics: humification of organics, adsorption on soil particles, and occlusion within soil aggregates (Anderson, 1979; Black, 1968, p. 414-416; Payne, 1985). Additionally, the factors are interdependent. Under the same climate and vegetation, the nature and amount of humic substances are related to the quality of soil particles, and the quality and extent of soil structure is related to the soil organic matter, soil mineralogy, and particle size distribution.

Humification

More humified soil organics, those with higher molecular weight and aromaticity, are thought to resist microbial attack (Anderson, 1979). It is logical that the soil microbes must be able to decompose nearly any natural organic substance (Alexander, 1977, p. 130; Payne, 1985). And research results have been equivocal for a strong humification role in protecting soil organics (Skjemstad et al., 1986; Payne, 1985).

Aromatic substrates that might be expected to occur naturally in soils do not seem to resist soil microbial attack. Several monomeric aromatic compounds when added to soil were decomposed to CO_2 as effectively as glucose (Huntjens et al., 1981). They and others suggest the apparent resistance of some phenolics is due to polymerization side reactions that occur during oxidative degradation (Haider and Martin, 1975). Several genera of soil bacteria cleave polymers, chosen as models of lignin and humics, if supplied with an available energy substrate (Rast et al., 1980). Some authors mention a likely cooperative or mutualistic attack on soil organics by microbes (Payne, 1985; Rast et al., 1980; Sato, 1981).

Highly humified organic matter in a silt fraction free of clay, and less humified organic matter in a clay fraction, decomposed at similar high rates so long as soluble energy substrate was supplied (Payne, 1985) (Fig. 3.1). Dormaar and Pittman (1980) noted that the lignin content increased in crop residues decomposing underground in fallow and in root residues decomposing over winter in grassland soil, but decreased again when plant activity resumed in the spring or due to

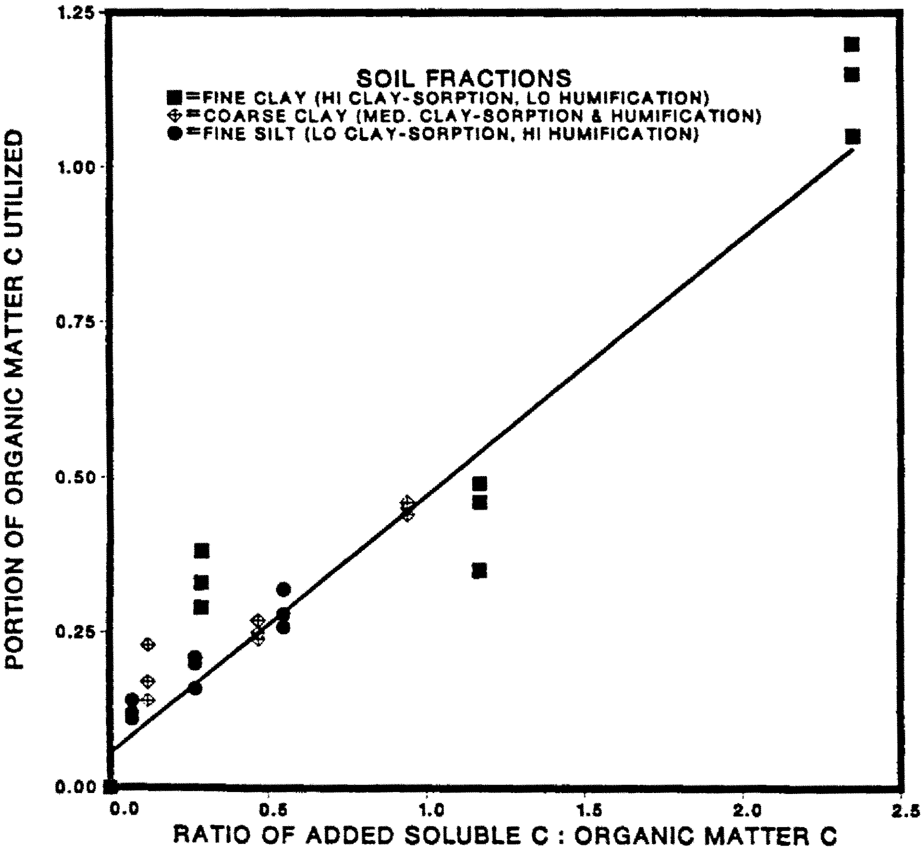


Fig. 3.1. Effect of added soluble carbon substrate (cellobiose) on the extent of microbial utilization of the carbon in soil organic matter associated with different fine soil particle size fractions (i.e., chemically different soil organic matter fractions). Data of Payne (1985).

planting. It is suggested that stabilization by adsorption and humification arises primarily from the insolubility of adsorbed or humified substrates (Payne, 1985).

Adsorption

Adsorption of organics, especially by clays, is thought to thwart enzymatic microbial attack by hindering geometry or substrate accessibility. Adsorption reduces the solubility of the adsorbed organics. If substrate solubility is important, then soil microbes should require readily available energy to excrete and maintain in the soil solution the enzymes necessary to attack less soluble substrates. Addition of soluble substrate promoted decomposition of clay-adsorbed organic matter (Payne, 1985). Addition of plant residues promoted loss of soil organic matter under both laboratory (Table 3.2) and field conditions (Broadbent, 1947; Broadbent and Norman, 1946; Rouse, 1947). It appears that soil microbes do attack humified, lignified, or adsorbed materials when provided adequately available energy.

Table 3.2. Effect of decomposition of added (^{13}C-labelled) sudan grass (Sorghum vulgare L.) residues on mineralization of soil organic matter in soil incubated for 11 days. Data from Broadbent and Norman (1946).

Sudan grass residue added to 100g soil	CO_2 evolved (total)	CO_2 from Sudan grass	CO_2 from soil
-g-	-mg-		
0	48.7	-----	48.7
1	633.1	417.8	215.3
2	980.7	651.5	329.2

Occlusion

Research demonstrating stabilization of soil organic matter by occlusion in specific structural units is rare. But research showing increased microbial attack when soils are physically disturbed is common

(Black, 1968, p.416-418). Grinding a previously sieved sod soil caused a 140% increase in CO_2 evolution (Powlson, 1980). Reaggregation of dispersed clays decreased loss of occluded organic matter, despite added soluble substrate (Payne, 1985). Artificial aggregates protected ^{14}C-labeled starch until they were disrupted by mechanical or wet-dry treatments (Adu and Oades, 1978). For this discussion it does not matter whether disturbance promotes microbial attack by increasing physical exposure or improving aeration. It is only important that microbial attack increases with physical disturbance and with added energy substrate.

Plant-Control of Microbial Activity

Whether plant activity controls the soil factor(s) limiting microbial activity has not been directly examined. However, plant roots are known to release significant amounts of organic compounds into the soil, probably causing the "rhizosphere effect". Further, plant root growth physically disturbs the soil. Plant root activity, then, both physically disturbs the structure of soil and increases readily available energy supplies. It seems reasonable, in view of the previous discussion, to expect that plant activity would increase microbial activity and consequently, decomposition of soil organics.

Plants Stimulate Mineralization of Soil Organic Matter

Analysis of the data of Beale et al. (1955) and Johnston et al. (1942), as examples among several, indicates an increase in annual decomposition of soil organic matter in agricultural soils as the time under actively growing plant cover increases. Using isotopic nitrogen (^{15}N), Bartholomew and Clark (1950) concluded that total mineralization in cropped soil was four times that in the same soil in fallow. Haider et al. (1987) reporting a ^{15}N study of the effects of plant roots on denitrification observed, "The mineralization of organic N seems to be greatly enhanced by the presence of plants." In other work the microbial biomass in fallow plots changed little in a year, while in plots planted to wheat and pasture the microbial biomass increased from the time of seeding (Ladd et al., 1981). Large biomass increases occurred in the planted soils shortly after fall rains. Biomass responses to the rains were slower and much smaller in the unplanted soil. Note that these observations contradict the "competition" hypothesis. Despite an absence of plant competition for nutrients in the unplanted soil, the microbial biomass did not respond to moisture inputs as it did in the planted soil where plants were competing for both moisture and nutrients. These results indicate that plant activity controls microbially-restrictive soil factor(s) to permit higher levels of microbial activity.

Plants Suppress Decomposition of Their Own Residues

If plant root activity only removed the effects of the restrictive soil factor(s) to permit unrestricted microbial activity, there would be nothing to stop the microbes until all C substrate had been exhausted. Such control would not conserve ecosystem integrity. Effective plant control must assure not only adequate mineralization -- i.e., adequate levels of appropriate microbial activity--to meet plant nutrient requirements during growth, but also must assure that when plant activity ceases, microbial activity is restricted such that net mineralization is minimized. Mineralizable N in grassland soils has been found to decrease through the growing season, being restored during the winter (Fig.3.2). The results of Eagle (1961) and Richardson (1938) suggest microbial activity leading to net N mineralization predominates during active plant growth despite high levels of available C (from plant inputs) and low levels of available N (due to plant uptake). That is, plant activity induces net mineralization in the presence of an apparently high C:N ratio -- the condition traditionally considered to promote immobilization. The increase in mineralizable N during the winter suggests that microbial activity leading to net mineralization of N is reduced, despite less available C and presumably more available N due to an absence of plant "competition" for N. Summarizing these results, mineralizable N increased over winter when plants were inactive and decreased during spring and summer when plants were active, the latter condition apparently inducing microbial activity leading to net N mineralization.

Merckx et al. (1985) presented data suggesting that plant activity may control soil factor(s) to stabilize root-derived organic substrate. Wheat (Triticum aestivum L. var. Sicco) plants grew in a phytotron for 32 days with the shoots in an atmosphere enriched with labeled $^{14}CO_2$ and the roots in pots with a separately controlled atmosphere with no label. The arrangement enabled measurement of ^{14}C-labeled photosynthate translocated from shoots to roots, lost from roots to soil (including microbial biomass and root-derived materials), and respired by roots and microbes to gaseous $^{14}CO_2$.

The study examined the dynamics of root-derived ^{14}C in two soils of different texture, a sandy soil (960 g kg^{-1} sand, 20 g kg^{-1} silt, and 20 g kg^{-1} clay) and a silty clay loam (130 g kg^{-1} sand, 500 g kg^{-1} silt, and 370 g kg^{-1} clay). Activity levels indicated that the silty clay

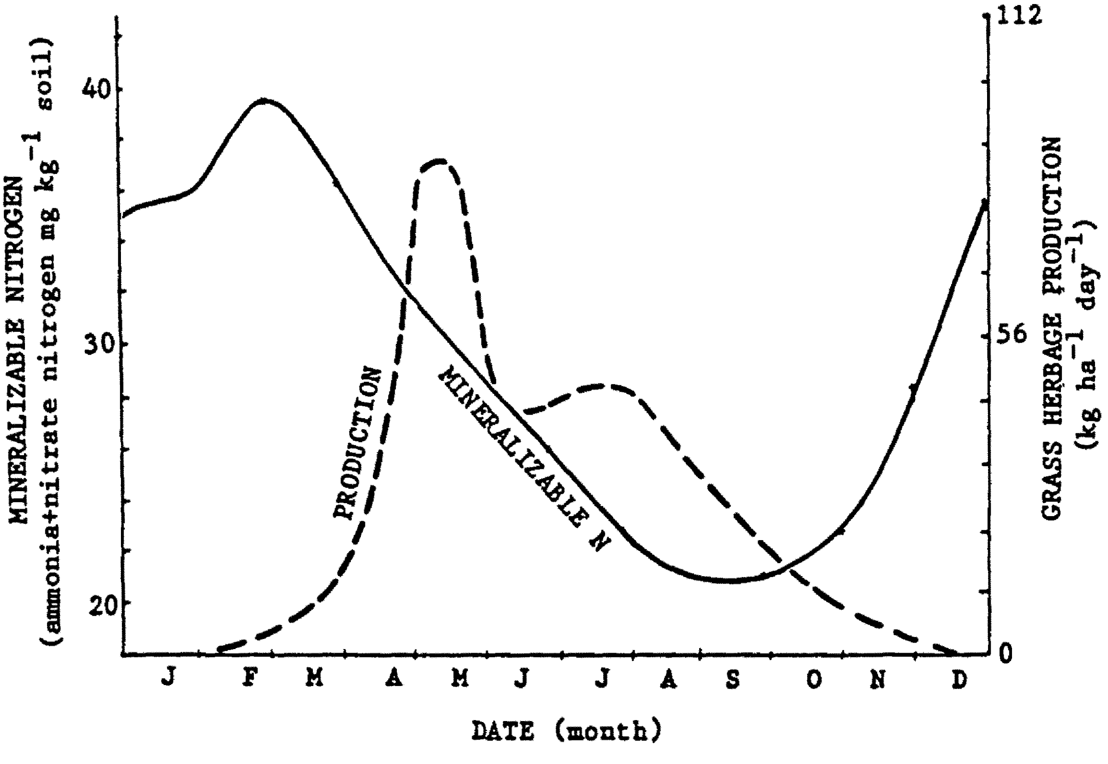


Fig. 3.2. Seasonal patterns of potentially mineralizable nitrogen under grasses and above-ground production of pasture grasses in England. Adapted from Richardson (1938), and Anslow and Green (1967).

loam soil accumulated more labeled microbial biomass and retained more of the fixed ^{14}C in root-derived materials, despite more root biomass and soil respiration in the sandy soil (Fig.3.3).

The data from the sand show that after 18 days of shoot exposure to $^{14}CO_2$ root biomass and soil contained 63% of the ^{14}C translocated from the shoots to the roots, decreasing to just over 61% after 32 days exposure (Fig. 3.4). In contrast, in the more clayey soil the translocated ^{14}C remaining in the roots and soil increased from 61% after 18 days shoot exposure to $^{14}CO_2$ to 71% after 32 days exposure.

In the sandy soil, decompostion of translocated ^{14}C-organics to $^{14}CO_2$ increased with increasing root ^{14}C, indicating decomposition of root-derived materials was related to release of labelled substrate from the roots (Figures 3.3, 3.4). In the more clayey soil, the portion of translocated ^{14}C-organics respired/decomposed (to $^{14}CO_2$) decreased as the quantity of roots and root-derived materials increased (Figures 3.3, 3.4). That is, plant activity affected the more clayey soil in a manner that resulted in stabilization of recently released organic substrates.

Results from another phytotron ^{14}C study carried out by Dormaar and Sauerbeck (1983), using blue grama, also indicated a distinct stabilization of recently deposited root-derived carbon (Table 3.1). During the first simulated summer, when all photosynthetic labelling occurred, 27% of the ^{14}C activity translocated below-ground was recovered in the soil while 31% was respired/decomposed to $^{14}CO_2$. Despite release of more than half the ^{14}C activity from recoverable roots during the simulated fall/winter, bringing that recovered in the soil to 48%, only 4% of the ^{14}C was respired/decomposed to $^{14}CO_2$. The second summer began with 48% of the ^{14}C activity in the soil and 16% in

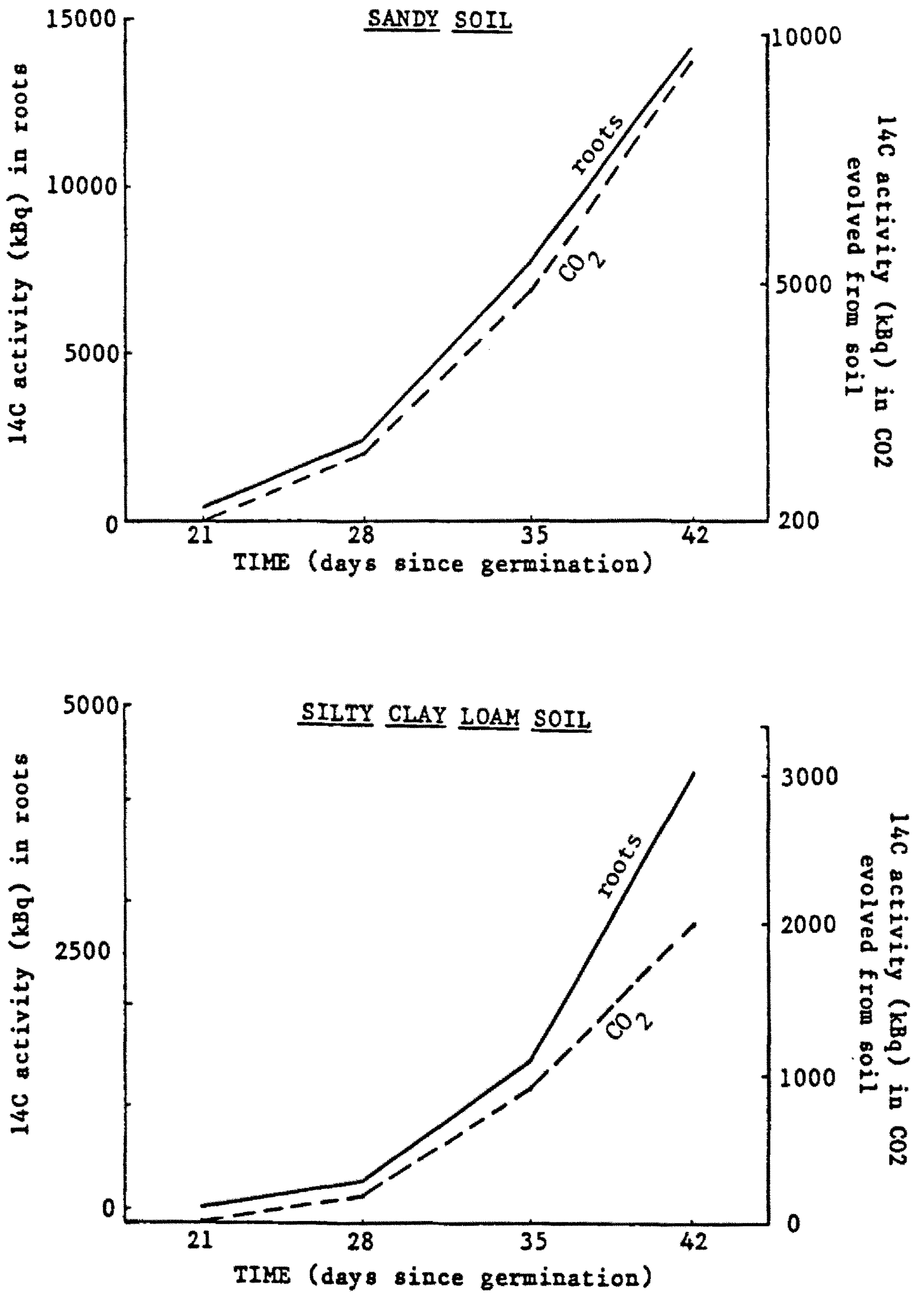
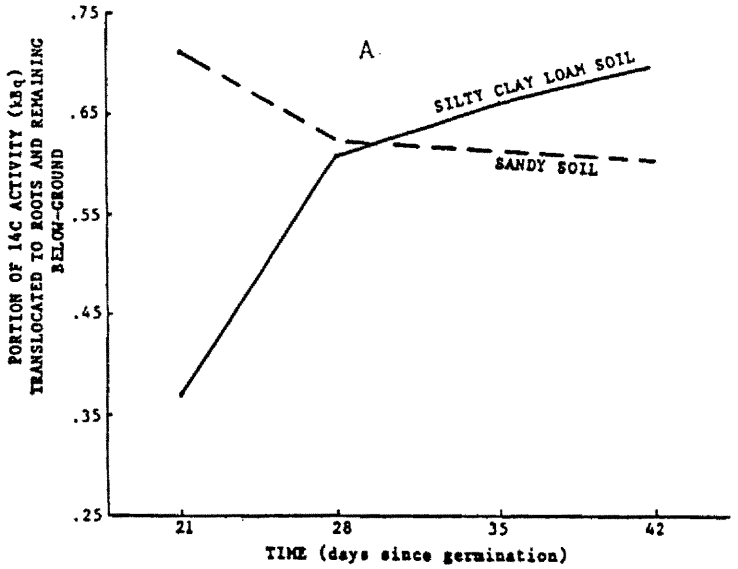


Fig. 3.3. Effects of soil texture on the ^{14}C activity (kBq) in the roots in, and CO_2 (root+microbial respiration) evolved from, two soils (a sandy soil and a silty clay loam) planted to wheat (Triticum aestivum var. Sicco). Data of Merckx et al. (1985).



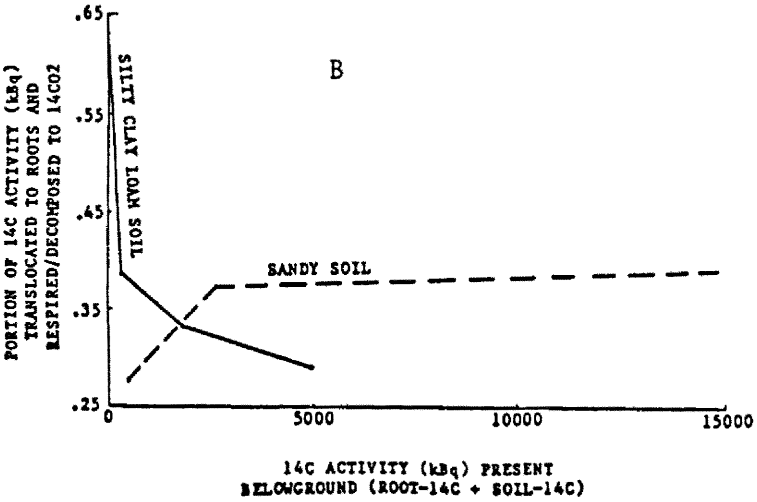


Fig. 3.4. Effects of soil texture on the stability of recently wheat-root-derived, ^{14}C-labelled organics.
(A) The portion of ^{14}C activity translocated to roots that remained in the soil and roots, i.e., that was not respired/decomposed to $^{14}CO_2$, on each sampling date.
(B) The effects of the amount of ^{14}C (activity) in roots and soil on the portion of ^{14}C (activity) translocated to roots and respired/decomposed to $^{14}CO_2$. Data of Merckx et al. (1985).

the recoverable roots (35% having been evolved as $^{14}CO_2$ over the preceeding two seasons), but less, probably considerably less, than 4% of the ^{14}C was respired/decomposed to $^{14}CO_2$. This is all the more striking when one considers that the second summer was the period of the greatest total ($^{12}CO_2$ + $^{14}CO_2$) soil respiration. The results indicate the ^{14}C deposited by the roots during the preceeding seasons was remarkably stable during the second summer.

An important role for clays in plant-induced stabilization of root-derived materials is shown, also, by the results of Craswell and Waring (1972 a, b). Those authors examined the effect of grinding on N mineralization in soils of different clay contents. Grinding grassland soils increased the mineralization of N only in soils with more than 10% clay. Further, the effect was more pronounced for soils containing montmorillonitic clays than for those with kaolinitic clays. Their findings suggest that grinding disrupts soil structural units in which clay is an essential component and which stabilize labile soil organics as long as the units remain undisturbed.

The findings of Craswell and Waring (1972 a, b), Dormaar and Sauerbeck (1983), Merckx et al. (1985), and Richardson (1938) taken together, suggest that plant activity stabilizes labile organics in root and microbial residues by inclusion within structural units formed as a consequence of the effects of root activity on soil particles, especially clays. Further, the character and extent of microbial decomposition may be strongly influenced by such plant-induced stabilization.

CONCLUSIONS

The information presented indicates that the "plant control" hypothesis is reasonable. Plants may control microbial activity by at least two root effects on the soil environment. Growing plant roots physically disturb the soil structure while releasing readily available energy into the surrounding soil. Stabilization of energy substrates by occlusion within plant-induced structural units, the formation and efficacy of which seems to depend on clay particles, assures that energy availability will limit microbial activities when plant roots are inactive. Under these conditions plants control the supply of microbially available energy; microbial activity is dependent on plant activity and competition cannot occur. Thus, the "plant-control" hypothesis rejects the idea that plant-microbial competition dominates plant-microbe relationships.

Plants depend on microbial activity to release adequate supplies of plant-available nutrients just as the microbes depend on the plants for energy, but unrestrained microbial activity would result in harmful losses of nutrient resources from the ecosystem. Plants acquire energy and release it into the soil in accordance with the supply of plant available nutrients and the plant-favorability of soil conditions, and, as the most important "movers" of soil particles, induce the development of soil structure, promoting and restricting microbial activity and release of plant-available nutrients according to the level and type of plant activity.

The substrate-stabilizing effect of soil structure is well supported. Also, it is historically established that grasses are particularly beneficial to soil structure, but no mechanistic or conceptual model has been able to adequately explain the soil structuring effects of plant root activity. A model capable of integrating the interdependent structurally related functions of plant, soil, and microbe is needed. Lack of such a model probably has hindered fertility/productivity research. The need for such a model may be met by a recent conceptualization of the functional dynamics of soil/plant/microbe systems (Payne, 1985; Payne and Norstadt, 1984, 1985). That conceptualization is compatible with the "plant control" hypothesis, based on relatively simple fundamental principles, and flexible enough that assimilation of new information improves its applicability, yet sufficiently defined to generate experimentally testable hypotheses. It is a potentially useful new tool which may lead to a better understanding of soil fertility/productivity. It may, since it models soil/plant/microbe interactions in plant/soil systems, lead to development of an agriculturally useful definition of soil fertility/productivity as a biophysical quality of an ecosystem rather than a physicochemical property of soil.

Chapter 4. THE RHIZOCENTRIC MODEL OF SOIL STRUCTURAL DEVELOPMENT

INTRODUCTION

With few exceptions, productive soils are well-structured. Production problems and increased erosion, often associated with deteriorated soil structure, have interested many researchers (Beale et al., 1955; Johnston et al., 1942; van Bavel and Schaller, 1950; Wilson and Browning, 1945). Research on soil aggregation has been favored, over that on intact soil structure, perhaps because the former is easier to observe and measure. A voluminous literature on soil aggregation (Harris et al., 1966; Tisdall and Oades, 1982) has accumulated from the continuous efforts to understand soil structure as a major factor affecting soil productivity even though aggregation and structure are not the same (Allison, 1973, p. 316).

The term "soil productivity," as commonly used, is misleading. Soils do not produce -- plants do. On the other hand, plants do not produce efficiently on unsuited soils. In other words, as Jacks (1963) suggested, productivity is not a physicochemical quality of a soil (or a biological property of plants), but a biophysical quality of a plant/soil system. This statement has several consequences. If productivity is a quality of a plant/soil system, then it follows that in any stable plant/soil system a mutual dependence between the plant community and soil will have evolved. Further, the apparently obligate association of soil structure with productivity suggests structure may be the biophysical quality inherent in and essential to stable, productive plant/soil systems (see Chapter 3). It also follows that stable plant/soil ecosystems are highly integrated, made up of intimately and extensively interrelated plant and soil components; and that an understanding of how those essential interrelationships are controlled in stable, productive plant/soil systems might provide more than temporary, "band-aid" answers for production problems which until now have been considered structure-, fertility-, or erosion-related. The test of this thesis, of course, will be whether it eventually contributes something to development of a "sustainable" agriculture.

The present author is not aware of any conceptual model that considers soil aggregates to directly affect soil fertility/productivity and simultaneously attributes to them unique structural/developmental functions in the soil body. This chapter presents the rhizocentric model of soil structural development, a model that credits a wide spectrum of the biophysicochemical qualities of plant/soil systems to soil structures and structural processes. At times, the presentation may seem circuitous and repetitive, but that is because of emphasis on interactions. The model emphasizes interrelationships and their consequences rather than single factors.

PREVIOUS MODELS OF SOIL AGGREGATION

Several conceptual models attempt to explain how soils aggregate. Harris et al.'s (1966) thorough review of soil aggregation literature discussed, among others, Emerson's (1959) early model of soil crumb structure. That review reported an almost bewildering number of often contradictory results. Not surprisingly, no conceptual or general model could handle the information, let alone reconcile disagreements.

A more recent, thorough but concise review of the literature on soil aggregation may be found in Tisdall and Oades's (1982) presentation of their hierarchical model for soil aggregate structure. They expanded their model to explain soil aggregate development and discussed implications for management practices. Their model, probably the most well-developed model presented in the literature, is based on extensive studies of Australian red-brown earths, and postulates two types of aggregates: macroaggregates, those of diameters >250 micrometers, and microaggregates, diameters <250 micrometers. They regarded macroaggregates to be ephemeral assemblies of microaggregates stabilized by "transient" (polysaccharides) and "temporary" (roots and hyphae) binding agents. In contrast, they suggested that "persistent" binding agents (degraded, aromatic humic material associated with inorganic soil components) stabilize microaggregates. Only the development of "stable particles 2-20 μm" and "aggregates <2 μm diameter" were specifically discussed by them.

Later, Oades (1984) clarified their model and the genesis of the two principal aggregate size classes. He considered macroaggregates to form as growing roots and hyphae "enmesh" sets of microaggregates. Annual replacement of roots and hyphae catch portions of old roots and hyphae within newly-bound macroaggregates. Decomposition of internalized root and hyphal fragments and microbial residues eventually forms persistent binding agents and stable microaggregates. Elliott (1986) interpreted the results of his study of aggregates from a Pachic Haplustoll in Nebraska as corroborating the Tisdall and Oades model.

Tisdall and Oades (1982) based their model on assumptions about the principal roles of soil structure. One assumption was that favorable soil aggregation assures soil physical conditions favorable for plant growth. Another was that in order to enhance the plant-favorability of soil physical conditions water-stable aggregates must be able to remain aerobic internally while retaining plant-available water. Those authors emphasized three points as implications of their model:
(i) microaggregates are essentially permanent structures, relatively insensitive to management practices, while macroaggregates are temporary and sensitive to management, and (ii) plant roots improve soil structure by assuring "...the best distribution of mucilage and energy source for microorganisms..." and (iii) "...additions of organic matter will serve the same effect" (Oades,1984). They did not discuss the possibility that these points might not be implications of their description of soil structure and structural processes, but of the assumptions upon which their description was based.

Results of new research and reinterpretation of old data indicate that it should be possible to advance beyond the Tisdall and Oades model. This chapter presents a new model which suggests that soil aggregates are intrinsic in soil formation/structure/function and that soil structure is a primary cause of the stable fertility/productivity of undisturbed plant/soil systems. The new model synthesizes many aspects of previous models and new information into a plant-oriented conceptualization of soil structure/function reminiscent of Bradfield's (1937). A few implications of the new model follow: (i) Ultimately tillage operations worsen soil structure (Bradfield, 1937). (ii) Aggregates <250 micrometers in diameter are dynamic and sensitive to management. (iii) Organic matter amendments are not substitutable for plant roots as "structural improvers". (iv) Soil structure affects not only the physical but also the biological and chemical processes and conditions of the soil. (v) These effects are due to a plant-controllable biophysical organization of soil particles, especially clays, which results in diffusion limitations, hence, stabilization of organic matter and nutrients in relatively anaerobic microsites within water-stable aggregates.

UNEXPLAINED OBSERVATIONS AND DATA

The ideas behind the model came from considering observations made during work for the thesis entitled "Studies of the Mechanisms of Stabilization of Organic Matter in Semiarid Soils" (Payne, 1985). Observations and corroborative data indicated that the physical, chemical, and biological processes occurring in certain water-stable aggregates (WSA) differed from those occurring in the rest of the soil. For example, the clay content (<2 micrometer, weight basis) of these WSA was considerably higher than the rest of the soil. Also, when water-floatable plant residues retained by 0.250-mm sieves were excluded, the WSA passing through that sieve contained more nitrogen (N), phophorous (P), and organic carbon (C) than the nonaggregated soil (that passed the sieve). WSA with diameters >0.05 mm had higher C:N ratios than the rest of the soil.

These same WSA (diameter >0.05 mm), when ultrasonically dispersed, unexpectedly released the dark colors and strong odors typical of anaerobic decomposition processes! Similar dispersion of remaining soil, that not in >0.05-mm WSA, or of bulk soils did not produce unusual odors or colors. The fine silt dispersed from these WSA had ammonium bicarbonate-diethylenetriamine-penta-acetic acid (NH_4HCO_3DTPA)-extractable levels of iron, manganese, and nickel (Fe, Mn, and Ni) that were 4, 53, and 35 times higher, respectively, than the fine silt not in WSA.

These results indicated the soil in WSA perhaps as small as 0.05 mm in diameter contained more organic matter, and that this organic matter was qualitatively different from, possibly less decomposed than, that associated with unaggregated soil. Further, the odors, colors, extractabilities of Fe, Mn, and Ni, and redox potential changes which occurred upon dispersion of WSA indicated the occurrence of localized anaerobiosis within larger WSA (Norstadt and Payne, 1984; Payne, 1985). Sexstone et al. (1985) have directly measured low redox potentials inside aggregates, using microelectrodes.

What in soil theory could account for the apparent anaerobic microsites in such small WSA? Initially it seemed reasonable to believe that it might be particles of relatively undecomposed organic matter in the cores of water-stable aggregates, as suggested by Tisdall and Oades (1982) and Oades (1984). Anaerobiosis, then, might be due to high oxygen demand associated with the decomposition of occluded organic matter in the aggregate cores. However, other observations made it illogical to suppose that there were sufficient anaerobic residues to cause the marked odors and colors observed on dispersion of WSA.

Consider, for example, the following: The strongest indications of anaerobiosis effused from a sample of the Harney series Typic Argiustoll which had been air-dried, ground to pass a 2-mm sieve, and stored air-dry in paper containers for six months prior to study. Most, if not all, anaerobic microsites within transient macroaggregates or in organic fragments that had developed clay-mucilage coatings (per Oades, 1984) should have been aerated during sample preparation.

And what is to account for the high clay content of WSA? Payne's data (1985) showed that the clay content of the WSA (stable to immersion from an air-dry condition) from two uncultivated soils (a Typic and an Aridic Argiustoll) must have been at least twice that of the bulk soil. Analysis of the data of Dormaar (1983) show that WSA (stable to immersion following capillary wetting) from cultivated and uncultivated sites on a Haploboroll had a clay content ranging from 1.3 to 2.2 times that of the bulk soil. Other researchers have reported similar results (Harris et al., 1966, p. 140). Clay enrichments like these imply that each aggregate contains essentially all the clay from a volume of non-aggregated soil nearly equal to the volume of the aggregate itself. How is this clay segregation accomplished? Previous models, with the possible exception of Oades (1984), have implied that aggregates develop as roots and hyphae "enmesh" randomly distributed particles, or as such random distributions are glued together near fragments of organic matter.

Is it not possible, even necessary, in view of the high clay contents, that some factor or factors other than chance encounter concentrate the clay and organic carbon in WSA? Reference is to WSA capable of protecting anaerobic microsites, maintaining them intact during extended dry, aerated storage, even during air-dry processing and grinding, and immersion in water from an air-dry state. Surely, these are not characteristics of randomly assembled soil aggregates.

REQUIREMENTS OF THE NEXT MODEL

A new model was needed to explain the observed qualities of WSA: (i) high organic carbon content; (ii) high clay content (in undisturbed soils, at least 1.5 times the clay content of the bulk soil); (iii) relatively small size (from <0.05 to 2.00 mm in diameter); (iv) high structural stability (resistant to dry grinding and immersion); and (v) apparent anaerobic microsites and, by inference, unique microfloral, chemical and physical properties.

Toward a General Concept of Soil Aggregation

The soil aggregation phenomenon is bewilderingly complex. Previous efforts to explain the aggregation phenomenon and the qualities of WSA have concentrated on various suggested mechanisms including, among others: clay involvement with water dipoles, cation bridges, and precipitated and irreversibly dehydrated colloids; cementation of clays and larger soil particles by organic substances; microbial production and degradation of the cementing organic substances; entanglement of soil particles by roots and hyphae; and the effects of wet-dry/freeze-thaw cycles. This list is not complete. The number of suspected mechanisms is large and the number of possible interactions even larger. (And the number of physicochemicaly-oriented models may be expected to increase each time a likely new mechanism is discovered.) The number of possible combinations of physicochemical aggregation mechanisms, each combination potentially resulting in a different aggregation condition, is legion. Yet there are surprisingly few soil aggregation conditions that are biologically favorable over the long-term, and these conditions are not known to arise spontaneously or to be self-sustaining in soils uninhabited by plants.

Allison (1973, p. 315-345) offered a different perspective on aggregation. He emphasized spatial and temporal requirements, and suggested that desirable aggregation results from the simultaneous operation of mechanisms effective in the distinct processes of aggregate formation and stabilization. Formation is primarily a spatial effect, becoming apparent when finer particles are moved into close proximity and oriented so that physicochemical forces can hold them together on drying (ibid, p. 317). Time is a distinctly more important factor in stabilization -- recently-formed aggregates are generally less stable than older ones (ibid, p.315-317). Allison pointed out that forces responsible for aggregate formation usually do not provide long-term stablility, and some of the best stabilizing agents have no effect on aggregate formation (ibid, p. 317).

Favorable soil aggregation conditions in stable plant/soil systems are the result of the interplay of many different mechanisms. The interacting mechanisms and the effects of the interplay vary through space and time. It is thus apparent that specific-mechanism-oriented models will have only restricted applicability. A general model, based on a factor of general and fundamental importance, would be much more applicable and valuable in agricultural research and practice.

The Likelihood of a Single Causative Agent

Allison's distinction between formation and stabilization and the implied effects of space and time provide direction in the search for a fundamental cause of favorable soil aggregation. Stabilization, though it may be concurrent, cannot precede formation in time or space. Conversely, formation is precluded when and where stabilization is fully effective. The complexities of the aggregation process and the multitude of possible outcomes, most of which are biologically undesirable, suggest that desirable aggregation is the consequence of an orchestration of aggregation mechanisms and their interactions through space and time. It is unlikely that an orchestration of such complexity and extensiveness could be successfully arranged and conducted by more than one "conductor". The reasoning is that a general model of biologically desirable soil structure and function should concentrate not on identifying each instrument in the soil aggregation orchestra -- instruments which humans cannot "play" even if identified -- but, instead, on identifying the conductor, the apparently uniquely qualified agent of control and coordination, and its method.

The Plant Root: A Uniquely Qualified Agent

The single causative agent would have to be present throughout the soil and yet, on a very localized scale, able to affect the distribution of clay, organic carbon, microbial activity, and perhaps oxygen. Further, as pointed out by Allison (1973, p. 326-330), the mere presence of organic matter does not consistently result in the development of desirable soil aggregation. Whatever it was, it would also have to possess the ability to organize the clay such that shrink-swell processes would not disrupt aggregates or excessively ventilate their interiors. And it would have to exercise its abilities reliably and consistently over long periods of time.

The proverbial association of good soil structure with an undisturbed plant cover, especially a grass cover, and the character of plant root activities pointed to the plant root as a prime candidate. The plant is, in undisturbed soils, essentially the only source of organic carbon (Allison, 1973, p. 325; Dommergues et al., 1978; Foster et al., 1983; Thompson, 1952, p. 42). Water flow to an absorbing root probably affects the distribution of clay particles in its vicinity (Clarkson and Robards, 1975; Oades, 1984). Root-induced pressure differences and water flow might explain the tangential orientation of clays about roots and nearby particles (Foster et al, 1983). Active plant roots are, along with microorganisms, the soil's principal consumers of oxygen (Breymeyer et al., 1978; Foster et al., 1983). Active plant roots preferentially stimulate anaerobes and other members of the soil microflora -- the well-established "rhizosphere effect" (Alexander, 1977, p. 423-429; Allison, 1973, p. 85-86). Roots are the principal "movers" in undisturbed soils, rearranging the soil matrix as the growing root extends and expands through the soil. Plants must integrate essentially all the biologically-important environmental factors during their growth (Grable, 1966). And stable plant communities can be expected to integrate environmental factors and respond through growth and production, influencing the soil consistently through diurnal, seasonal, and climatic cycles for centuries. The rhizocentric model is based on the apparently unique ability of roots to induce all the physical, biological, chemical, and temporal conditions essential to form and stabilize the aggregates necessary to the maintenance of soil "productivity" (Allison, 1973, p. 316-343).

THE RHIZOCENTRIC MODEL

Soil structure is a dynamic, biophysical quality of plant/soil systems. The living plant root simultaneously affects both the organization of soil particles and microbial activities--these are key points! The root-induced organization of the soil becomes the soil structure and controls soil processes from shortly after the appearance of the young root to long after its death, or until destructive alteration of the root-induced organization. The discussion that follows describes the present author's conceptualization of how the plant root induces development of an essential component of desirable soil structure: the water-stable aggregate.

The Plant Root and Aggregate Initiation

Aggregate initiation begins when a new plant root appears. Penetrating the soil, the young root pushes soil particles aside or into adjacent voids, and reduces nearby pore space (Barber, 1971; Drew, 1979; Foster et al., 1983; Huck, 1979). At the same time the root tip consumes water, nutrients, and oxygen at a high rate, exudes various organic substances, and sloughs cell debris (Foster, 1983; Merckx et al., 1985; Trofymow, 1984). Some of these organic materials, in conjunction with the physical disturbance of the soil matrix, disperse clay particles which are moved in the water flowing to the root (Clarkson and Robards, 1975; Oades, 1984). At the root the water is taken up, while the clays are layered-down on the root surface, on or within the mucigel, if present, or in voids among nearby particles (see the EM work of Foster et al., 1983; Jenny and Grossenbacher,1963; Kilbertus, 1980).

Microorganism Numbers

Microorganisms are ubiquitous in the soil, and the extending root is thereby continuously inoculated. Nutrient uptake by and extension of the root tip are exceedingly rapid relative to the time microbes need to adjust to nutritional and environmental changes (Trofymow, 1984). Consequently, microbial reproduction and activity at or near the root tip are comparable to non-rhizosphere soil, despite high levels of available substrate (Foster et al., 1983).

Microbial Responses

When the microbes, most importantly the bacteria, finally respond to the more-than-adequate supply of readily-available carbon substrate, the preceding nutrient uptake by the root has imposed a microbial need for nutrients other than carbon (Trofymow, 1984). They respond to the new constraints with enzyme systems that act on organic matter to increase solution levels of N, P, etc., (McGill and Cole, 1981; Payne, 1985). However, the large, active root surface behind the root tip enables the plant to obtain a significant portion of the released nutrients. The microbial effort converts the most available, root-derived materials, as well as significant amounts of soil organic matter, to microbial biomass and residues. All of these are interspersed within the accumulating clay matrix (Foster et al., 1983; Kilbertus, 1980).

Building the Aggregate Matrix

The continued uptake of water and nutrients causes further accumulation of organized clay around the root, embedding rhizosphere microbes and soil particles in a thickening clay/organic matter matrix.

Uptake of water, accumulation of clay, and release of readily-available carbon substrate slow as the root matures. The live mature root is now shrouded in a well-formed, but unstable organization of soil materials: a <u>newly-developing</u> <u>soil</u> <u>aggregate</u>.

The internal particle arrangement, clay, and microbial activity of the new soil aggregate already exert a retarding effect on diffusion of gases, water, and ions. Some fungal intrusions into this localized organization may occur, but restricted diffusion within the clay/organic matrix markedly limits microbial utilization of embedded substrates. Oxygen supplies probably are not limiting when water movement to the root is rapid, i.e., near the root tip, despite high rates of root respiration. On the other hand, the mature root is "shielded" from the influence of the soil environment external to it by the embedding matrix and the root's own mature morphology and physiology (Clarkson and Robards, 1975). Further, the mature root's respiration rate is slower than that of the root tip (Lemon and Wiegand, 1962). Oxygen supplies may not become crucial (determining whether local microbial activity is more aerobic or anaerobic) inside the aggregate until after root death.

Aggregate Activity after Root Death

Root death, here considered a relatively continuous process beginning with decortication, causes another major input of carbon substrate to the soil. Now, however, the input is different from that produced by new root growth (Foster et al., 1983). Whereas, exudates and debris from the young root elicited a marked microbial response, that is not the case with the mature/dying root. Carbon substrates are mostly structural or structurally-associated plant tissues embedded within the clay/organic matrix. Certain restrictions, peculiar to the immature new aggregate, moderate microbial activity near the dead root.

For example, restrictions on diffusion, especially of gases and anions, are an effect of the clay. The relatively fixed spatial arrangement of nutrients, microbes, and carbon substrates within the matrix of clay and other soil particles effectively places microbes and substrates in microscopic compartments, isolated from one another (Foster et al., 1983; Griffin, 1981; Kilbertus, 1980). This isolation stifles microbial activity, even though considerable quantities of microbially available energy and nutrients may have accumulated in the clay/organic matrix by the time of root death.

Microbial activity inside the new aggregate cannot be ignored though. Due to the diffusion constraints, degradation of carbon substrates inside the aggregate is slower and qualitatively different from degradation occurring outside the aggregate. Because the clay/organic matrix assures that the interior always holds some moisture and freezes at temperatures below 0°C, low rates of microbial activity are possible as long as some carbon is available, even through extended periods of relatively extreme dryness or cold (Dormaar and Sauerbeck, 1983; Dommergues et al., 1978). Inclusion within clay/organic matrices protects soil microbes sensitive to dessication (Kilbertus et al., 1979).

Moderated decomposition inside the aggregate, characterized by some degree of C excess in a partially to fully anaerobic environment, results from diffusion restrictions. The aggregate has a low O_2 diffusion rate. If dry, shrinkage reduces or eliminates pore continuity through the clay/organic matrix; and if moist, water occupies or blocks the pores (Lawrence et al., 1979; Newman and Thomasson, 1979). Any microbial activity in the moist aggregate further burdens the internal O_2 supply. An internal supply of available substrate, augmented by the capacity of the clay/organic matrix and organic core to shrink or swell in response to moisture changes and yet retain physical integrity, allows the immature aggregate to act as a nutrient sink (Emerson and Dettman, 1959; Newman and Thomasson, 1979; Norstadt and Payne, unpublished data). Dissolved nutrients, carried in or diffusing through water absorbed into the moist aggregate, may internally be incorporated into microbial biomass, metabolites, or adsorbed substances. Also, at these times there is potential for denitrification, if NO_3^- levels outside the aggregate are high enough, or for N-fixation, if solution levels of available N are low.

Stabilizing the Aggregate

Over time the root residue "core" is converted to organics of primarily anaerobic, microbial origin, integrated as part of the consolidated clay/organic matrix. Reduced forms of some elements may accumulate. Shrink-swell cycles and the effects of external compressive forces, like plant root activity and freeze-thaw processes, adjust the aggregate's size and shape to offset any volume reductions resulting from degradation of root residue.

Abiotic factors affect the clay and the microbially-produced organic components to stabilize the aggregate, also. Drying events shrink the clay/organic matrix, reducing distances between the oriented clay particles in the matrix. Less water and shorter distances between clay particles increase the concentration of and inter-particle bridging by adsorbed organics. More effective organic cementing of the closely packed, oriented clay particles results.

The reduced inter-particle distances, more effective organic cementation, and perhaps irreversible dehydration of some of the organics, fix particle arrangements, possibly inhibiting re-entry of water into some of the inter-particle spaces. Once the particle arrangements are fixed, age-hardening of the clay matrix enhances the structural stability of the aggregate, also (Molope et al., 1985).

It is suggested that the mature, non-slaking, water-stable aggregate, the aggregate always present in naturally fertile upland soils, is produced by a dynamic, biophysical process. Its formation and stabilization are totally dependent on an organization and sequence of soil biological and physical components and events induced during, and only during, the life cycle of active plant roots. Limited diffusion within the aggregate results in accumulation of nutrient-rich, labile, partially-decomposed root and microbial residues, microbial products, and reduced, inorganic forms of some nutrients. Root residues predominate in the immature aggregate, but are unrecognizable in the mature aggregate (Norstadt and Payne, unpublished data).

Aggregate De-Stabilization and Dispersion

If undisturbed, a mature aggregate might be expected to "fall apart," as organics slowly degrade, and act as a "slow-release" nutrient source or "stable organic matter pool". However, succeeding plant roots likely exploit the newly-formed aggregate before enough time passes for it to achieve maximum functional strength. One can visualize that the plant roots and soil in an undisturbed, stable system continually interact in a dynamic, yet harmonious, tearing-down and building-up of soil aggregates/structure.

Moist, plastic aggregates accomodate root growth better than rigid, primary mineral particles. Further, as a root pushes by a plastic aggregate, the root molds and disturbs the structural organization of the aggregate. Whenever aggregate organization is modified, some of the diffusional constraints are relieved and internally-retained nutrient forms are exposed. Needed soluble nutrients are absorbed by the growing root, and that uptake may prepare exposed sorptive surfaces for further activity. The rapid, root-stimulated microbial activity mineralizes labile organic nutrient forms previously stabilized inside the aggregate. Some of the mineralized nutrients are used by the plant, some by the microbes. Succeeding events create yet another new aggregate structure, assuring that the cyclical coupling of plant and microbial activity is not broken.

Clays in the root-disturbed aggregate are dispersed by the physical forces of the root, root-induced microbial attack on clay-associated organics, and possibly certain root exudates. Formation of a new aggregate begins immediately as the dispersed clays are drawn to and reorganized about the new root. Root-initiated aggregate deformation then, assures a timely release of nutrients that minimizes losses through leaching and volatilization. And fully as important, root-initiated aggregate dispersion provides timely and appropriate release of clays, in amounts and locations (relative to active roots) that minimize clay loss through eluviation.

THE SOIL STRUCTURE EQUILIBRIUM

Marshall (1962) defined soil structure as "the arrangement of the soil particles and the pore space between them." It would seem that, without any plant activity, this arrangement is a physical quality, changing with each effective input of mechanical energy. The abiotic chemical and physical environment will determine the quality and effective size of soil particles. As an instance, the mechanical energy of water and wind move particles of clay, sand, or even gravel, according to their effective particle size, in surface erosion.

Fine particles in most soils are distributed within a matrix of coarser particles, and percolation of water through the soil will cause selective downward movement of the smallest particles. Here is vertical "erosion" of clays, leading to the formation of argillic horizons, pans, or, under extreme conditions, possibly even loss of clay from the soil profile. The structure of uninhabited soils (i.e., soil systems as contrasted to plant/soil systems), then, is principally a product of erosional and depositional rearrangements of soil particles. A stable structure is achieved when the soil reaches an abiotic physical and chemical equilibrium with its environment. Whether the resulting structure is or is not favorable to plant growth is purely coincidental.

Structural Dynamics in Undisturbed Systems

In soils inhabited by plants a major portion of the effective energy inputs are of biological origin. The arrangement and rearrangement of soil particles, as well as the degree of aggregation which determines their effective size and functions, result from complex biophysical processes. Principally, these transformations involve plant roots, soil microbes and mineral particles. From the vantage point of the rhizocentric model, plants are seen as the first cause or sole biological source of energy inputs, both mechanical and chemical, in beneficial soil structural formation and function.

It follows that the outcome of the processes is controlled by the quality of plant energy inputs and their fractional-part of the total energy inputs to the soil in a given environment. Soil structure becomes more favorable to plant growth as the relative importance of plant inputs of structurally-effective energy increases.

Plant inputs occur through root action that rearranges soil particles and controls microbial activity. Subsequent microbial activity enhances the effectiveness of soil rearrangement by providing necessary materials to stabilize the root-induced structure. Later on, soil particles, especially clays and organics, respond to environmental events, such as drying, to transform the root-induced organization of plant/soil components into a stable aggregate. To recast the concept, the plant root rearranges soil particles, creating a microenvironment that regulates microbial activity; all three components, root, soil particles, and microbes, act in concert within the microenvironment. An aggregate develops that has a structurally-secured, but plant-accessible, nutrient store.

Aggregates form and stabilize, but not simultaneously. Favorable arrangements of soil microbes and particles can only occur during periods of plant root activity while most stabilization occurs some time later -- depending mostly on environmental conditions -- from some time before to some time after root death. Further, exploring plant roots disperse existing aggregates and initiate formation of new ones. This conceptualization requires that soil structure in an undisturbed plant-inhabited soil is dynamic. The undisturbed soil is structurally steady-state, i.e., as a matter of course, structural disturbance does not occur independently of structural restoration.

The number of aggregates, for example, is essentially constant, not because inherent aggregate stability prevents dispersion, but because aggregates are being dispersed and reformed at the same rate. Under these conditions structurally bound nutrients are not subject to loss from the soil profile except by plant uptake. Clay particles, essential to effective plant-induced structural stabilization of organic matter and nutrients, are retained in the root-affected zone. The stability of the undisturbed plant-soil system is apparent not only as a balanced composition of the plant and microbial communities, but also by the ecologically sound structural and nutrient status of the soil. Thus, one can appreciate that the productivity of the undisturbed plant-soil system is dependent upon a continuous, sensitive, plant-oriented, perhaps even plant-dominated, dynamic process. This complex process governs the distribution of nutrients among its biotic and abiotic components, stabilizes the nutrients in periods of plant inactivity, and releases them at times appropriate for plant use.

Structural Dynamics in Agricultural Systems

Given the previous arguments, one can reason that deleterious changes in soil structure result from any inappropriate inputs of structurally effective energy. Soil-based culture of agricultural crops has developed around inputs which are inherently, though not intentionally, inappropriate.

In most cropping systems tillage is perhaps the dominant structure-affecting energy input. Too often, tillage interrupts or terminates the root-induced sequence of soil structural events. A plant-favorable soil organization is initiated when crop roots begin to establish control of the "arrangement of the soil particles and the pore space between them." Crucial arrangements of biotic and abiotic components arise from gradual and sequenced applications of mechanical, chemical, and biological energies throughout the root-affected soil body. Suppose those innumerable, small-scale, but structurally essential, plant-controlled events are superseded by management control that invokes large, episodic applications of mechanical energy on a macroscopic scale. What has tillage done beyond randomizing some portion, if not most, of the root-induced arrangement of soil particles and pores that the crop may have had time to accomplish?

Aggregate formation, initiated by crop root organization of the soil, is disrupted by tillage before aggregate stabilization can become effective. One of the principal effects of such disruption is relief of the root-induced structural constraints on microbial activity that would otherwise control the extent and quality of microbial soil processes and help to stabilize plant and microbial residues. This interruption causes the soil environment to become more oxidized and uniform (Linn and Doran, 1984).

Consequently, there is loss of organic matter and an increase in the relative importance of more highly aromatic organic matter, such as humic acids and high-lignin residues (Dormaar, 1983). Concurrently, the complexity and stability of the soil microflora declines (Linn and Doran, 1984; Stotsky, 1972). Such changes translate to losses in the quality that might be termed "effective fertility" or "plant/soil system resiliency", i.e., the ability of the soil-plant system to withstand environmental, pest, or disease stresses and temporary nutrient shortages. Such losses are difficult to define and measure and go beyond that ascribed to losses of nutrients to harvest and physical deterioration of the soil.

Possibly a more subtle change in the structurally-effective energy inputs in cropped soils arises from differences between the root behavior of crop plants and their non-domesticated counterparts. The effects of these differences in energy inputs will probably become more apparent as the extent or intensity of tillage decreases with the adoption of reduced-tillage practices. Plant species and even cultivar varieties differ in mature root morphology and growth habit (Raper and Barber, 1970). The quality, quantity and nutrient content of root exudates and debris, and the timing of their release into the soil likely differ as well. Breeding efforts have developed high-yield varieties with higher shoot:root ratios and improved abilities to divert photosynthate to the seed head (Mitchell, 1984, p. 30-31). It is likely that in cultivars, compared to progenitors, root residues and their nutrient content have declined. Reduced plant investments of root-derived energy and nutrients in the soil hinder the ability of the plant/soil system to maintain its productivity. One might suspect that the quality of mechanical and chemical energy inputs from the roots in cultivated soils, continuously planted with the same crop, is more restricted than for soils in a rotation which includes extended periods under perennial vegetative cover. More extensive and/or more rapid reductions in the size and diversity of the microflora and loss of system productivity are expected if a natively fertile, well-structured soil is brought under cultivation in a monocultural cropping system than if put into an appropriate rotation system.

SUMMARY

The material and energy flows and control relationships suggested by prior models of soil structure and by the new rhizocentric model of soil structural development are summarized in the following series of diagrams (Figures 4.1 through 4.9). Fig. 4.1 depicts the flows and relationships, implied by prior models, for plant/soil systems in which soil structure is not plant-controlled.

The rhizocentric model suggests two levels of soil structural development, that of rhizocentric soil aggregation and that of plant-controlled soil (super-aggregate) structure. The rhizocentric model suggests that soil structural development is a root-induced biological process during which resource flows and control relationships change. Stages in the soil aggregation process, each with different resource flows and control relationships, can be designated in terms of the life cycle of plant roots. The resource flows and control relationships for a series of such stages are depicted in Figures 4.2 through 4.8.

The rhizocentric model suggests that the soil aggregation process is cyclical, being reinitiated seasonally as root activity resumes. Once a sufficient number of rhizocentric aggregation cycles have been completed, most of the soil particles, particularly the clay particles, are involved in or with rhizocentrically structured aggregates. The cumulative effect of cyclical repetition of the rhizocentric aggregation process is, then, organization of the soil at a super-aggregate level.

The resultant organization of aggregates, non-aggregatable (larger) soil particles, and inter-aggregate/particle macropores is a historically developed, root-induced, phytocentric (directed by and to plants and plant activity), plant-controlled soil structure. The resource flows and control relationships for the soil in plant/soil system with a rhizocentrically developed, structurally plant-controlled, phytocentrically organized soil structure are depicted in Fig. 4.9. Resource flows into, out of, and through the soil are continuously regulated by the present or accumulated structural effects of plant activity. The rhizocentric model also suggests that cultivation interrupts, or eliminates the phytocentricity of the organization of soil particles, and, consequently, plant-root-control of soil processes, allowing untimely and inappropriate microbial activity and resource flows. Ultimately, net losses of plant nutrients occur and soil structure degrades, leading to decreased productivity. Returning structural control of the soil to plants by adopting reduced tillage practies may not cause short-term production gains, compared to tilled soils, if damage to the soil's biophysical condition has been too severe or if rhizocentric soil aggregating activity of crop plants is appreciably less than or different from that in locally well-adapted plant/soil systems.

AUTHOR'S NOTES: It should be mentioned that there is no intention to imply in this presentation of the rhizocentric model that soil structural units which may also be procedurally definable, hence, usefully describable, as "water-stable soil aggregates" cannot form through other processes. It is intended, however, to portray rhizocentrically formed and organized aggregates as having unique, plant-control enabling, biophysical properties which are determined by their organization and composition and that this organization and composition obtains only as the consequence of root activity of certain plants, particularly certain grasses. It is further proposed that rhizocentric aggregate formation is the principal process of structural development in the surface soil of stable and productive grassland plant/soil systems.

Also, as the author, I wish to emphasize that the rhizocentric soil aggregate formation/structural development process is a continuous, biologically induced and controlled process, not an incrementalized, mechanistic process as might be suggested by the following series of sequential diagrams purporting to depict different stages in the process. The diagrams are naively simple and the depicted stages arbitrarily defined to facilitate presentation of the rhizocentric concept of soil structural development. In real plant/soil systems, the onset, duration, intensity, and spatiotemporal extent of the lower-level processes suggested as occurring in the different stages could vary considerably among and within plant/soil systems in which plant-control is achieved through rhizocentric control of soil structure. Further, it is my opinion that rhizocentric soil structural development is an evolutionary, irreversible process not likely subject to reliable mathematical description or numerical simulation. I recommend against any attempt, which the following diagrams might seem to invite, at numerical simulation modeling of the rhizocentric soil structure concept. I believe such efforts would violate the premises and purposes upon which the rhizocentric model is based. Indeed, I am concerned that these diagrams do not meet the linguistic requirement that scientific descriptions be rendered only in languages capable of describing the subject of study unequivocally, that the presentation of these digrams violates the principles of wholistic reasoning (see the Appendix), and include them only for purposes of summary and upon the advice of others.

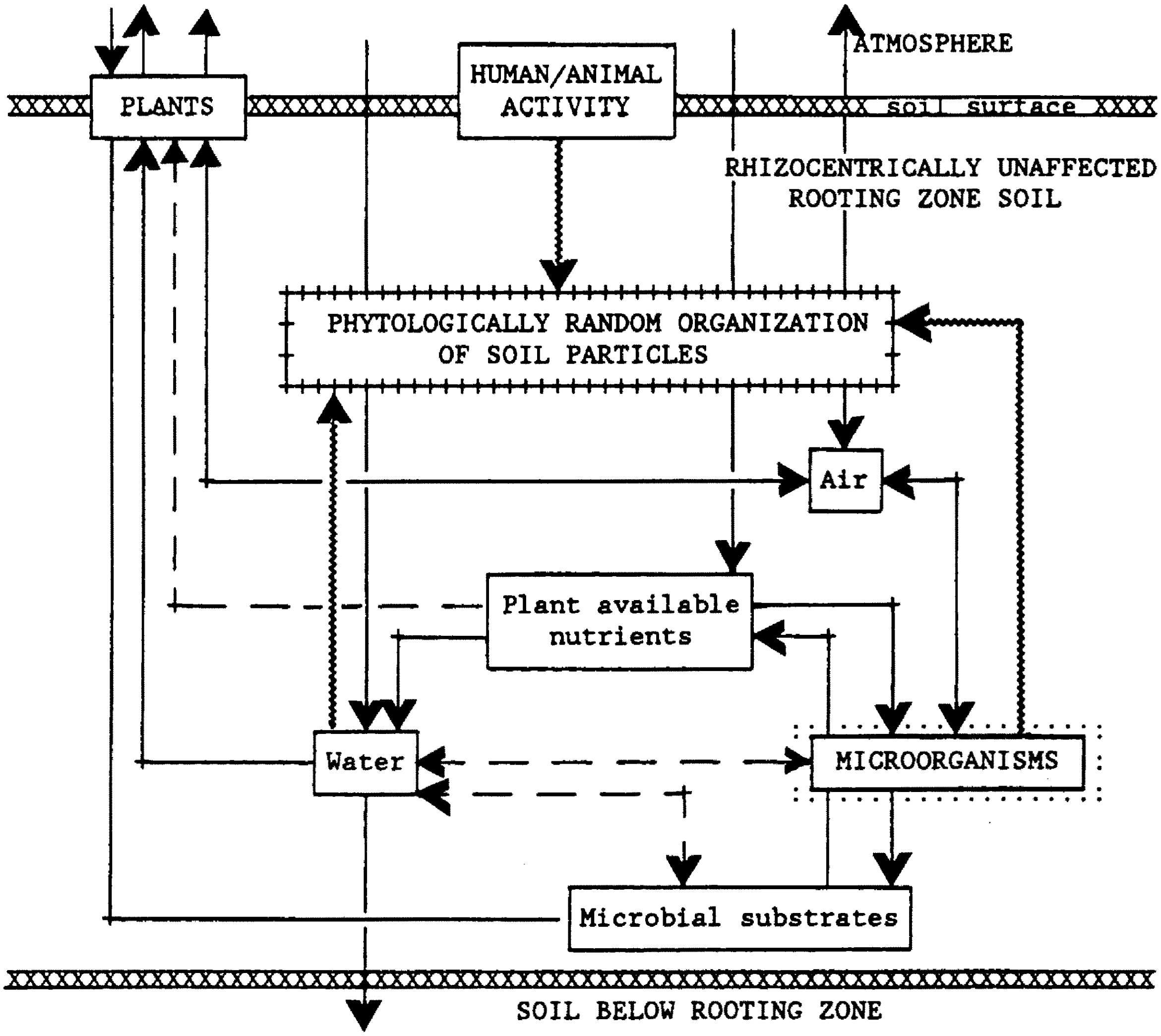


Fig. 4.1. Material (→) and energy (↝) flow diagram for a soil the structure of which is not plant-controlled. (Relatively larger flows or more effective control connections are depicted as solid lines, smaller or less effective by broken lines.) Plants are principally consumers of water, air, and plant nutrients. The behavior of the plant/soil system is controlled by the character (mineralogy, size distribution, organic matter content, etc.) of the soil and the organization of soil particles (soil structure) which is determined by the energy inputs from the environment (principally through the actions of water), animals, humans, and microorganisms. This diagram represents the situation in sparsely plant-inhabited, highly tilled, or recently exposed soils and summarizes the more important material and energy flows and control relationships assumed, expressed, or implied by conceptual models of soil structure prior to the rhizocentric model. Water is the primary transport medium. The movement of water and the resources it carries is governed largely by the soil structure, which is not directly influenced by plant activity.

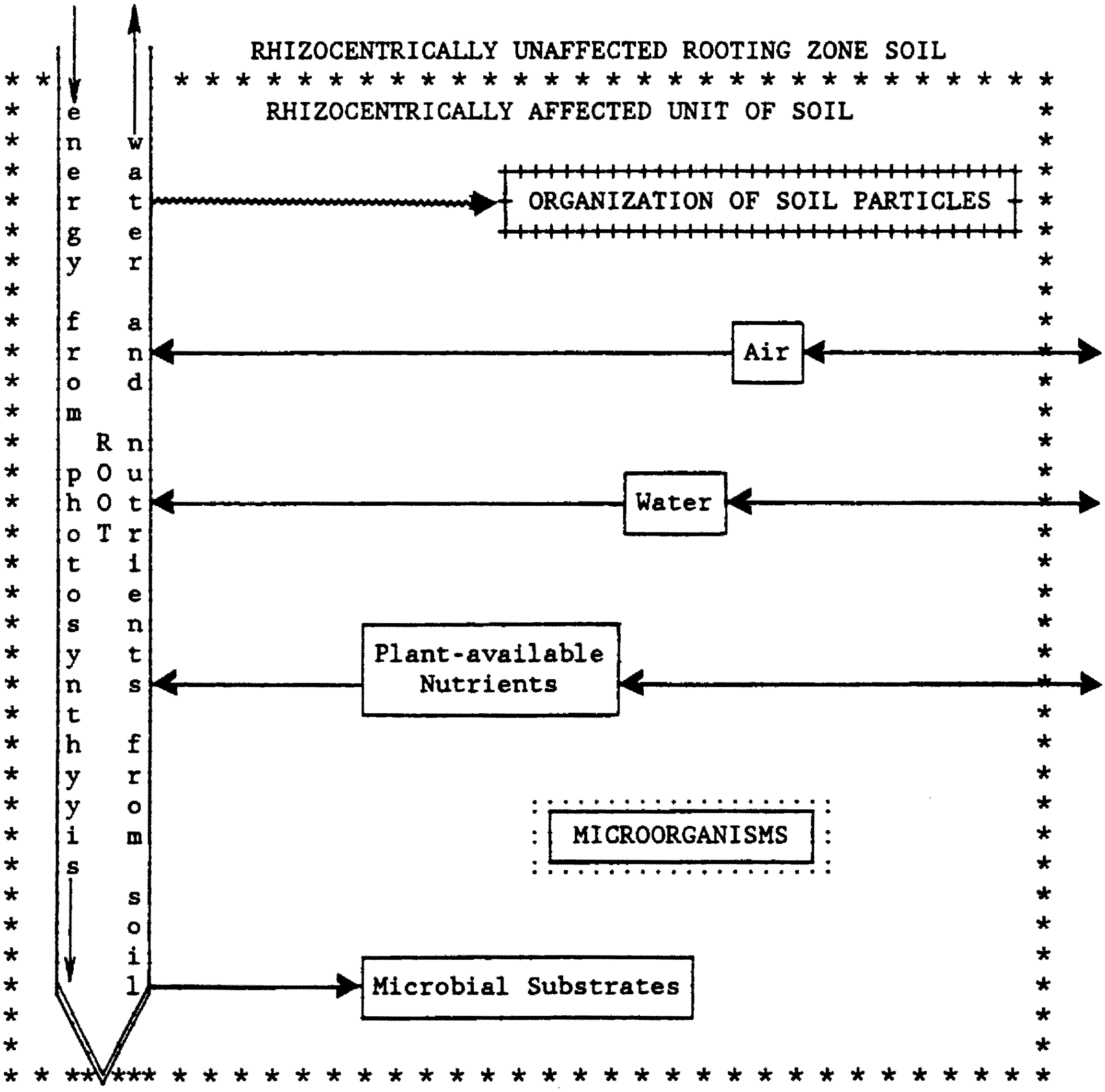


Fig. 4.2. Material (→) and energy (⇝) flow and control (∘∘∘∘∘∘∘∘) diagram for the first stage in the development of a rhizocentrically structured soil aggregate. (Relatively larger flows or more effective control connections are depicted as solid lines, smaller or less effective by broken lines.) A root has just penetrated a volume of soil that will become a rhizocentric aggregate. Uptake of plant-available nutrients by the passing root tip is intense. The root displaced particles from its path during its penetration of the soil, resulting in compaction of the surrounding soil matrix, but not a sufficient alteration of the organization of soil particles to measurably affect (relative to the soil outside the rhizocentrically affected volume) the mass flow and diffusion of water, air, and nutrients through the soil to the root, or the movement of microbial substrates from the root into the soil. The microorganisms present in the soil volume have not yet had time to respond to the substrates being released by the root.

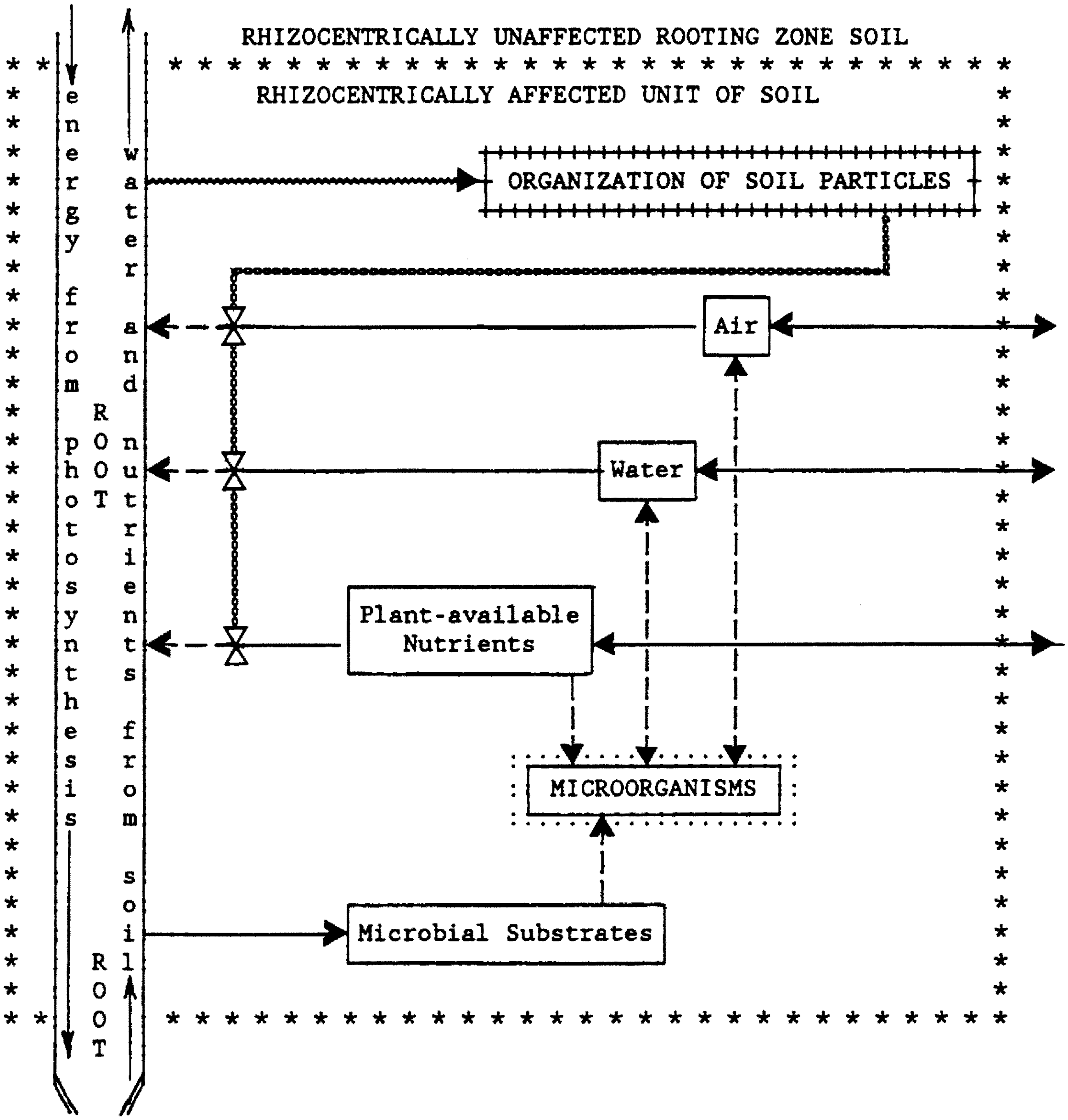


Fig. 4.3. Material (—▶) and energy (〰▶) flow and control (∘∘▷◁∘∘) diagram for the second stage in the development of a rhizocentrically structured soil aggregate. The root has extended through the volume of soil. The effects of the root activity have altered the organization of soil particles to affect the diffusion and mass flow of material within the soil volume under the root's influence. The accumulation of clays carried by water mass flowing to the root, and root-released organics within the root-compacted matrix of larger soil particles near the root surface results in the partial inhibition of the movement of water, air, and nutrients to the root. The microorganisms have begun to respond to the substrates released by the root, competing weakly with the root for air and what little remains of the supply of plant-available nutrients.

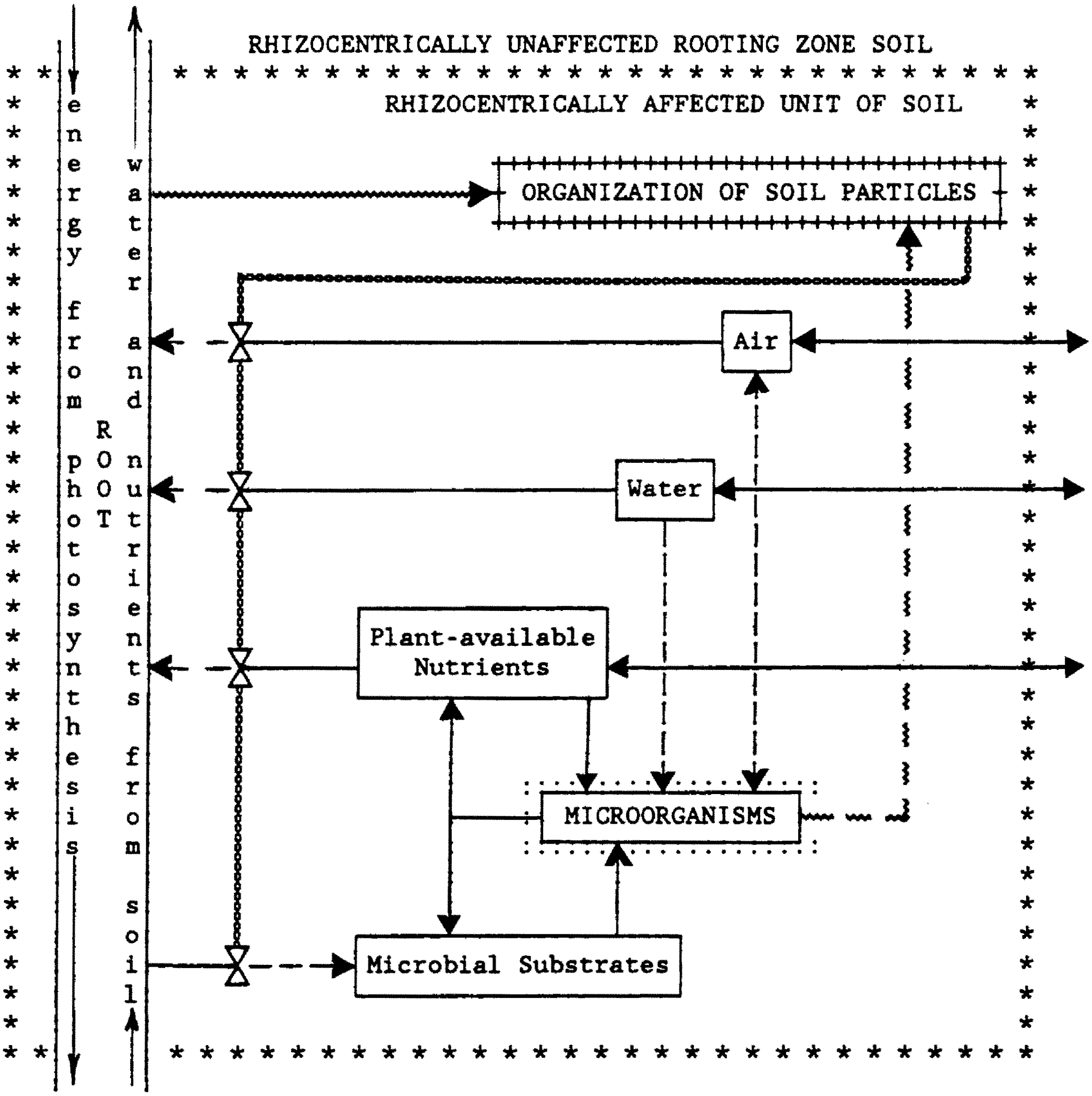


Fig. 4.4. Material (⟶) and energy (⟿) flow and control (⋈) diagram for the third stage in the development of a rhizocentrically structured soil aggregate. The portion of the root within the volume of soil has now matured. Enough clays have accumulated in the soil matrix near the root surface to begin inhibiting the flow of nutrients, air, and water to the root, and microbial substrates from the root. The microorganisms respond to the high energy availability and low nutrient availability by releasing enzymes which cause release of plant-available nutrients from soil organic matter (considered part of the microbial substrates pool). The root is still an able competitor for some of the nutrients released by the microbial activity.

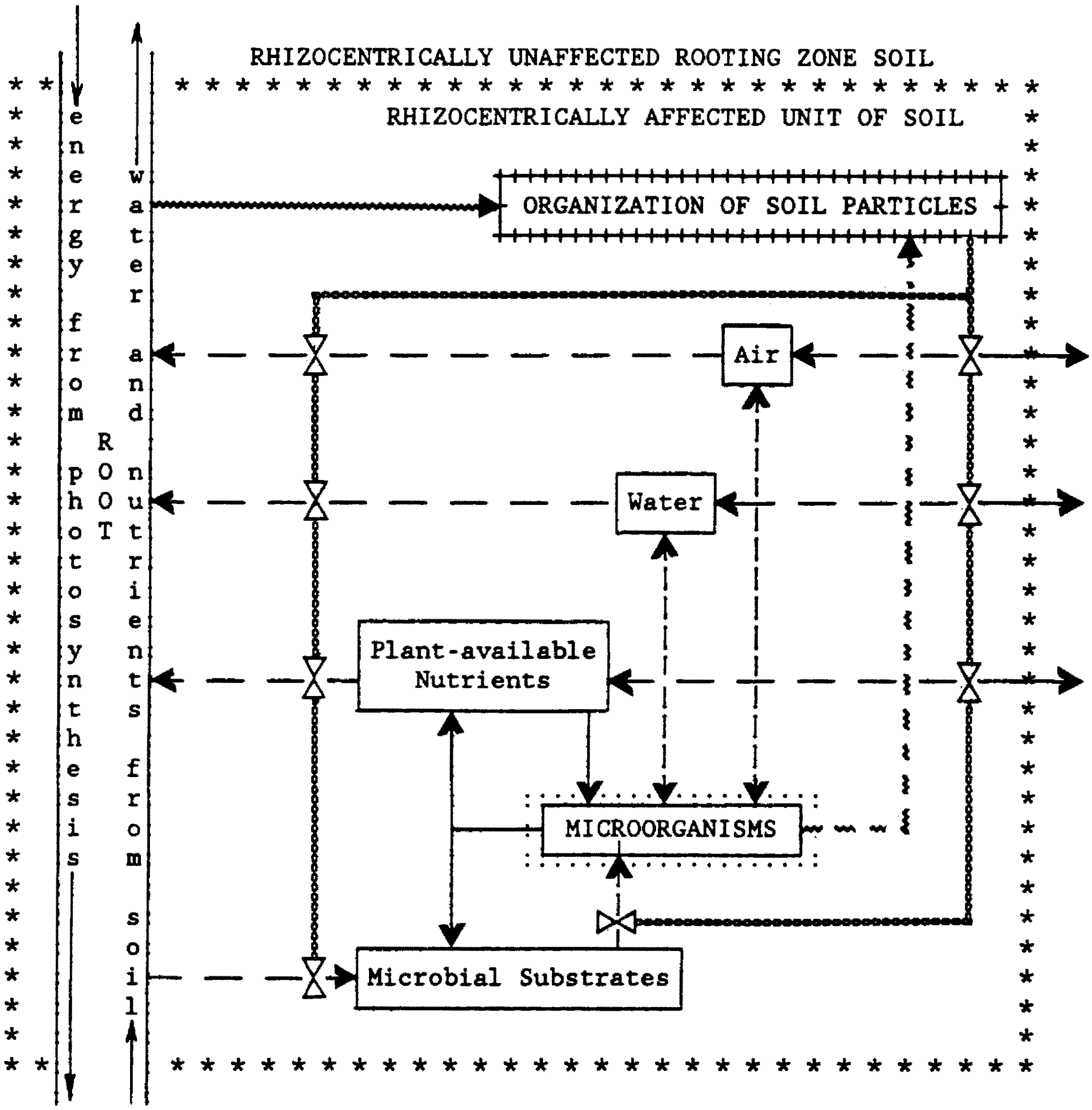


Fig. 4.5. Material (→) and energy (⇝) flow and control (⋈) diagram for the fourth stage in the development of a rhizocentrically structured soil aggregate. Clays have accumulated in the soil matrix near the root surface to the point that transfer of materials to and from the root is strongly inhibited. Little, if any, more clay accumulation will occur. The zone of rhizocentric organization of soil particles has now extended far enough that the microorganisms utilizing the root-released substrates are no longer able to access air that is freely exchanging with that in the soil outside the rhizocentrially affected volume. Anaerobic microbial activity, which may have begun much earlier, becomes more important. Some of the microbial biomass produced earlier dies, its content of plant-available nutrients and microbial substrates being retained within the structurally diffusion-constrained interior of the forming aggregate.

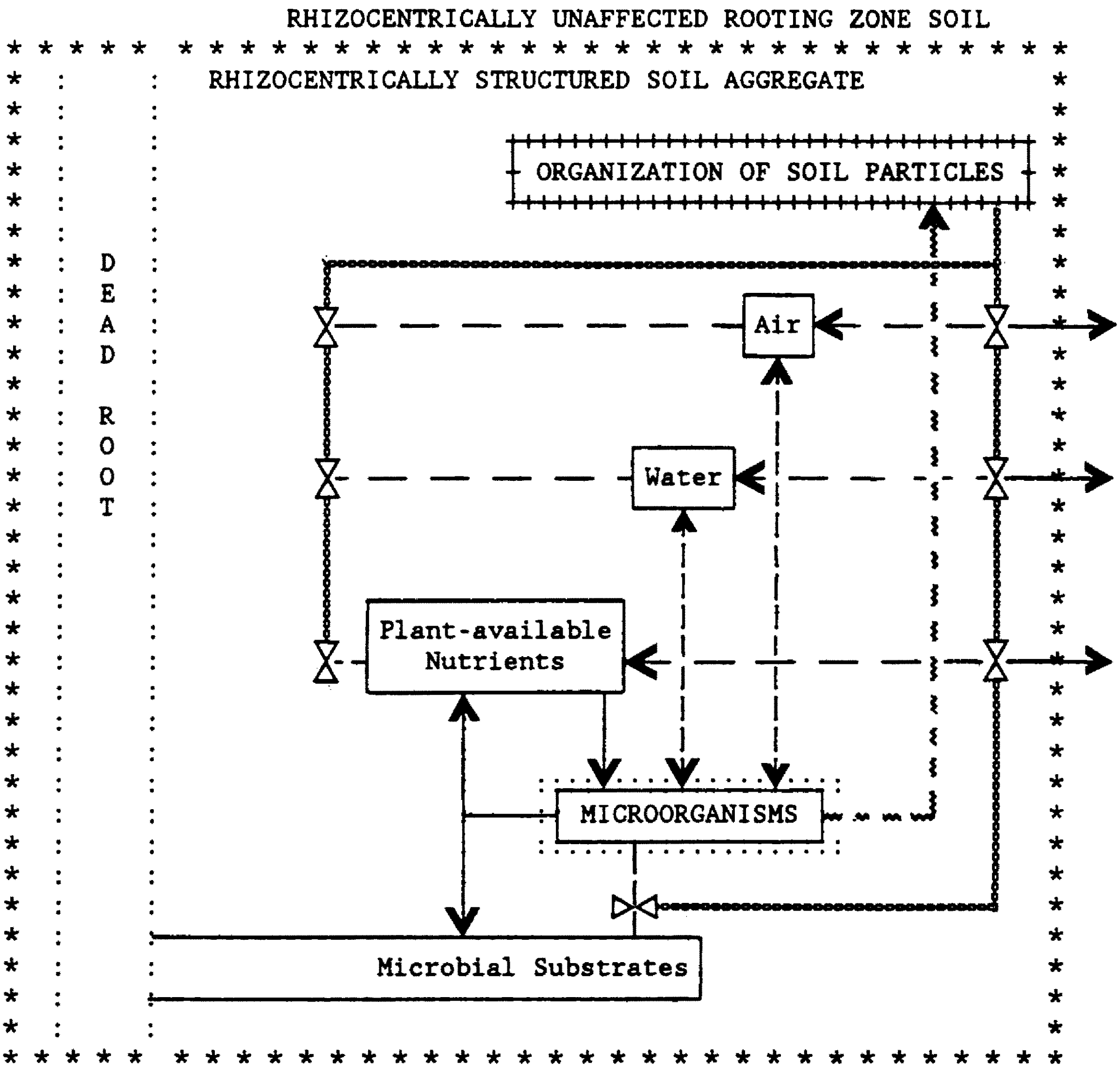


Fig. 4.6. Material (⟶) and energy (⇝) flow and control (⋈) diagram for the fifth stage in the development of a rhizocentrically structured soil aggregate. The root is now dead, its remains now microbial substrates. The interior of the forming aggregate is now so well isolated from the external soil environment that microbial decompositon of the root residues and other microbial substrates proceeds mostly through anaerobiosis. The products of the anaerobic decomposition processes accumulate within the aggregate as anaerobic metabolite and biomass residues.

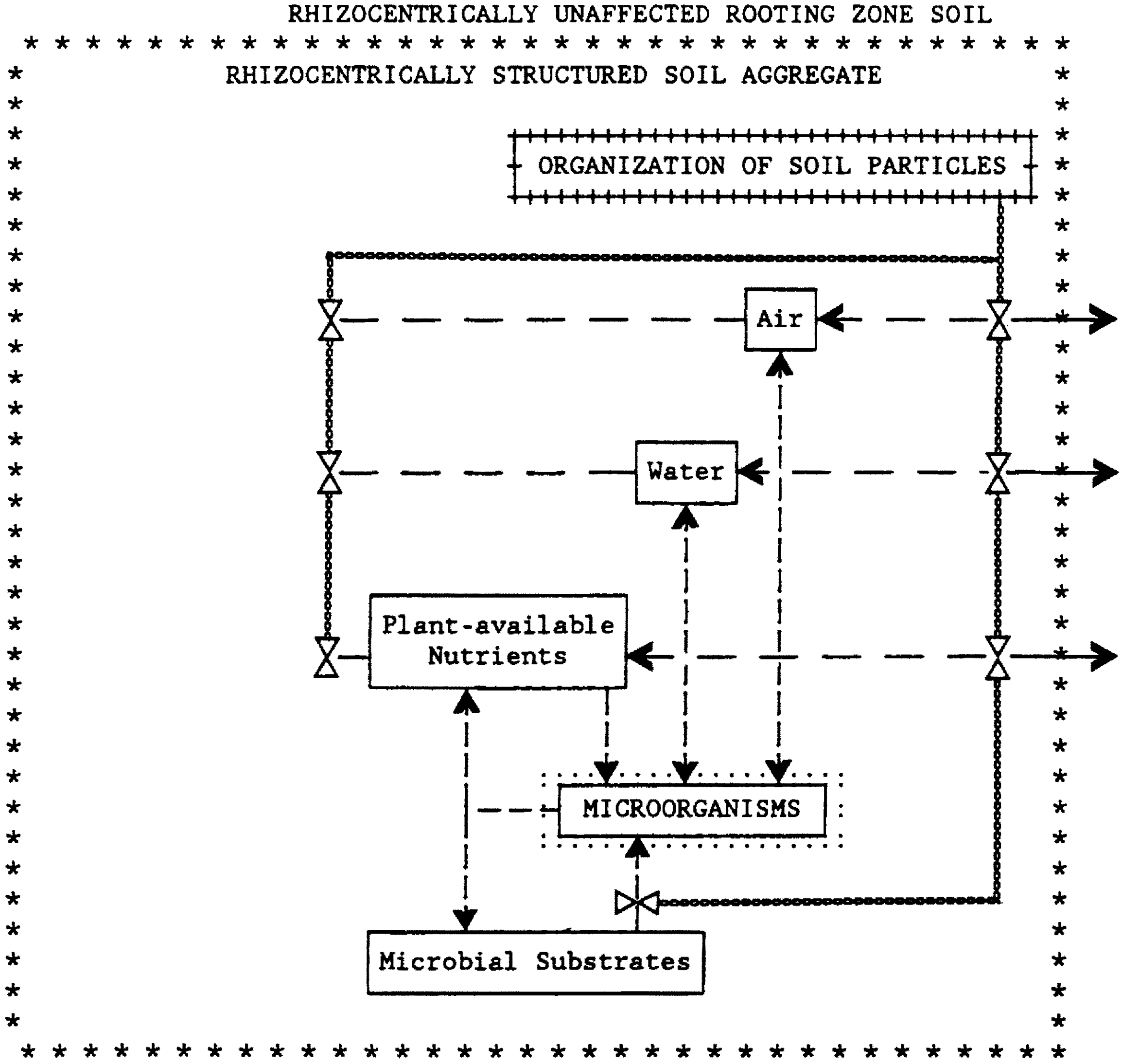


Fig. 4.7. Material (——▶) and energy (∞∞∞▶) flow and control (∞▷◁∞) diagram for the sixth stage in the development of a rhizocentrically structured soil aggregate. In the fully matured rhizocentric aggregate almost all the root residue have been converted into anaerobic microbial biomass, metabolites, or product compounds. The diffusion limiting organization of soil particles in the rhizocentric aggregate protect the accumulated anaerobic organic products and residues from exposure to the air and the aerobic degradation processes which would ensue upon such exposure. The anaerobic residues act as cementing agents to maintain the stability of the aggregate, which may remain intact in the soil matrix for years if the rhizocentric organization of soil particles is not disturbed.

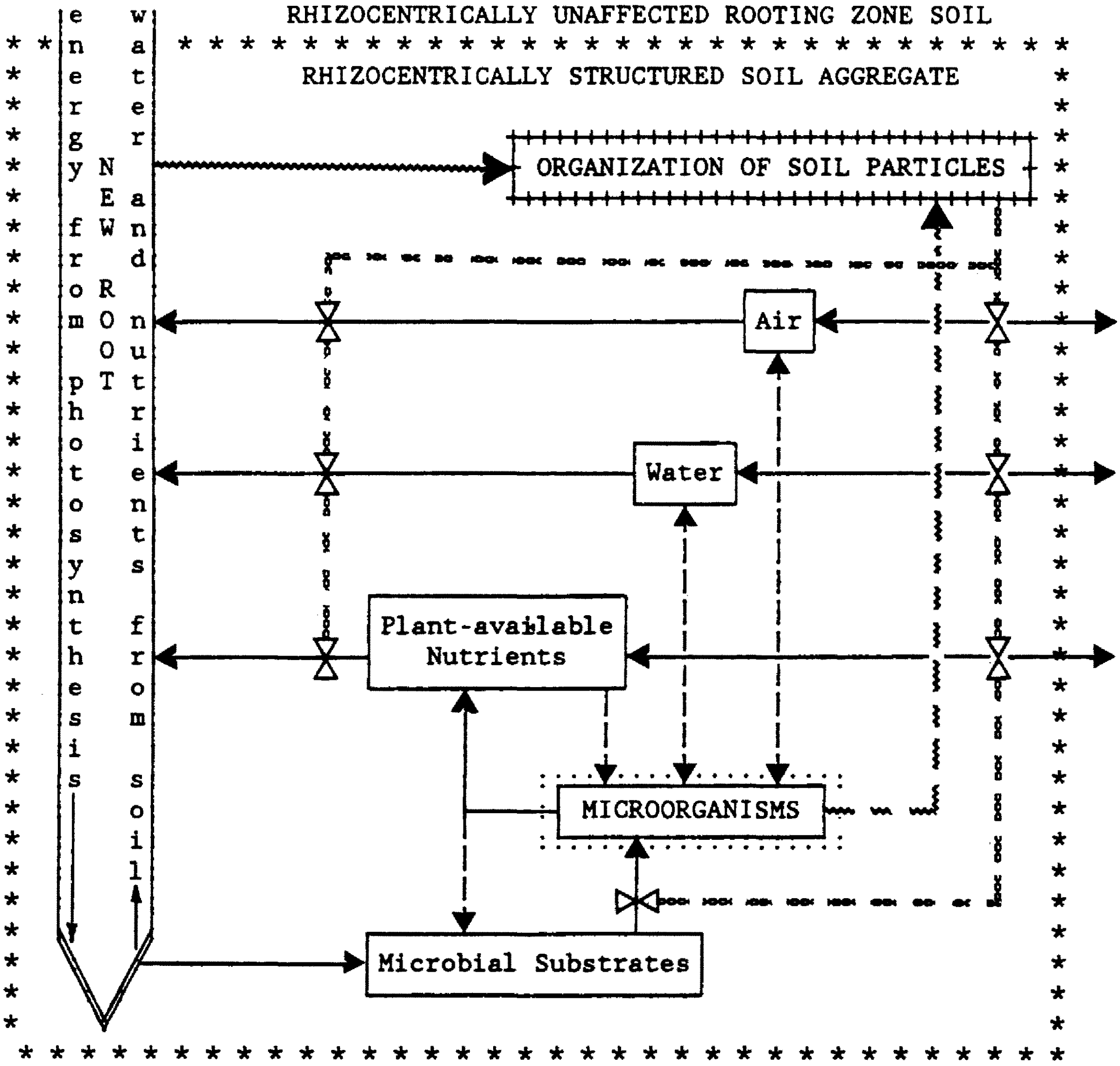


Fig. 4.8. Material (⟶) and energy (⟿) flow and control (⋈) diagram for the seventh stage in the development of a rhizocentrically structured soil aggregate. The rhizocentric aggregate is stable within the soil matrix, but plastic when moist, allowing a passing root to disrupt the diffusion limiting structure of the aggregate. Disruption exposes the accumulated anaerobic residues to an aerobic environment in which they are rapidly decomposed, releasing plant-available nutrients, much of which will be taken up by the passing new root. Decompositon of the organics enhances the release of the already disturbed clays to be rhizocentrically reorganized by the new root, continuing the cyclical structural process. As the rhizocentric structural cycle is repeated, more and more of the clay in the soil becomes involved in rhizocentric aggregates and the range of interaggregate structural arrangements becomes limited. At the structural limit, a maximally plant-controlled, phytocentrically organized soil structure obtains.

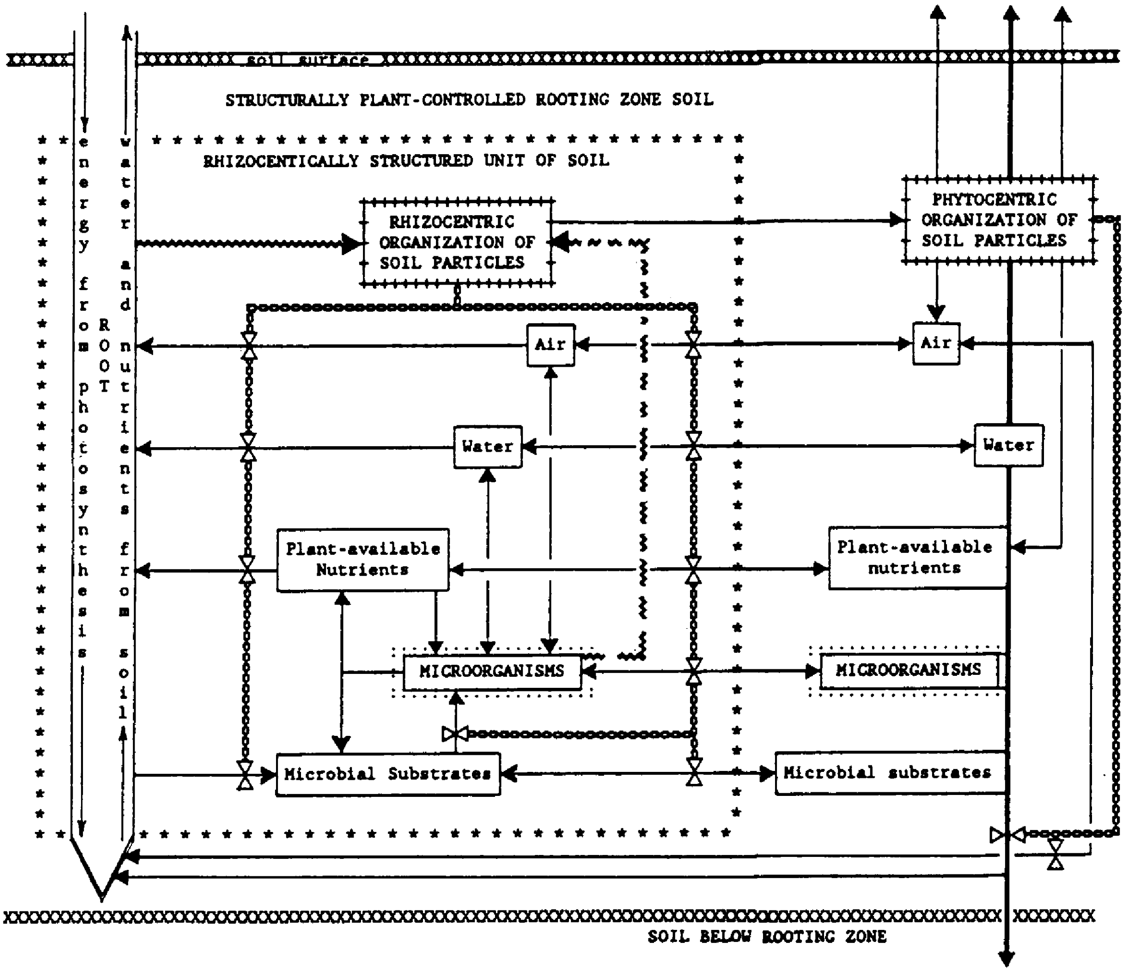


Fig. 4.9. Material (⟶) and energy (⟿) flow and control (▷◁) diagram for a rhizocentrically developed, structurally plant-controlled, phytocentrically organized soil. Plants are the principal suppliers of energy and consumers of energy and material resources in the plant/soil system. The energy inputs associated with root activity induce rhizocentric organization of soil particles, which enables formation of rhizocentrically structured aggregates and a phytocentrically structured soil, in which the plant-related effects of soil phenomena are optimized. For example, the moisture-sensitive microporous structure of rhizocentrically structured aggregates allows storage of of plant-available water, but inhibits diffusional exchange of biophysically plant-accessible nutrients in their interiors with the water and air in the interaggregate macropores. The interaggregate macropore system endows the soil with desirably high permeability to air and water, but nutrients not stabilized against events in the macropore system would be subject to loss from the plant/soil system in any mass flow of macropore water out of the rooting zone. Hence, rhizocentric aggregate structure and phytocentric soil (super-aggregate) structure, in conjunction, allow nutrient losses to be minimized while moisture use efficiency is maximized.

Chapter 5. STRUCTURALLY ENABLED SELF-REGULATION IN PLANT/SOIL SYSTEMS: TESTING THE PLANT-CONTROL HYPOTHESIS AND RHIZOCENTRIC MODEL

INTRODUCTION

The life system on earth depends on the unique photosynthetic and chemosynthetic capabilities of plants, and human survival is dependent on the continued productive success of a certain few of those plants. Agriculture began about 100 centuries ago when humankind discovered that by modifying the environment it could increase production of those most desirable or directly useful plants. Several specific environmental factors that influence plant growth have been identified over the last 100 years. Specific soil environmental factors include moisture supply, soil structure and aeration, soil reaction (pH), and the supply of mineral nutrient elements. Agricultural practices modify the environment by manipulating the factors, but the interplay among them ultimately controls production, and such interactions remain poorly understood (Tisdale and Nelson, 1975, p. 22-51).

The "plant-control" hypothesis (PCH) and associated "rhizocentric model" (RM) of soil structural development provide new conceptual vantage points from which to observe and consider the behavior of plant/soil systems. The PCH is that stable plant/soil systems must control the interactive environmental factors that affect plant growth --otherwise they could not be stable -- and that such control can be accomplished by physically organizing the system in such a way as to control the biological utilization of energy. The RM suggests how plants induce the formation of a phytocentric soil structure which allows plants to control the biological utilization of energy substrates even after they have been released from the biomass which they constituted or in which they were produced. The PCH and RM might lead to a practical understanding of the interplay among the factors that influence plant growth and to development of a more stable and productive agriculture.

The PCH/RM are new, however, and the extent of their credibility is yet to be established. Examination of the naive representation of the RM in Figures 4.2-4.8 reveals that the variables and parameters which would have to be measured to test the validity of the RM independent of the PCH are only very difficultly measured (for example, the movement of gases within an unstable soil structural unit), or not quantifiable (e.g., "organization of soil particles"). Indeed, during the early stages of formation not even the boundaries of the "rhizocentrically affected unit of soil" can be materially identified in any practical sense. Similar problems affect testing the PCH independent of the RM. The PCH and RM in conjunction (PCH/RM), however, imply certain consequences (predictions) which are testable. Testing of PCH/RM predictions can be based on a single assumption about the conditions which must be met if a plant/soil system is to be stable and productive "into perpetuity" (USDA-ARS, 1983). That assumption is: resource outputs from the system cannot exceed inputs to the system. Fig. 4.9 suggests some of the parameters which might be examined in order to test some the PCH/RM concept. Essentially these involve any material which may be transported into or out of the soil by plants or mass or diffusive flow of gases, liquids, or solids, and relationships among plant activity, soil structure, and the variables that the RM concept suggests may be affected by structure (among others, soil permeability and erodibility, mobility and structural distribution of soil colloids, chemical character of soil organic matter, availability of plant nutrients encountered natively in the soil).

This chapter presents preliminary and indirect examinations of the credibility of the PCH and RM through examination of the ability of the higher level PCH/RM concept to deal with existing data, presents some of our own experiences using these new ideas (Norstadt and Payne, 1984, 1985; Payne and Norstadt, 1984, 1985), and illustrates their logical consistency. The examinations also serve to demonstrate the predictive and interpretive usefulness of the PCH/RM in the analysis and interpretation of data from published studies concerned with the environmental factors affecting plant growth. The discussion is oriented toward temperate grassland and agricultural soils, under traditional agronomic crops or vegetation dominated by grasses. Although details of the RM, and necessarily then the PCH/RM, will differ for plant/soil systems dominated by other types of vegetation, it is suggested that plant-control and the role of structure in enabling plant-control (i.e., the PCH) are generally applicable.

It should be noted that the RM is the more specialized (lower level) concept, and that predictions made using the PCH/RM are restricted to the range of the RM (narrower than the range of the PCH). Consequently, demonstrations that PCH/RM predictions are incorrect should be regarded as refutations of the RM, but not necessarily of the PCH. On the other hand, demonstration of the validity of PCH/RM predictions must be regarded as supporting (but not proving) both the RM and PCH.

SOIL STRUCTURE

According to the RM, plants determine soil structure because their roots control, through space and time, the physical and biological conditions affecting soil structure. It is especially important that root activity affects fine soil particles, most notably clays. The RM holds that the sum total of root activity continuously disperses clays, moves the clays to and re-arranges them about the active root, and stabilizes the rhizocentric arrangement. When soil structure is root-controlled, none of the sequence of clay-affecting steps occurs independently, and clay dispersal (as a consequence of root activity) is the rate limiting step. Consequently, there are no dispersed clays during periods of root inactivity. Thus, the location and arrangement of clay particles is closely controlled, optimizing expression of the beneficial qualities of clays while minimizing their potentially harmful effects.

One may predict, based on the RM, that any interruption of the continuous, cyclical, rhizocentric soil-structuring process will increase the quantity of readily-dispersible clays in the soil. Oades (1984) cited results indicating an increase in dispersible clay due to cultivation. Dormaar's (1983) study of the effects of cropping to spring wheat on the water-stable aggregates in a Typic Haploboroll also agree with this prediction. Mass balances for the particle size fraction data reported by Dormaar (Table 5.1) show that interruption of the rhizocentric structural cycle (by tillage and cropping) immediately increases the amount of water-dispersible clays, and that despite important increases in soil aggregate stability over winter, the effect is cumulative -- water-dispersibility of clay was greater in wheat-

Table 5.1. Water-dispersibility of clays in a Typic Haploboroll soil after 68 years of wheat production. Data from Dormaar (1983).

Cropping pattern	Water-dispersible clay (% of total clay in soil)[1]	
	Pre-plant[2]	Post-harvest[3]
Crop-fallow		
following wheat	26	42
following fallow	32	46
Continuous cropping		
wheat	2	16
Native prairie mixed species	0	0

[1] Water-dispersible clay equals the total soil clay minus the clay in water-stable aggregates.

[2] Sampled in April 1980.

[3] Sampled in August 1976.

fallow soils, where plant-control was interrupted for longer intervals than in continuous wheat. In contrast, all clay in the native prairie soil was stabilized the year round.

Colloid Movement

The RM postulates that water flowing to the an absorbing root carries colloids that accumulate and become organized about it (see the electron micrographs of Campbell and Porter, 1982). Eventually, the accumulated colloids block further water movement to the root. One would predict, then, for a given soil, a limited range of clay contents in rhizocentrically formed water-stable aggregates. Referring again to Dormaar's (1983) data (Table 5.2), water-stable aggregates in soils cropped to wheat averaged 27.6% clay (range 22-35%), those under native prairie averaged 28.3% (range 26-30%), while whole soil contained only 16-18%. Dormaar's data indicates an elevated clay content may be an intrinsic property of water-stable aggregates, unrelated to soil or aggregate organic carbon (C) content, extent of soil aggregation, apparent aggregate age (before vs. after fallow), or sand or silt content of the aggregate.

Table 5.2. Characteristics of soil and water-stable aggregates after 68 years of wheat rotations on a Typic Haploboroll. Data from Dormaar (1983).

	Cropping patterns			
	Crop-fallow rotation		Continuous cropping	Native prairie
	fallow	wheat	wheat	mixed species
	$g\ kg^{-1}$			
	Whole soils			
Clay	170	180	160	170
Sand	420	380	400	410
Organic carbon	13.2	13.9	19.5	30.8
Water-stable aggregates	436	485	541	632
	Water-stable aggregates			
Clay	264	277	288	283
Sand	503	521	525	422
Organic carbon	16.1	17.4	16.2	27.1

Using the RM one can anticipate that infiltration problems can develop due to loss of control of clay-sized particles when the rhizocentric structural cycle is interrupted. Tillage operations, for example, interrupt the structural cycle, randomize the arrangement of soil particles, and temporarily increase the macropore volume. Flowing water moves dispersed clays through the network of tillage-induced macropores until the character of the pore system (and thus water flow) changes. Such change occurs at the interface between the plow layer and the underlying unplowed soil. At the interface, the tillage-induced, mechanically organized macropore system of the plow layer meets the root-induced, biologically organized pore system of the undisturbed soil beneath. Decreased flow rates and a filtering effect at this interface of mechanically and biologically organized soil bodies cause deposition of the dispersed clays. Continued deposition eventually forms an essentially impermeable layer. The RM indicates that these massive structures, known as plow pans or "compaction layers", will develop in plowed soils having adequate clay regardless of tillage implement compressive loads on the soil.

Structure Affects Shoot:Root Ratios

Another effect of increased clay dispersibility is suggested by the RM. The postulated rhizocentric clay accumulation is self-limiting, ending when the accumulated clays block water flow to the root. The faster the active root surface accumulates clay, the sooner water and nutrient diffusion to the root will end. Therefore, given adequate supplies of nutrients, moisture, and air (or other factors which can affect rate and extent of root growth), a plant growing in a coarse-textured or well-structured soil -- soils with less readily-dispersible clay -- will not require as large a root system as the same plant growing equally vigorously in a heavier textured or poorly-structured soil.

Norstadt and McCalla (1971) grew Lee variety spring wheat (Triticum aestivum L.) to maturity in pots containing quartz sand or Holdrege silt loam soil from a cultivated field. The soil was fertilized initially at levels indicated by soil tests and the sand was flushed weekly with a nutrient solution. Pots were watered three times per week. Because different methods were used to add nutrients in the two treatments, it is possible that differences in plant growth between the two treatments might have been due to differences in nutrient supplies. Indeed, at harvest the plants (grain + straw + roots) grown in sand contained 38% more nitrogen than those grown in soil. However, most of the increased nitrogen in the sand treatment was due to an increase in the nitrogen content of the straw. The nitrogen contents of the roots and grain were not significantly different between the two treatments, and the mass of grain produced was not different (Table 5.3). Further, in the soil treatment the total nitrogen recovered in the plants accounted for 54% of the nitrogen added as fertilizer, without considering that taken up from native soil (non-fertilizer) nitrogen, likely a significant source. So, although it is not certain that the plants in the two treatments were exposed to the same concentrations of nutrients, it appears that in both treatments the

Table 5.3. Yields and shoot:root ratio of spring wheat grown in texturally different rooting media. Data from Norstadt and McCalla (1971).

Rooting medium	Yield[1]			Shoot:root ratio
	grain	straw	roots	
	------------	g plant^{-1}	------------	
Sand	0.71	1.21	0.13	9.3
Silt loam	0.74	0.91	0.17	5.4

[1] Values are means of 32 plants.

plants were exposed to nutrient levels which permitted at least maximum grain production (for the experimental culture conditions), and perhaps some luxury consumption. Despite the apparently likely adequacy of nutrient supplies in both treatments, the plants grown in soil had 25% more root mass and 33% less shoot mass than those grown in sand. Plants grown in soil had a shoot:root ratio of 5.3 while the ratio for those grown in sand was 9.4.

The data of Merckx et al. (1985) show a similar effect of texture in the shoot:root ratio of wheat (T. aestivum L. var. Sicco) grown in pots containing a sandy or a silty clay loam soil. The plants were not grown to maturity, the experiment being terminated when the plants were 42 days old. At the end of the experiment wheat plants grown in the sandy soil had a shoot:root ratio of 1.1, while those grown in the silty clay loam had a ratio of 0.73.

There are field data suggesting the predicted effect of more (dispersible) clay on root mass, also. Reduced tillage practices generally improve soil structure and presumably reduce the dispersibility of soil clay. (No-till cropping systems are of particular interest, because their soils are structurally controlled by plants yet lack the potentially confounding effects of the continuous presence of live, perennial roots.) Meisinger et al. (1985) noted that conventional-tillage corn produced 10% less dry matter than reduced-tillage corn. Those authors proposed that fertilizer application rates should be higher for reduced tillage because more plant biomass requires more nitrogen. Hargrove (1985) reported that no-till corn plants were larger and that their roots were more active (as indicated by rubidium tracer technique) than corn plants grown in conventionally tilled soil.

Organic Matter and Soil Structure

Different effects of above- and below-ground plant residues on organic matter (OM) and soil structure are foreseeable from the RM. It is obvious that above-ground residues are only indirectly related to desirable soil structure. There is (to this author's knowledge) no proposed mechanism which would enable leaf and stem residues, even when plowed into the soil, to relocate and reorganize enough clays to form stable soil aggregates with the unique biophysical properties of rhizocentric aggregates, properties proposed here as essential to the soil structure of many stable and productive plant/soil systems. Only the biophysical effects of root activity and root residues are necessary to, and capable of, inducing the development and maintenance of such a favorable soil structure.

Skidmore et al. (1986) studied the effects of winter wheat (*T. aestivum* L.) and sorghum (*Sorghum bicolor* <L.> Moench) cropping and residue management on soil physical properties. Leaf and stem residues were hauled or burned off, or were incorporated in the amount produced by the crop or at twice that amount. Except for differences in the time of planting, harvest (and a post-harvest discing operation associated with residue treatments) and fertilizer application -- necessitated by the different seasonality of the two crops -- the treatments were identically (conventionally) tilled. After 13 years, there were no significant differences among the residue management treatments in content of aggregates >0.84 mm in diameter, wet or dry aggregate stability, bulk density, aggregate density, or aggregate size distribution. However, all measured properties differed between soils cropped to sorghum and those cropped to wheat (Table 5.4). Soil structure was essentially unaffected by the presence (or absence) of incorporated above-ground residues, as anticipated from the RM.

Table 5.4. Structure-related properties of a relatively uniform silty clay loam (Aridic Argiustoll) after 13 years of cropping to wheat (Triticum aestivum L.) or sorghum [Sorghum bicolor (L.) Moench]. Data from Skidmore et al. (1986).

Structure-related soil property	Crop		
	Wheat	Sorghum	LSD (0.01)
Aggregates >0.84 mm (%)	70.1	47.1	2.6
Wind erodibility index [Mg/(ha/yr)]	27	101	17
Dry aggregate stability (%)	88.9	83.7	1.6
Wet aggregate stability (%)	39.6	52.5	7.1
Bulk density (Mg/m^3)	1.23	1.05	0.07
Rupture stress (kPa)	312	216	52
Aggregate size distribution (geometric mean diameter, mm)	2.86	1.18	0.23
Saturated hydraulic conductivity (micrometers per second)	6.7	49.4	14.4
Organic matter content (g/kg)	18.6	20.7	0.9

Also, one can predict from the model that tillage does not, in practice, eliminate rhizocentric plant-control of soil structure, but limits the extent and effect of that control. If tillage eliminated rhizocentric control of soil structure, then there should have been no difference between Skidmore et al.'s wheat-cropped and sorghum-cropped soils, since all soils were similarly tilled (Hooker et al., 1982). The soils, however, did differ in all measured physical properties -- differences that result, the RM explains, from differences in the two crops' root activities. Further, the wet stabilities were typical of tilled soils (Feng and Browning, 1946; Kemper, 1966; Malik et al., 1965), undisturbed soils characteristically having higher stabilities. Thus, plowing does not eliminate plant-control of soil structure but diminishes the extent through space and time, and consequently the effectiveness of that control.

Roots, Aeration, and Structure

Adequate root aeration is essential to the health of upland plants. The RM postulates that the clay/organic matrix, which envelops mature roots, functions as an effective diffusion barrier, limiting root access to soil air. Therefore the mature roots of upland plants should be tolerant of low oxygen (O_2) levels. Such tolerance is graphically apparent in Huck's (1979) time-lapse study of cotton (Gossypium sp.) root growth. Oxygen deprivation was fatal to an immature root, but mature roots behind dead tips apparently resumed normal activity after O_2 supplies were restored -- even after several hours of O_2 deprivation. The sensitivity of the immature root is accomodated by the RM that indicates the tip and immature root do not accumulate sufficient clay in well-structured soils to reduce gas diffusion rates to injurious levels.

Luxmoore et al. (1970) also found upland plants have a good tolerance of low O_2 levels. They estimated from their study of root respiration that the O_2 concentration at which respiration was half-maximum to be 16% O_2 for rice roots, but only 8% for corn roots. Their data further suggests that corn root respiration was not as well adapted to high O_2 concentrations as rice root respiration. Roots were sectioned at uniform intervals beginning at the root tip. When O_2 levels were increased from 20.8% to 80%, respiration increased in all rice root sections. Among the corn root sections, only the tip showed a markedly increased respiration rate while 4 of the 9 other corn root sections showed decreased respiration rates when O_2 levels were changed from 20.8% to 80%. The results clearly imply a greater tolerance, if not respiratory adaptation, of corn to lower O_2 levels than rice -- an apparent paradox, resolved if, as suggested by the RM, the formation of a diffusion-limiting clay/organic envelope about the roots of upland monocots is a normal occurrence.

MINERAL NUTRIENTS

Soil aeration has marked effects on the plant-availability of nutrients. Anaerobic conditions can enhance processes, like denitrification, which reduce nutrient supplies. On the other hand, availability of certain nutrients, iron and phosphorus for example, may be increased under anaerobic conditions (Bohn et al., 1979, p. 250-270). Organic matter is less extensively decomposed and thus stabilized under anaerobic conditions. The PCH suggests that plants have evolved mechanisms for structuring plant/soil systems to optimize the availability and stability of nutrient supplies. The RM suugests that the optimization in grasslands is accomplished by root adjustment of the biophysical environment near the root, inducing diffusional constraints that determine the quality and extent of microbial activity. Relatively anaerobic conditions and compartmentation which develop in structural units about the mature root stabilize labile OM. The following sections present data indicating the possible validity of such implications of the RM.

Root Morphology and Function

One may predict from the RM that diffusion limitations about the surfaces of a mature root restrict nutrient uptake to only a small portion of the entire root under field conditions. Burns (1980) concluded, from a review of the literature, that most crops can grow normally with less than 15% of their roots exposed to nitrate nitrogen (NO_3^--N). The results of Haider et al. (1985, 1987) and Power et al. (1986) suggest that nitrogen uptake (N) does not increase geometrically with increased size of the root system.

The RM explains that plants (in undisturbed soils) avoid microbial competition for available nutrients, because plant-available forms are taken up by the advancing root before microbes can respond to root-derived substrates (Trofymow, 1984). The microbes active in the energy-rich, nutrient-poor soil environment dominated by the maturing root obtain nutrients by decomposing nutrient-rich soil OM. The large portion of the plant root surface unoccupied by microbes (Foster et al., 1983, p. 8), assures that the plant reaps considerable benefit from the enhanced mineralization. Thus, mineralization of soil OM is a necessary consequence of plant root activity, and much of the nutrients taken up by plants in a given season are supplied from root-induced, microbial mineralization of soil OM.

Power et al. (1986) used isotopic N (^{15}N) to study the effects of residue levels on N uptake from soil, fertilizer, and residue by no-till corn (*Zea mays* L.) and soybeans [*Glycine max.* (L.) Merr.]. They found that most of the N utilized came from mineralization of native soil N. Nitrogen fixation was likely in the soybeans and fixed N could not be differentiated from native soil N. In corn, though, it was estimated that 88%-90% of the N taken up was native soil N despite a spring fertilizer application that could have provided 16%-28% (assuming half of the fertilizer N was unavailable in the soil or volatilized) of the corn's N requirements, but actually only provided 5%-8%.

Utilization of native soil N by corn increased as crop residue levels increased (moisture conditions improved and plant size increased), but its relative importance as a N source did not. Each year the fertilizer was surface broadcast before planting. Its distribution within the soil body presumably depended on soil moisture content and the quality of moisture inputs, known to differ among the surface residue treatments. The native soil N, on the other hand, was much more uniformly distributed throughout the soil body. If, as predicted from the RM, the mineralization (and utilization) of native soil (organic) nutrients is a direct consequence of root activity, then, in Power et al.'s study use of native soil N should be proportional to root mass. Use of fertilizer N, however, would depend on its effective entry into the soil body.

Assuming shoot:root ratios were reasonably consistent across residue treatments, then the mass of above-ground plant residue (stover) may be used an indicator of corn root mass. The ratio of [fertilizer N taken up]:[stover mass] was not consistent across residue levels (different moisture conditions) but, the relationship among levels over time was consistent (Table 5.5). This finding suggests that utilization of fertilizer N was not as closely related to root mass as it was to the effects of residue levels -- presumably on the entry of fertilizer N into the active rooting zone. In contrast, the ratio of [native soil N taken up]:[stover mass] was consistent across residue levels (Table 5.5) suggesting that the utilization of native soil N is directly related to plant size (root mass). This result agrees with the RM condition that each root, as it grows, induces mineralization of native organic nutrients within its effective rhizocentric range.

Table 5.5. Effects of crop residue levels and time on ratio of fertilizer or soil nitrogen (N) taken up to stover production by corn (*Zea mays* L.). Data from Power et al. (1986).

Residue treatments[1]	Ratios of N uptake[2] to stover mass		
	July 1980	Oct 1980	Oct 1981
%	Grams fertilizer N per kilogram stover		
0	1.9	1.1	1.2
50	2.9	2.1	1.7
100	1.9	1.6	1.3
150	2.3	2.0	1.8
	Decagrams soil N per kilogram stover[3]		
0	1.7	1.2	2.4
50	1.6	1.2	2.3
100	1.6	1.3	2.1
150	1.6	1.4	2.1

[1] 100% = Amount of crop residue at the end of preceding crop year. Residues were removed to achieve lower (0 and 50%) levels. Residues removed from lower level treatments were added to plots as necessary to achieve the 150% residue treatment.

[2] N uptake = stover (straw) N + grain N. Root masses and N contents were not determined.

[3] Uptake of soil N was consistently ten-fold greater than uptake of fertilizer N. To facilitate comparisons of the ratios for the two N sources, soil N uptake was divided by 10, that is, the units used for soil N are ten-fold larger than the units used for fertilizer N.

These field results (Power et al., 1986) are supplemented by the results of Haider et al. (1987). The latter work is particularly interesting because, in contrast to the former, it was done under laboratory conditions using a loamy soil low in OM (7 g kg^{-1} organic C). Roots were severely spatially constrained compared to field conditions -- corn (*Zea mays* L. cv. Brillant, Harms-Bielefeld) was grown, 2 plants per 15-liter pot. The soil contained 0.62 g kg^{-1} total N and was amended with ^{15}N-labelled fertilizer, at experiment initiation each pot containing approximately 1000 mg of NO_3^--N. More labelled NO_3^--N was added in subsequent irrigations such that, by the end of the experiment (85 days), a total of 2950 mg had been added to each planted pot, 1220 mg to each unplanted pot. These additions maintained NO_3^--N concentrations between 11 and 63 mg N kg^{-1} soil in the planted pots and between 30 and 61 mg N kg^{-1} soil in the unplanted pots, i.e., the soils were never allowed to become even marginally deficient in available inorganic N.

Despite the spatial constraint on root extension, relatively low levels of native soil N, and the continuously high levels of available inorganic N, 20% to 25% of the N taken up by the corn came from mineralization of native soil organic N. Haider et al. concluded from their results that plant roots increase mineralization of N from soil organic matter.

A Rhizocentric View of Roots and Organic Matter

Note that these results (Haider et al., 1987; Power et al., 1986), agree with the RM and indicate that <u>mature roots do not induce mineralization</u> of organic nutrients. If mature roots enhanced degradation of soil OM until senescence, then utilization of native soil N would be geometrically related to plant size (root mass). On the contrary, plant utilization of native soil organic nutrients is linearly related to root length (Table 5.5). Each unit length of root corresponds to a roughly cylindrical, rhizocentrically-affected volume of soil, i.e., each length of root has an associated rhizocentrically affected "length" of soil.

From its associated "length" of soil the advancing root extracts nutrients by several means. It mechanically and chemically disturbs soil structure, exposing the more available nutrients and labile, nutrient-rich organic matter. Available inorganic forms are taken up directly. Access to the organic forms is obtained through (cooperative) stimulation of soil microbes by release of readily available (energy) substrates. Substrate release diminishes as clays and organic debris accumulate about the maturing root. The potential for immobilization is high about the mature root, but diffusional constraints prevent high levels of microbial activity. The diffusional constraints and microbial activity cause partially anaerobic conditions, permitting slow nutrient transformations, and assuring the presence of labile OM for long periods after root death.

Plants Control Nitrogen Through Soil Structure

Haider et al.'s (1987) results support other PCH/RM predictions. For example, plants induce microbial mineralization of labile or nutrient-rich OM only in the rhizocentric range of the root. The intensive diffusion gradient toward the root assures rapid absorption of any plant-available nutrient forms. Mineralization of soil OM releases ammonium nitrogen (NH_4^+-N), i.e., N obtained from the root-induced mineralization will be taken up in that form. Haider et al. (1987) noted that N mineralized from soil OM did not show up in the nitrate pool, and suggested that this it was taken up as NH_4^+-N.

The PCH/RM explains the inherent stability of natural plant/soil systems (as compared to agricultural plant/soil systems) as a consequence of plant/soil system structure. The RM, as presented, deals specifically with structure in grassland soils. Obviously, reliable nutrient supplies are essential to a stable plant/soil system. If the PCH/RM concept is correct, then it should explain how natural plant/soil systems stabilize, or otherwise assure adequate supplies of, for example, plant-available N.

Direct uptake of NH_4^+-N from root-induced mineralization of soil OM, and the sheer power of the immature root as a nutrient sink, assures minimum accumulation of NO_3^--N in the soil that, along with root-control of readily available energy, minimizes N losses to leaching and denitrification while plants are active. However, to be stable the N supply must be protected when plants are inactive. It follows from the PCH/RM that NH_4^+-N and relatively labile organic N will be more prevalent in mature, structurally plant-controlled soils than in, for example, tilled soils. This prevalence is consequent to the stabilization of organic matter and NH_4^+-N within certain parts of rhizocentrically-formed soil structural units, portions of which are observed as water-stable aggregates (WSA). The stabilization is intrinsic to the uniquely dynamic, biophysical organization of rhizocentrically-formed structures that limits O_2 access to, while permitting some diffusional exchanges and microbial activity in, the aggregate interior. The high organic C contents and lower O_2 concentrations expected in structurally-plant-controlled soils do not favor nitrification. The well-known low NO_3^--N contents, greater importance of NH_4^+-N, and greater lability of organic N in undisturbed grassland soils are thus explained by plant-control of soil structure, with no need to invoke continuous high rates of root consumption of NO_3^--N or allelochemical effects. The PCH/RM explains the converse situation in cultivated soils as a result of structural disturbance.

PLANT CONTROL: AN EVOLUTIONARY NECESSITY

Early Nutrient Supplies: Available but Unstable

If the PCH/RM is correct, then soil OM is overwhelmingly important to stable plant/soil systems. Plants, particularly grasses, apparently are obligated to preferential use of the nutrients in soil OM. Hopkins (1948) speculated on the origin of this dedication and the changes in nutrient forms that occur during development of "skeleton soils" into mature soils

> We can reasonably assume a steady fall in the favorable proportion of active to inactive nutrients...Plants, finding their soluble nutrients less abundant in the soil, began to evolve with an eye to the next best thing, the nutrients loosely attached to the humus. An increasing dependence upon humus, and increasing association of plant roots with humus, developed." (p. 78-79).

The PCH/RM is in accord with the view that the plant-humus (and thus plant/soil) relationship is an evolutionary development of resounding importance to agriculture -- indeed to life on earth as we know it.

As supplies of nutrients in the soil diminished, supplies of organic nutrients may be presumed to have increased. Plants "with an eye to" (a functional ability to use) the organic forms would clearly have an adaptive advantage under these circumstances. We can assume that primitive plants were no more capable of direct utilization of organic nutrients than modern plants. Indeed, it can be reasoned that plants capable of direct utilization of organics would be functionally independent of microbes and consequently evolutionarily doomed. Those plants successful in accessing organic nutrients were likely those tolerant of microbial activity on or near the roots. Evolution selected for this tolerance trait. Note that development of the ability to obtain nutrients (via microbial activity) from organic sources did not and does not require a plant to give up its ability to use inorganic nutrients, since it is, in fact, a complementary rather than a replacement capability. A further adaptive advantage belonged to those plants in which an ability developed, not to just tolerate, but to selectively stimulate those microorganisms or microbial activities most important to mineralization of organic nutrients. The converse reasoning may be used with respect to microbial evolution. As plants became more successful and important as a source of energy substrates, microbes more capable of meeting plant needs would have an adaptive advantage over those not so endowed. However, such evolutionary developments lead to a dilemma.

The Dilemma: Available or Stable Nutrients

Hopkins (1948, p.78-79) pointed out the ability of "humus" to retain nutrients against leaching and fixation on or in minerals, i.e., the ability of OM to preserve an ecosystem's nutrient supplies and assure long-term survival. He also felt that plants evolved "with an eye to...the nutrients loosely attached to humus," that is, labile organic nutrients. The ability of soil OM to stabilize nutrients against losses to leaching, fixation, etc., depends on resistance to microbial attack, its "recalcitrance". On the other hand, the ability of OM to supply plant nutrients depends on its susceptibility to microbial attack, its "lability". Therein resides the dilemma: If nutrients, organic or inorganic, in the soil were to be useful for plant growth, then they must have been sufficiently labile. If the nutrients were labile, then they were subject to losses and eventual exhaustion; and consequently, the plants using them, indeed the entire ecosystem, were doomed. Probably many more plants, and the ecosystems dependent upon them, failed (in the evolutionary sense) than succeeded. Plants that did not evolve beyond this dilemma, those that did not encounter an effective means of controlling or avoiding the lability/recalcitrance dilemma, survived as opportunistic and "pioneer" species, but could not serve as the dominant producers in stable plant/soil systems. The PCH-RM can be used to address the lability/recalcitrance (of soil OM) dilemma. The PCH/RM accomodates evidence that no soil OM is truly recalcitrant (Payne, 1985).

The Solution: Stabilize Availability

There are two apparent successful adaptations. One, common among modern dicots, involved direct symbiotic relationships exemplified by the legume-rhizobial and mycorrhizal symbioses. These are a refinement of the selective stimulation of microbial activities beneficial to plants, giving plants access to otherwise inaccessible nutrients. This adaptation, seen at an extreme in modern tropical rain forests, minimizes contact of nutrients with the soil and by-passes the lability/recalcitrance dilemma associated with dependence on soil-borne nutrients. It follows that stable plant/soil systems which rely heavily on this adaptive approach will not have fertile soils.

The second successful adaptation, apparently restricted to certain monocots, is most fully developed in modern perennial prairie grasses. Energy is expended, not to access new or alternative nutrient sources, but to physically alter the soil itself, to change "the rules of the game". Again the effect is to control microbial activity, selectively stimulating beneficial activity, but with a different method of control. Root-induced rearrangements of soil particles in zones of plant-stimulated activity impose diffusion constraints, slowing and altering microbial activity. Relatively large, root-defined structures can not be rapidly disrupted by the much smaller soil microbes, but the mechanical forces applied by the growing root can disrupt an aggregate in a few hours, perhaps even minutes. Thus, root-controllable physical isolation of OM permits stabilization of labile nutrient forms against microbial activity without sacrificing plant access. Plant-accessible labile nutrients, mostly organic, can accumulate until the soil structural capacity is reached. Beyond this structural capacity, unstabilized plant residues with high C:N ratios assure immobilization of free nutrients appearing during periods of plant inactivity. Such immobilized nutrients would not be of immediate use to plants, but help assure the adequacy of future nutrient resources. (No doubt there are many ecosystems transitional between the tropical rain forest and the temperate grassland. In those, and the plants important in them, there are probably gradations and peculiar refinements of the two successful adaptations just outlined.)

The preceeding discussion from the PCH/RM perspective is speculative, but any discussion of ancient plant evolution must be. However, the discussion introduces two important PCH/RM hypotheses:

(i) any given soil has its own, limited structural capacity, and (ii) the dedication of plants to organic nutrients is an evolved (inherited) trait.

ORGANIC MATTER AND SOIL CHEMISTRY

The RM says that a soil's structural capacity depends on the quantity and quality of root-rearrangeable clays in that soil. The structural capacity is reached when all these clays are involved in rhizocentrically formed aggregates. (Recall Dormaar's (1983) results (Table 5.1) showing no readily dipsersible clays in undisturbed prairie soil.) Rhizocentrically-aggregated clays are highly organized and intimately associated with OM. Therefore in structurally plant-controlled soils most, if not all, mineral colloidal surfaces are physically occluded or occupied with adsorbed organic molecules. Rhizocentric control of soil structure consequently brings soil chemistry under plant control.

Ion Exchange Properties

Evangelou and Blevins (1985) studied the soil phase ion-exchange/solution phase interactions of basic cations (Ca^{++}, Mg^{++}, K^+, NH_4^+) in long-term tillage systems on a Typic Paleudalf in Kentucky. At sampling, three treatments (no-till + 336 kg N ha^{-1} yr^{-1}, no-till + no N, and conventional tillage + no N) had been applied annually for 13 years to the silt loam soil previously in bluegrass pasture for 55 years. No-till resulted in a "drastic decrease," compared to conventional tillage, in the ion exchange selectivity for K^+ vs. (Ca^{++} + Mg^{++}), but an increase in the selectivity for K^+ vs. NH_4^+. Those authors suggested these effects were related to "organic matter content as well as possible colloidal surface modification of the soil inorganic phase due to specific adsorption of organic molecules."

The PCH/RM suggests that structural disturbances (by tillage) of rhizocentric soil structure (undisturbed in no-till systems) will result in qualitative, as well as quantitative, changes in the soil OM. Ion selectivity was more clearly related to lack of tillage than to OM. From the conventional tillage (structurally disturbed) to the no-till + no N treatment, OM increased by 23.3 g kg^{-1} and the K^+ vs. (Ca^{++} + Mg^{++}) and K^+ vs. NH_4^+ selectivities changed significantly (Table 5.6). From the "no-till + no N" to the "no-till + N" treatment, the OM again increased significantly (by 15.4 g kg^{-1}) but, there were no significant differences in the K^+ exchange selectivities between the two no-till treatments. These results clearly indicate that the ion-exchange properties of the no-till soils were governed by a different quality, and not quantity, of effective soil ion exchanger than were the exchange properties of the tilled soil.

In order to pursue this point further, one may consider that there was a substantial amount of OM (21.3 g kg^{-1}) in the conventionally tilled soil. Therefore, if the quality (chemical character) of OM was consistent across tillage treatments, then changes in ion exchange properties should be proportional to changes in OM content -- regardless of whether the exchange sites in OM accumulate additively or by occluding and replacing mineral exchange sites. But, the "drastic decrease" in K^+ selectivity from tilled to untilled treatments shows the changes in exchange properties were not proportional to changes in OM content. Further, since considerable OM was present even in the

Table 5.6 Chemical and physical soil properties after 13 years of no-till and conventional corn (*Zea mays* L.) cropping on a Typic Paleudalf. Data from Evangelou and Blevins (1985).

	Treatments		
	conventional till + no nitrogen (N)	no-till + no N	no-till +336 N[1]
Quantitative data			
Organic matter (OM) g kg^{-1}	21.3	44.6	60.0
Cation exchange cap.(CEC) cmol(+)kg^{-1}	15.9	19.3	23.4
Saturation paste point (SPP)[2] kg kg^{-1}	0.522	0.707	0.860
Qualitative indices			
Selectivity coefficients[3]			
K^+ vs. (Ca^{++} + Mg^{++})	5.9	3.8	3.5
K^+ vs. NH_4^+	2.1	2.5	2.6
Ratios[4]			
CEC:SPP	30.5	27.3	27.2
CEC:OM	0.75	0.43	0.39
SPP:OM	24.5	15.9	14.3

[1] Fertilizer applications were the same for all three tillage treatments, except no N was applied to the conventional tillage and one no-till treatment while the other no-till treatment received 336 kg ha^{-1} yr^{-1} NH_4NO_3.

[2] Mass of water required (oven-dry soil weight basis) to form a saturated paste as described by the U.S. Salinity Lab. Staff (1954).

[3] Selectivity coefficients are those described in Evangelou and Blevins (1985) and reflect the ability of K^+ to displace the compared ions from soil ion exchange sites.

[4] These ratios are presented as dimensionless because they index collective properties of a soil as a whole, just as a carbon:nitrogen ratio (C:N) indexes a collective property of, say, a kind of plant tissue. In order for such ratios to be meaningful, all that is necessary is that both the property indicated in the numerator and that in the denominator be properties of the whole which the ratio is used to describe. No functional connection between the two properties within the whole is necessary or implied, although such connection may exist. Such ratios should not be misinterpreted: a plant's C:N does not imply that all or any of the plant's carbon is in nitrogen-containing compounds. Neither should a soil's CEC:OM, for example, be interpreted as suggesting that all or any of that soil's CEC is attributable to its OM.

conventionally tilled soil, such a "drastic decrease" in ion exchange selectivity suggests that the greater plant-control of structure in the no-till soils resulted in "effective elimination" of a considerable portion of the inorganic exchange sites probably due to either physical (structural) occlusion or adsorption of OM onto the inorganic exchange sites.

Structure and the Chemical Character of Organic Matter

The RM postulates less oxidized conditions within rhizocentric structural units and consequent stabilization of more reduced organic forms. Interference with the root-control of structure would be expected, therefore, to cause greater losses of reduced than oxidized forms of OM. Also, elimination of the diffusional constraints would result in the decomposition of organic residues under more oxidized conditions and consequent replacement of lost, relatively reduced OM with OM synthesized under more oxidized conditions. Dormaar (1979) specifically examined the characteristics of the OM from cultivated and undisturbed sites for each of six different Canadian prairie soils. He found cultivation generally resulted in decreased importance of aliphatic -C-H and $-NH_2$ groups in side chain components, and increased -COOH content.

If the formation of highly condensed, relatively highly aromatic humic molecules is a side-effect of oxidative microbial degradation of organic substrates (Haider and Martin, 1971), then it follows that the OM characteristic of more oxidized environments would have higher humic acid:fulvic acid ratios (HA:FA). Under similar conditions tilled soils are predicted to be relatively more oxidized, and consequently have higher HA:FA, than undisturbed soils. Dormaar (1979) found, contrary to this RM prediction, that the HA:FA of *whole soils* decreased as a result of cultivation. However, tillage reduces the effectiveness of rhizocentric structural control and therefore this RM prediction should only apply to the organic matter in the *rhizocentrically-induced soil structural units* which survive the effects of tillage. In a later study of the water-stable aggregates in one of those six prairie soils (a Haploboroll), the HA:FA ratio of the OM in all aggregate size classes studied had increased under cultivation, in harmony with the PCH/RM and oxidative polymerization hypothesis.

Dormaar (1979) reported effects of cultivation on the distribution of OM among the sand, silt, and clay fractions, also. The soils were ultrasonically dispersed before fractionation. Cultivation resulted in a shift of C from the sand to the clay size fraction, increased extractability of the C in all size fractions, and decreased C content (thus, increased O content) in the extractable OM. The infra-red absorption (IR) spectra also showed that cultivation resulted in a decrease in the importance of C-H and phenolic -OH groups, especially in the sand size fraction, and $-NH_2$ groups, especially in the clay size fraction. These results indicate that the lower (whole soil) HA:FA ratios for cultivated soils result from an accumulation of less structurally-affected, relatively nitrogen-poor, more highly oxidized OM formed during decomposition of plowed-in, rhizocentrically-unaffected crop residues. Linn and Doran's (1984) study of differences in the composition of the microbial populations between tilled and no-till soils likewise reflect a less oxidized soil environment in structurally-undisturbed soils.

Also, Dormaar (1979) presented the IR spectra of (whole soil) resin-extracted OM for three of the six soils he studied. The three soils were from climatic zones with different moisture regimes. The IR spectra of OM extracted from undisturbed soils showed consistent differences across the climatic zones. The drier the climate the more important aliphatic -C-H and $-NH_2$ groups. Phenolic -OH groups were important across all three zones. In contrast, the OM of the cultivated soils (all in wheat-fallow rotations) did not vary across the climatic zones. That is, when the structurally effective inputs (wheat root activity and tillage) are consistent, the quality of the soil OM will be consistent despite pedogenetically important differences in climate. The PCH/RM predicts that soil structure is the dominant environmental factor determining the character of soil OM, and that the stability of a plant/soil system depends on the ability of (the system's) plants to control soil structure.

Soil Organic Matter and pH

If the PCH/RM is generally applicable, then soil pH should also be subject to plant-control through structural control of soil OM. Referring again to Dormaar's (1979) results, the total acidity of OM from undisturbed prairie soils was always lower than that of OM from cultivated soils. Further, earlier studies by Dormaar (1974, 1975) indicate a close association between exchangeable calcium and OM. A greater ion-exchange preference of OM for calcium, especially OM in structurally undisturbed (no-till) soils, is also suggested by the results of Evangelou and Blevins (1985) (discussed above). The lower total acidity of, and increased retention of calcium by, the OM of structurally plant-controlled soils clearly suggests that soil pH can be controlled through control of the character and quantity of soil OM.

Pratt (1961) showed the probable importance of pH-dependent cation exchange capacity (CEC) in soil pH buffering. Bohn et al. (1979) pointed out that, while some of the charge of layer silicates is pH-dependent, all charge on OM is pH-dependent. It follows that if pH-buffering is related to pH-dependent CEC (charge), then, in most agronomically useful soils, OM should be very important to pH buffering. Magdoff and Bartlett's (1985) study of the pH buffering capacities of 51 Vermont soils also supports that role. The latter authors concluded that OM apparently has an important role in buffering soil pH. In fact, for the Vermont soils studied, pH titration curves were mainly OM titration curves.

Increases in the concentration of soluble salts in the soil solution lowers pH (Tisdale and Nelson, 1975, p. 404-405). Those authors cite three major sources of soluble salts in the soil solution: mineral weathering, organic matter decomposition, and addition of fertilizer. The PCH/RM suggests that the ability of plant/soil systems to deal with inputs is related to the evolutionary experience of the plants controlling the plant/soil system. Generally, plants have evolved in environments in which weathering minerals and decomposing OM were the major sources of soluble salts. These two sources provide long-term, low-intensity inputs, that are subject to evolved plant control mechanisms. Fertilizer inputs are evolutionarily new challenges to plant/soil systems. The inputs are intense, short-term, and generally not subject to plant control except through plant uptake and subsequent influence on soil properties, especially CEC.

Ion-Exchange and pH Interdependence

It is worthwhile to again consider in this regard the work of Evangelou and Blevins (1985) on the ion exchange properties of a Paleudalf cropped to corn under three tillage/fertilizer treatments. There were two no-till treatments, one receiving 0 kg ha^{-1} yr^{-1} (NT) and the other 336 kg ha^{-1} yr^{-1} (NTN), and one unfertilized conventional tillage (CT) treatment. Soil samples were mixed into saturation pastes. To simulate fertilizer applications, all samples were amended with 5.55 mmol kg^{-1} NH_4Cl and 0.97, 1.94, 4.46, or 8.92 mmol kg^{-1} KCl, both added in solution during preparation of saturated pastes. After 24 hours equilibration, the pH, solution phase cation (Ca^{2+}, Mg^{2+}, NH_4^+, K^+) concentrations, and soil phase exchangeable (1 mol L^{-1} NaCl) cations were measured.

Fig. 5.1 was developed to examine the solution phase H^+ concentration ($[H^+]$), instead of pH, and the sum of the concentations of all measured cations ($[M^+]$) as related to the soil phase exchangeable K^+ (soil-K^+_x). In all three soils $[M^+]$ increased as soil-K^+_x increased. However, a marked increase in $[M^+]$ occurred at soil-K^+_x = 0.8 cmol (+) kg^{-1} for the conventionally tilled soil but, not until soil-K^+_x reached 1.2 cmol (+) kg^{-1} in both the no-till soils. The pH in the no-till soils was apparently well buffered as long as soil-K^+_x was below 1.2 cmol (+) kg^{-1}. Above this soil-K^+_x level, pH buffering (apparent in the slope of the $[H^+]$ vs. soil-K^+_x line) differed between the two no-till soils, the difference probably due to a history of lower base saturation in the fertilized soil (Blevins et al., 1983). In the tilled soil pH buffering appeared consistent over the range covered by

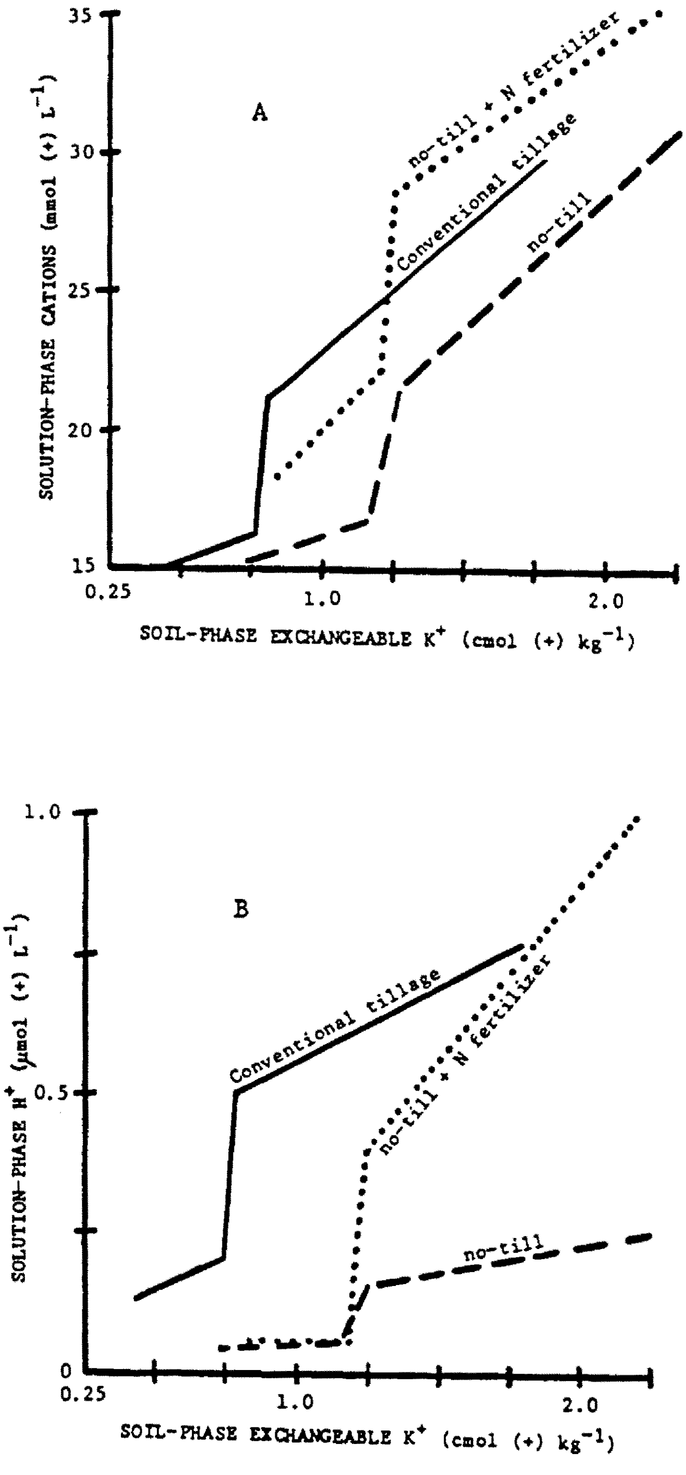


Fig. 5.1. The relationship between soil-phase exchangeable potassium and the solution-phase concentration of (A) major cations (Ca^{2+}, Mg^{2+}, NH_4^+, K^+), and (B) of hydrogen ions for saturated pastes of a Typic Paleudalf in corn (Zea mays L.) under continuous tillage and fertilizer treatments for 13 years. Data of Evangelou and Blevins (1985).

the data, except for the marked change at a soil-K^+_x level of 0.8 cmol (+) kg^{-1}.

Since the two no-till soils had different OM contents (44.6 vs. 60.0 g kg^{-1}), the strong similarities in the CEC behavior and the consequent pH buffering qualities (change at soil-K^+_x = 1.2 cmol(+) kg^{-1} and strong pH buffering at soil-k^+_x < 1.2 cmol(+) kg^{-1}) were likely related to similarities in the character, rather than the quantity, of the OM present. Analogously, the differences between the tilled soil and the no-till soils are more likely related to differences in the character of the respective soils' OM, rather than differences in OM content among the soils. These similarities and differences in OM character are predicted from the PCH/RM as the consequences of plant control (or lack of plant control) of soil structure and function.

Differences between the tilled and no-till soils in soil organic matter/structure/cation exchange relationships are also indicated by the data from Evangelou and Blevins (1985) given in Table 5.6. The quantitative measurements increase consistently from CT to NT to NTN. The qualitative indices, however, show the no-till soils were similar to each other, but different from the tilled soil, in every case. Among the qualitative indices the three treatments were most similar in their cation exchange capacity:saturation paste point (CEC:SPP) ratios[3]. The two no-till soils had essentially identical CEC:SPP. Since all three treatments were applied to the same soil, textural differences probably were minimal and the SPP may be considered an index of moisture holding capacity (U.S. Salinity Lab, 1954) and structure. The CEC:SPP ratios,

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[3] See Table 5.6 footnote [4].

then, indicate that CEC is closely related to structure, that CEC has the same relationship to structure under no-till corn regardless of OM level, and that this relationship is not the same as existed under conventionally cultivated corn.

These results show, as the PCH/RM anticipates, that the character of a soil's OM, and, hence, its chemical properties (here, pH buffering and ion exchange behavior) depend on soil structure, which is controlled by the plant (or plant-and-plow) activity that affect the soil.

MOISTURE SUPPLY

Historically, agricultural experience suggests a link between plant control of soil structure and a more plant-favorable moisture supply. During the last century Dokuchaev and Kostychev showed that continuous cultivation of chernozems for cereal production increased susceptibilty to drought (Kononova, 1961, p. 23). Kostychev showed soil physical properties which favored retention of moisture were associated with the accumulation of humus under perennial grasses (ibid.).

Soil Organic Matter and Structure: The Moisture Connection

The data of Evangelou and Blevins (1985) (Table 5.6) furnish recent evidence of the effectiveness of plant control of soil structure in improving moisture supply. The SPP is directly related to water holding capacity for most soils of moderate texture -- the available water holding capacity (WHC) is approximately one-quarter of the amount held at SPP (Bower and Wilcox, 1965, p. 934). Differences in the OM content of the CT, NT, and NTN soils appear to be directly related to differences in the estimated SPP for the three soils, suggesting that increases in water holding capacity in the NT and NTN compared to the CT soil were a consequence of the parallel increases in OM content (Table 5.6). However, as was the case with the CEC, direct examination of the data is deceptive. In order to obtain an index of the contribution of OM to the WHC of each soil as a functional whole (i.e., not each soil as a simple composite, the WHC of which is a property defined by summation of the WHC effects of the mineralogy, texture, and OM content of the soil)[4] the SPP:OM ratios of the three soils can be calculated. The values of the SPP:OM turn out to be 24.5, 15.9, and 14.3 for the CT, NT, and NTN soils, respectively. Again, as with the CEC:SPP and CEC:OM ratios, both the no-till soils had similar SPP:OM values which were distinctly different from the SPP:OM of the CT soil. Further, as can be anticipated through use of the PCH/RM (but not through any other conceptual model with which the author is familiar) the SPP:OM values for the no-till soils were lower than that of the conventionally tilled soil. The relationships among the SPP:OM values of the three soils suggest that the OM content was more strongly related to the ability to retain moisture in the CT than in the no-till soils. That is, in the no-till soils where plant-control of structure was less disturbed (more effective), the OM content was less strongly related to the SPP (water holding capacity) than in the CT soil where structure was to a large extent mechanically determined.

The PCH/RM suggests that this effect results from the ability of the plant roots to organize the soil particles so that hydraulic conductivity and moisture retention are simultaneously optimized.

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[4] See Table 5.6 footnote [4].

Rhizocentric organization of soil clays into water-stable aggregates increases water-stable porosity. Thus, plant-control of soil structure enables soil to consistently accept water at higher input rates than the same soil with tillage-induced structure (except, in some soils, for a brief period following a tillage operation). This is vividly shown in the infiltration rates of sod and parallel cultivated soils (Mazurak and Ramig, 1962, 1963).

Moisture Dynamics of Rhizocentric Structural Units

Rhizocentrically-organized, water-stable aggregates are expected to have unique properties that increase available soil water holding capacity. These aggregate's high clay content would increase their moisture holding capacity, but not necessarily their available moisture holding capacity since the water of hydration of clays is not plant-available. In order to improve retention of available moisture, clay particles must be arranged such that the aggregate has more pore volume when moist than when dry.

Rhizocentric structures have such an arrangement. Layer silicate clay particles are "puddled" around the root, laid down with the a-b plane parallel (c axis perpendicular) to the longitudinal axis of the root and drawn into close packing by the matric suction of the active root and later by environmental events. The geometry of the aggregate is thus dominated by the organic (root) core and proximate concentric laminations of clay particles about the core. The minimum and maximum diameters of the aggregate are defined by this physical arrangement. The minimum occurs when the aggregate is dry, all clays and OM are shrunk and pore volume is minimal (Fig. 5.2). Upon wetting the

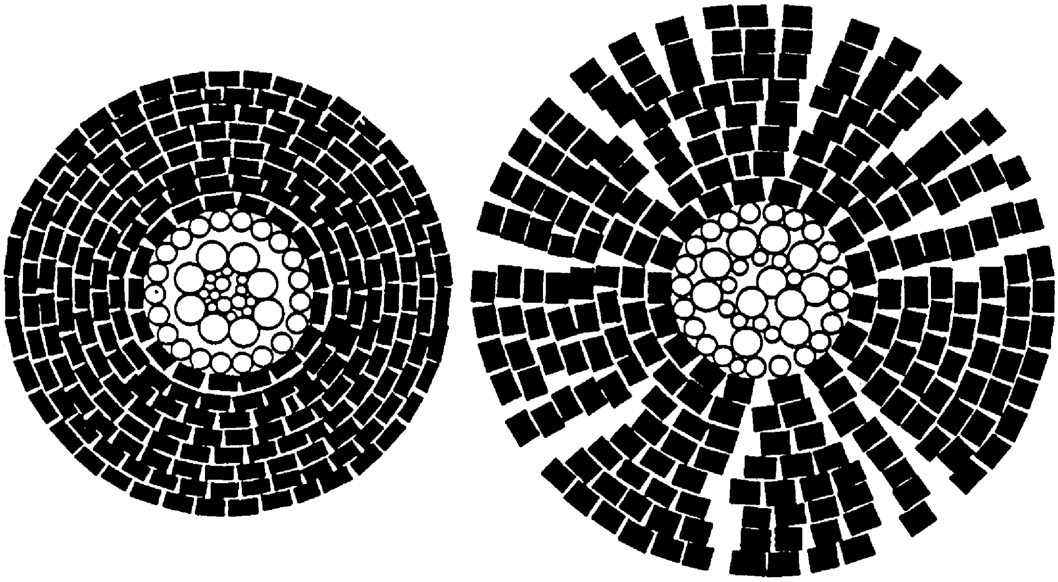

Fig. 5.2. A diagrammatic representation of the effect of moisture on the porosity of aggregates formed by concentric lamination, with face-to-face organic bonding, of swelling layer silicate particles around an organic core.

aggregate swells (Emerson and Dettman, 1959; Norstadt and Payne, unpublished data), but layer silicates swell essentially only along the c-axis. If the clay platelets were not cemented together at the areas of face-face contact, such swelling would disrupt the aggregate. However, the rhizocentric arrangement of the clays and organics is such that, most swelling in the clay-dominated "shell" results in radial expansion, with only limited tangential expansion. Consequently, submicroscopic water-filled "cracks" develop upon wetting (Fig. 5.2). (Assuming a cylindrical aggregate, the formation of such cracks would be unavoidable if the tangential expansion was not greater than radial expansion by a factor of "pi".) As the soil dries, the clays (and organic core) shrink, resulting in radial contraction and reduction of the moisture-induced microporosity. Thus, the water intake/output behavior of such aggregates is not a constant-pore-volume phenomenon, because the pores which accept and hold moisture are dynamically dependent on the amount of water present. (In reality the clay particles may not be so well organized as depicted in Fig. 5.2, and such structures would be expected to contain enmeshed silt and sand particles. These and other factors complicate the discussion, but do not eliminate the effects of the principle just discussed.)

Newman and Thomasson (1979) presented evidence indicating such behavior, and concluded, from studying the effects of drying on soil porosity in 15-to-20-mm soil peds, "much plant available water released from clay soils at potentials less than -15 bar (-1500 J Kg^{-1}) is controlled by shrinkage of the soil rather than by emptying of pores."

I have observed air-dry, water-stable aggregates from Colorado grassland soils as they were wetted with a capillary pipette. The aggregates did not swell equally in all three dimensions, but preferentially in two dimensions. When the longitudinal axis of the root residue core could be identified prior to wetting, the aggregates were observed to swell typically by 0.2 mm mm^{-1} perpendicular to the root's longitudinal axis, but by <0.05 mm mm^{-1} along the axis. Also, the air-dry aggregates wetted quickly. Swelling appeared to occur after wetting. Artificial aggregates, broken from air-dried puddled soil clays, wetted slowly, swelling and dispersing while wetting. Mineral particles from the same soil and the same size as the aggregates often did not wet at all (unless physically forced into a water droplet). Determinations showed the water-stable aggregates held water at the rate of 0.7 kg kg^{-1} while the mineral particles from the same soil retained 0.1 kg kg^{-1}, at about -1.5 J kg^{-1}. Undecomposed and aggregate-uninvolved root residues retained relatively large amounts of water at low suctions but, dried rapidly in air compared to moist aggregates. These observations and the results of Newman and Thomasson (1979) were predictable using the PCH/RM and support the idea that plant-control of soil structure might optimize water management in plant/soil systems.

SUMMARY

The PCH/RM concept explains that in stable plant/soil systems plants control the soil environmental factors that affect plant growth and the interactions among those factors by controlling the soil structure of the system. In some environments plants achieve this control by rhizocentrically structuring the soil. PCH/RM interpretations of the findings of several studies of the soil environmental factors affecting plant growth have been presented. Data have been presented that supports the RM-postulated movement of colloids to plant roots. Other results indicate that the quantity of dispersible clays may affect production by altering root:shoot ratios. The PCH/RM-indicated roles of readily dispersible clays, and their likely prevalence in the soil environment over evolutionarily important time periods, may help explain the somewhat surprizing tolerance of upland plant roots for low O_2 levels. Elevated contents of rhizocentrically organized clays appear to be intrinsic in rhizocentric water-stable aggregates providing them with unique biophysical properties. The cumulative effect of seasonal or annual reinitiation of the rhizocentric aggregate forming process is development of a plant-controlled, phytocentrically organized structure at a super-aggregate (soil) level. Phytocentrically organized soil structure regulates such soil macro-properties as erodibility, bulk density, and permeability to air and water, and, consequently, microbial activity outside aggregates, while rhizocentrically organized aggregate structures regulate the quality and extent of aggregate-affected microbial decomposition processes, hence, the quality and quantity of soil OM, and, consequently, the soil chemistry. By inducing such structure, plants can control the forms of nitrogen and other nutrients which prevail in the soil, induce microbial processing of nutrients into structurally stabilized but plant-accessible (not to be confused with plant-available) forms, and promote more efficient utilization of soil moisture.

This dissertation presents only a small part of an extensive review of the literature covering more than 100 years of agricultural research. That review revealed no case in which the PCH/RM concept could not lead to logically consistent interpretations and/or conclusions, and often new hypotheses. The PCH/RM offers logically consistent explanations for previously unexplainable results. The PCH/RM appears a credible concept which merits critical examination as a new tool for use in attempts to understand plant/soil systems and to develop a stable and productive agriculture.

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APPENDIX

Appendix. A CRITICAL OVERVIEW OF GENERAL SYSTEM THEORY: FROM PHILOSOPHY TO PROCEDURE

PREFACE

When the manuscripts for chapters 3, 4, and 5 of this dissertation were presented to my graduate committee, one of the members asked how it was that I developed the concepts (models) presented in those chapters; that is, what was the method I had used. My response at the time was that I had not consciously employed any specific method to "develop" the concepts -- the ideas simply occurred to me after lengthy consideration of what had previously appeared, within other conceptual frameworks, to be self-contradictory behaviors of soils and agricultural production systems. I was in turn told that science without method is not science and a dissertation not based on a documentable methodology would not be acceptable. One of my committee members then stated his opinion that I had used a "systems approach" and that I should, for the purpose of finalizing an acceptable dissertation, set about demonstrating that use. In order to accomplish such a demonstration I began an examination of the "systems analytical" literature only to become convinced that I had not used a "systems approach" as it is currently understood, but had instead done something common in science: induce an explanation which would allow the observable behavior of a certain class of natural phenomena to become recognizably consistent and predictable. This appendix presents the development of the conclusions just stated, with an initial consideration of "systems theory" as it has been developed and invoked by "systems thinkers" and working into a consideration of inductive logic, its role in science and the difficulties underlying an adequate linguistic description of it.

INTRODUCTION

Systems approaches have become quite fashionable in scientific research. Systems concepts, however, are not new, dating back at least to the time of Aristotle (Checkland, 1981), and the implicit application of systems approaches may predate the dawn of recorded history. The impressive effectiveness with which systems approaches have been more recently applied in the physical sciences, and especially engineering, raised the expectations of biological and social scientists that systems approaches would similarly lead to the solution of problems in their fields. In practice, however, results have been uninspiring, often quite disappointing (Berlinski, 1976; Bertalanffy, 1968; Checkland, 1981). Poor performance has led some to abandon or ignore systems approaches as ineffective, others to attack general system theory (GST), the presumed conceptual foundation of all systems approaches (Berlinski, 1976; Lilienfeld, 1975). Much of this disappointment seems, to the present author, not to be due to an inherent errancy or impotency of systems approaches, but due to unreasonably high expectations -- expectations based for the most part on the impressive track record of systems engineering in computer and aerospace technology, and not on the results of any thorough examination of what GST is or what systems approaches can and, more importantly, can not do. I attempt in this appendix to present such an examination and to develop from it a framework for the presentation of a conceptual model of soil structure and control of resource-use efficiency in plant/soil systems.

THE G.S.T. DEBATE

If there is anything particularly striking in an exploration of (the literature on) general system theory, it is the ebullient enthusiasm of devotees of, and the derisiveness of attacks made by critics of GST (Berlinski, 1976; Bertalanffy, 1968; Checkland, 1981; Laszlo, 1972; Lilienfeld, 1975; Naughton, 1979; Saridis, 1977). This state of affairs is disappointing to scientists outside the exchange, since any useful ideas are lost in the rhetoric, and often blatant emotionality, of the debate. On the other hand, the debate has been raging long enough that at least some of the fundamental issues which divide the advocates and critics of GST have begun to emerge from the rhetorical haze. It is worthwhile therefore to examine the conflict.

A Theory or an Ideology

Initially it is necessary to determine whether or not there is such a thing as GST, about which a conflict might arise. Naughton (1979), a GST-opponent, has stated that if GST means a coherent body of tested knowledge, then there is no such thing as GST. This immediately begs the question, "Why then would one bother to oppose something which does not exist?" Are GST-opponents tilting at windmills? Clearly they are not. Researchers and practitioners in almost all the academic disciplines, and governmental and industrial decision-makers have accepted a variety of "systems" methods as the practical realization of GST. That is, major socio-economic decisions are being justified by invoking a theory which may not even exist. This is clearly a reason for concern and is the reason behind Lilienfeld's (1975) view that GST, never established as a theory, has already become an ideology, and its validity is therefore no longer likely to be subjected to testing .

Now if GST is not a body of tested knowledge, then it must be, at best, a body of untested knowledge, an induction. The social value of scientific knowledge may be considered to lie in its reliability, and that reliability is established through scientific testing -- deductive empirical verification. Since GST is scientifically untested, it should be considered unreliable. For most "theories" -- perhaps more properly, hypotheses or inductions -- this would not present a problem. All that is necessary is to run some experiments to test the induction. However, in testing GST a traditional scientific protocol has been violated. Full-scale human experiments have been run, before the validity of GST had been tested in any scientific sense. Or, as Ludwig von Bertalanffy (1968, p.99), considered by many the father of GST, put it,

> "The danger...is to consider too early the theoretical model as being closed and definitive -- a danger particularly important in a field like general systems which is still groping to find its correct foundations".

"Systems" devotees have changed GST from Bertalanffy's "theoretical model. . .still groping to find its correct foundations", to an expedient "movement" with such justifications as (Checkland, 1981, p.94)

> GST has little content beyond the level of analogies...Progress in the systems movement seems more likely to come from the use of systems ideas within specific problem areas than from the development of overarching theory."

This flies in the face of many GST critics whose concerns were concisely summarized by Berlinski (1976) who wrote, "...in great things great ambitions without great theories are insufficient..."

Unfortunately, despite his apparent appreciation of "the danger" inherent in too rapid an adoption of inadequately developed theory, Bertalanffy (1968) promoted such invocations of GST by referring to it as "a working hypothesis". One might expect a demotion from "theory" to "hypothesis" by the "father" of the theory would have diminished some of the enthusiasm for rapid application of GST. It has not. Instead GST; or more accurately its presumptuous extension,"the systems approach"; has been accepted by systems devotees as capable of explaining the behavior of even the most complex systems -- or as a GST-opponent put it "systems theory is a claim to total power" (Lilienfeld, 1975). It follows that if this claim is true, the ultimate test of GST must be to determine its ability to explain the most "complex" phenomena which we practically encounter -- for example, human societies. Hence, among the experiments necessary to establish the ultimate reliability of GST are regional, national, or even global socio-economic experiments. Herein lie the fundamental concerns of the opponents of GST -- much systems research simply can not be carried out scientifically, especially in the analysis and design of human bio-social systems. Each such effort to practically apply GST must be classed not as an application of reliable scientific knowledge but as an experiment, the results of which may not appear for years after its administrative/technical termination, or even the biological death of the experimenters. The results therefore can not be observed and recorded, hence they can not be analyzed or reproduced and consequently experiments can never scientifically test the validity of GST or any other hypothesis that would treat of such grand phenomena.

Of a more fundamental concern is what will happen to the human subjects of such experiments. If the experiment should succeed -- a "best case" outcome -- the result would be a smoothly operating, centralized, programmed and programmable society -- a dignified ant colony. And what if the experiments should fail? The objective of most such experiments has been to design and/or establish the minimum effective centralized control system necessary to set up and maintain a socio-economic system with certain functional, usually economic, characteristics. Failure of such experiments -- recall the results will not likely be apparent by the administrative end of the experiment -- implies a failure of the experimentally imposed control system to improve the object functions of the subject socio-economic system. In the case of shallow, short-duration, or soon-aborted experiments, the effects of failure might be only a temporary diversion of resources into the experimenters' pockets. On the other hand, if the experiment is protracted or requires major (functionally irreversible) changes in the previously existing controls, then failure would likely become apparent as prolonged degradation or collapse of the subject system. Therefore neither the success nor the failure of such grandiose "systems" experiments can yield anything better than what most rational individuals would consider minimally undesirable results. From a slightly different perspective, an ethical question is raised: Is man (wholesale) to be used to serve "science", i.e. as material upon which to test a "working hypothesis"; or is science to serve man, i.e. as a source of reliable knowledge, which might eventually include a general theory about "systems"?

Seeking Rigor or Selling Rigmarole

The extensive human and intellectual damage that may result from unqualified invocations of GST seems the primary concern of GST-opponents. This was, however, also a primary concern of Bertalanffy (1968, p. 14, 23, 31, 35, 52-53, 99, 119). Indeed, a "third party" examination reveals that the concerns of GST-opponents -- some of which Berlinski (1976) has presented with more than a touch of enjoyable sarcasm -- had troubled Bertalanffy at least 14 years earlier.

This awareness of both the advocates and opponents of GST suggests that GST, whatever that is, might indeed be a powerful conceptual tool, but does little more than to suggest that GST can not be specified by examining misapplications. So the conflict leaves the practicing scientist with a vague idea of what GST is not, but no idea of what GST is.

This is not altogether surprising when one considers that Bertalanffy (1968) himself frequently used statements of negation to discuss GST, "...it will avoid misunderstanding also to state what it [GST] is not." (p. 35) and "General system theory therefore is not a catalogue of well-known differential equations and their solutions." (p. 80). Ultimately Bertalanffy's principal contribution may have been his recognition of the need for a more general conceptual methodology in science and his call for development of a more generally applicable schema,

> "It seems legitimate to ask for a theory, not of systems of a more or less special kind, but of universal principles applying to systems in general . . . In this way we postulate a new discipline called General System Theory."

Regarding the often obtuse character of Bertalanffy's work on GST, Checkland (1981, p.93), a systems practitioner, stated that

Bertalanffy's writings indicated little development from the 1940's until his death in 1972. He even agrees with an outspoken GST-opponent, Lilienfeld (1975), who described Bertalanffy's work as "rather repetitious and even static in character". He goes on to describe Bertalanffy's unchanging view as a vision "that there would arise as a result of work in different fields a high-level meta-theory of systems, mathematically expressed" and continues, "The general theory envisaged...has certainly not emerged" (p. 93). He goes on to cite a GST-opponent's (Naughton, 1979) description of GST as "a melange of insights, theorems, tautologies, and hunches" and agrees with another opponent, Berlinski (1976, p. 10), that GST pays for its generality by lack of content, but then he licenses the carte blanche invocation of GST. Declarations such as these by Checkland beckon recollection of the previously discussed concerns of GST-opponents that acceptance and application of untested or inadequate theories is inherently dangerous. So it would appear that although "systems" devotees and opponents agree on certain undesirable characteristics of GST (and Bertalanffy's writing, as well), in practice the disagreement appears ideological and is resolved to little more than an agreement to disagree.

FUNDAMENTAL MISUNDERSTANDINGS: REFOCUSING THE DISCUSSION

The points on which the devotees and opponents agree serve to focus attention on (what I see as) some fundamental misunderstandings of GST. Consider Checkland's and Lilienfeld's concern with what they perceived to be the "rather repetitious and even static" character of Bertalanffy's work on GST. It is curious that neither GST-devotees nor their critics seem to have been concerned as to why Bertalanffy's work appeared repetitious and static.

In his later years Bertalanffy was held in high regard by "his fellow megalomaniacs" (Laszlo, 1972; Naughton, 1979). His notoriety may have allowed him to publish repetitiously, but then again repetitious publication is not rare in the scientific literature. Further, there appears no reason to believe that he achieved recognition as the modern father of GST, through anything other than lonely perseverance during his early years as a scientist. Hence, one ought to consider that there might be a legitimate purpose behind Bertalanffy's "megalomania", but that perhaps his means were inadequate or inappropriate. What was the purpose of GST as perceived by Bertalanffy? And why did his efforts to achieve the objective appear redundant and ineffective? Where was the inadequacy or inappropriateness in his means?

Bertalanffy was a biologist, and was consequently concerned with studying organisms as such. The behavior of living things presents something of a practical dilemma for traditional reductionist approaches in science, principally because the behavior of living things can not be reliably predicted from a knowledge of the parts of which they are comprised. Concern with the "whole as more than the sum of its parts" dates at least to the time of Aristotle. Bertalanffy, however, was in effect concerned with determining what knowledge is necessary before the behavior of a whole can be predicted from a knowledge of its parts.

He got off to an impressive start toward this objective (at least as early as 1945) by making two propositions (Bertalanffy, 1968, p. 55). First, when dealing with "complexes" (wholes) of "elements" (parts), three kinds of distinction may be made with respect to the elements in a complex: species, number, and inter-element relations. Second, the behavior of the whole will not be reliably predictable until one has knowledge of the quantities and qualities of the parts *and the relations among those parts* in the whole.

Since reductionistic approaches permit direct determination of the number and identity (species) of parts, but not the *functional* relations among them, Bertalanffy's statements were a challenge to, but not a rejection of, reductionistic approaches and the atomistic/mechanistic view of the universe. He did not find reductionistic approaches necessarily objectionable, just inadequate in many cases -- particularly in the study and explanation of the phenomena of most interest to biologists. And his propositions explained this inadequacy. While reductionistic analyses can resolve a whole into its parts and permit the identification of the resolved parts, they can not provide reliable information on the relations among those parts in the functional whole -- for those relations exist only in the functioning whole. A need for some non-reductionistic approach was apparent. And so we arrive, finally, at a positive statement by Bertalanffy (1968, p. 37) of what GST was to be and do, "General systems theory...is a general science of 'wholeness'" and its subject is the formulation of principles that are valid for wholes (systems) *not understandable by investigation of their isolated parts*. It seems reasonable to conclude that for Bertalanffy the purpose of GST was to provide scientists with a (practical and investigatively useful) conceptual tool which would enable development of a knowledge of holistic properties as reductionistic approaches had enabled experimental evaluation of atomistic properties.

Having identified Bertalanffy's definition of GST, its subject matter and purpose, we may move on to consider the reasons for the discussed ineffectiveness of his writings.

PHILOSOPHICAL FOUNDATIONS: ONTOLOGIES[5], EPISTEMOLOGIES[6], AND CONCEPTUAL METHODOLOGIES

It is helpful here to try to identify Bertalanffy's philosophical position, his intellectual belief system, at least as far as its effect is apparent in his writings. From his definition of GST and its subject matter and other statements (p. 55) one may infer he held "holistic" beliefs. On the other hand, in so far as his propositions (ibid., p.55) suggest that the behavior of a whole is determined by the quantities and qualities of and relations among its parts, he also held "mechanistic" beliefs. His philosophy of science differed from that of most modern scientists, but not because he denied "mechanism", "realism", or "atomism", without which the pursuit of scientific knowledge would be pointless. Neither did he deny the possibility that ultimately the "atomistic" philosophy, as well as its modern realization in the "mechanistic" belief that all natural phenomena will ultimately be explainable in terms of the laws of chemistry and physics, might be correct. Instead, he argued that a conceptual tool for dealing with "wholeness" was simply a practical necessity for scientific investigation of phenomena associated with organized complex wholes (ibid. p. 18, 48, 247).

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[5] The study of or theories about the nature of being, existence, or that which exists as it can be experienced.

[6] The study of or theories about the nature and grounds of knowledge, especially with reference to its limits and validity.

Perspectivism and Reductionism

It is noteworthy that Bertalanffy apparently never explored the terms "holism" or "atomism" in his writings. Instead he offered what at first appears to be an alternative concept which he called "perspectivism". He explored it only very little (ibid., p. 49, 247). Yet, perhaps the most enlightening and concise, though indirect, description of the intent of GST and the philosophy underlying it, is derivable from his first and apparently only useful discussion of perspectivism:

> We come, then, to a conception which in contrast to reductionism, we may call perspectivism. We cannot reduce the biological, behavioral, and social levels to the lowest level, that of the constructs and laws of physics. We can, however, find constructs and possibly laws within the individual levels. (p. 49).

Perspectivism is presented as an alternative to reductionism. However, the difference between the two is subtle. While reductionism might be considered the conceptual tool of atomism, and perspectivism potentially the conceptual tool of holism, perspectivism retains some "atomistic" character. It implies a hierarchy of cognitively accessible (epistemological or "knowable") levels. Phenomena of some empirical reference level are the "parts" by which higher (more complex) levels are defined, and those the "parts" of even higher ones, and so on, until at some sufficiently high level a finite epistemological "universe" is defined and the phenomena of the initial reference level become effectively "atomic" properties of that "universe". These same properties, cognitively "atomic" for the higher levels just discussed, are "sub-atomic" for still higher levels, but "universal" for relevant levels much lower than the initial reference level. In this the fundamental difference between reductionism and perspectivism becomes apparent. Reductionism is derived from an ontological belief in the empirical verifiability of an absolute level -- for conventional modern mechanicists that level is or will eventually be described by the laws of physics. Perspectivism, in contrast, is based on an epistemological belief that only relative "atomic" levels are cognitively accessible and therefore empirically verifiable, and that while some absolute irreducible level may exist, <u>its irreducibility is not empirically verifiable</u> (Wheeler 1980, p. 134).

A weakness in Bertalanffy's presentation of perspectivism is indicative of the inadequacies in his writings that have dismayed GST advocates and critics alike. He contrasted perspectivism and science concerned with "wholeness" to reductionism and science concerned with "parts". It might seem reasonable to conclude then, as many apparently have, that GST and perspectivism are synonymous with or in some way particularly endeared to holism. This, however, is a misconception that Bertalanffy should have taken measures to prevent (assuming, of course, he did not share this view). Unfortunately, he did not explicitly mention the nature of the relationship between perspectivism and holism (or even "wholeness") as he did the contrast between perspectivism and reductionism.

One is forced to second guess Bertalanffy's motivation in this regard. Was it simply an oversight? Perhaps his dedication to holism as a personal belief system was so strong -- as much of his writing suggests -- that the need for explicit discussion did not strike him. Or, though his writings do not suggest it, perhaps he considered it obvious that holism, as atomism or mechanicism, is an ontological concept -- a belief about the universe as it is -- and thus may be allied with but not meaningfully compared to perspectivism which, as reductionism, is an epistemological concept, concerned with the universe as it can be known or, for a scientist, observed and described. It is useful to examine the relationship of each of the ontological concepts of interest here to both reductionism and perspectivism. This enables, through a more thorough appreciation of Bertalanffy's contrast of perspectivism and reductionism, a better understanding of GST.

Reductionism and Science Based on an Ontology

Reductionism is a conceptual methodology derived from atomism. It is the conceptual foundation of reductionistic experimental approaches. In its modern form it is a mechanistic epistemology which implicitly assumes (i.e., is founded on the ontological belief that) the universe is mechanistic and imposes on any phenomena which it is used to study, the character of a mechanistic universe -- a universe in which every whole is ultimately no more than a mechanistic assembly of some ultimately irreducible physical (or, for more practical or broader-minded mechanicists perhaps, chemical) parts. Now it is here that the limitations of modern mechanistic reductionism -- that is, the reductionism that Bertalanffy concerned himself with -- and its relationship to holism become apparent.

Observations and interpretations based on mechanistic reductionism enable definition of wholes in terms of their physical (or chemical) parts <u>only</u>. If the purpose of science is to objectively and accurately describe the universe and phenomena occurring within it, then reductionism is an inadequate conceptual methodology because it may not be accurate and cannot be objective. Not accurate because it will provide an accurate description of a universe only in so far as that universe is, in fact, reducible, i.e., atomistic. Not objective because even if a universe were -- perish the thought -- not atomistic, it could only appear to be either atomistic or senseless to anyone observationally dependent on reductionism. Reductionistic science simply has no "scientific" access to evidence that the universe, or any phenomena occurring in it, might be anything other than atomistic -- say, for example, holistic.

Perspectivism and Science Based on an Epistemology

Now if the relationships of perspectivism to atomism and holism were simply the reverse of those of reductionism, then, with respect to the needs of science, perspectivism would suffer from the same fundamental inadequacy that debilitates reductionism, although perhaps opposite in observational effect. This is not the case, though. Whereas the adequacy of reductionism is dependent on the ontological validity of atomism, perspectivism is ontologically ambivalent.

Perspectivism is more skeptical, "a more modest view", than reductionism in that it is based on the (epistemological) belief that discoursive thinking can never exhaust the infinite manifoldness of material reality (Nicholas of Cusa, cited by Bertalanffy, 1968, p. 245-248). Perspectivism is holistic because it emphasizes the functional relation between parts and wholes, but it is not dependent on holism because it does not hold that the universe is the only true whole. Perspectivism is atomistic because it requires that wholes are composed of simpler parts, but it is not dependent on the validity of atomism because it does not require that the universe be composed of simple, irreducible particles. Neither does it rule out the possibility that either atomism or holism or both might in fact be correct. Instead it is an acceptance of complex functional wholes as humanly incomprehensible when described in terms of the universe as the single ultimate whole or as an immense number of ultimately irreducible "atomic" particles. Perspectivism is set then, not on an ontological, but on an epistemological foundation.

Now the ambition of science dependent on methods grounded on an epistemology must be humbler than that of science dependent on methods grounded on an ontology. Science observationally dependent on an epistemologically based conceptual methodology, say perspectivism, can not purport to accurately and objectively describe the universe as it is, but the universe and phenomena occurring within it only to the extent that these are accessible to human cognition and amenable to linguistic description. Perspectivistic science is more practical and objective in a broader sense than reductionistic science can be. If it should be that ontological reality is not fully cognitively accessible or linguistically describable, then perspectivistic science can accept a full description of reality as outside the domain of science without excluding the possibility that parts of reality are cognitively accessible -- hence without disabling science as a materially effective intellectual art-- and without presuming the nature of reality as a whole. Perspectivistic science presumes only that human knowledge is necessarily restricted to that of phenomena which are cognitively accessible for humans, and scientific knowledge is necessarily restricted to those portions of human knowledge which are describable in scientifically acceptable language.

RESOLVING THE AMBIGUITY IN G.S.T.

This reveals another weakness in Bertalanffy's writing and a point essential to an appreciation of GST. Bertalanffy's contrast of perspectivism and reductionism implies that GST must be an epistemology upon which perspectivism is based. Bertalanffy, though, referred to GST as a "general science of wholeness", a "model of certain general aspects of reality", a "theory", "discipline", "methodological maxim", "perspective", and "paradigm". Although there are meaningful relationships among all these descriptive terms, they are not synonymous with each other or with "epistemology", and the term GST, never specifically defined, is consequently stricken with overt ambiguity. Such ambiguity has been more clearly perceived by GST-opponents like Naughton (1979) than by "systems" devotees, whose practical efforts have been severely hampered by their apparently naive indifference to it and its importance.

Laszlo (1975) has suggested that the GST ambiguity of concern here did not exist for Bertalanffy, whose native language was German. The system/theory/science vocabulary in German, Laszlo suggests, has "broader meaning" than the closest English vocabulary. One may infer then that the GST terminological ambiguity never arose for Bertalanffy and might be internally resolved for those who speak German natively, or at least fluently. This, unfortunately, is of little help to those of us who are less than fluent in German. Worse still, it implies that scientists who might wish to benefit from GST should first become fluent in German -- a disheartening prospect. However, even if one were fluent in German, the "broader meaning" of the relevant vocabulary would require the reader either to have prior knowledge of the intent of each of Bertalanffy's usages, or to select the intended meaning or combination of meanings by trial and error each time a "broadly" defined term was encountered. Fortunately Bertalanffy was either quite proficient in the English language, or as a non-native speaker, unusually willing to consult a dictionary during his writing efforts. The present effort continues on the assumption his intended meanings are accessible to Bertalanffy's English language readers who are sufficiently cautious, patient, and equipped with a historically appropriate dictionary.

Laszlo (1975), like Bertalanffy (1968), described GST through a negation, stating that GST should not be construed as a "(scientific) 'theory of general systems'". This leaves English-speaking scientists with perhaps only one other sensible, alternative English word order for GST, that might more accurately carry Bertalanffy's Germanic intent into connotative English: GST is a general theory of systems. Now this alternative ordering implies, as Laszlo suggested, that GST is not a scientific theory as this is commonly understood by English-speaking scientists, but a systematically presented set of empirical, axiomatic, or philosophical concepts about "systems" in general. It should be noted that this interpretation of GST is obtainable directly, without changing word order, even in English, if one is willing to consider alternate, non-disciplinarian definitions of "science" and "theory" which are available in most dictionaries. This is an encouraging and interesting observation which suggests a probable correctness for the assumption that the English language is no less GST-capable than the German and also suggests the possibility that GST might be a theory more properly considered in some field other than "science".

Laszlo (1975) stated that GST is not a single theory in the scientific sense, but a new paradigm for the development of theories. Given that Bertalanffy (1968, 1975) made similar statements, it seems reasonable to conclude that GST is not a "general science" in the commonly accepted English connotation of these words, but, in fact, an epistemology, a set of axiomatic or philosophical concepts on the validity and limits of empirically obtainable knowledge. Although Bertalanffy (1975, p. 165-169) at one point wrote on "systems ontology" as though to suggest there might in fact be some ontologically real "systems", that discussion resolved into a statement of the conditional nature of scientific knowledge and led to a discussion of "systems epistemology". He pointed out that physical observation is questionable and that interactions, the existence of which is essential to the concept of systems, are themselves human conceptual constructs. He wrote, "...the distinction between real objects and systems as given in observation, and conceptual constructs and systems cannot be drawn in any common sense way." Hence, neither "systems" nor GST should be understood as ontological concepts, but as epistemological concepts, conceptual tools humans use to cognitively deal with ontological reality.

The central GST ambiguity is eliminated if one holds that what Bertalanffy called GST was actually comprised of, at least, a realistic epistemology; a conceptual methodology based on that epistemology, i.e. perspectivism; and an abstract experimental procedure, the "systems", or more properly, perspectivistic approach -- so-called "systems approaches" should be considered to be in accord with GST only when they can be demonstrated to be perspectivistic approaches. It now becomes necessary to define as unequivocally as practically possible, the GST epistemology, perspectivism, and the perspectivistic approach.

THE G.S.T. EPISTEMOLOGY: WE ARE NOT OMNISCIENT

The GST epistemology has already been described as the belief upon which perspectivism is based. That belief is, reiterating Bertalanffy's (1968, p. 248) citation of Nicholas of Cusa, discoursive thinking can never exhaust the infinite manifoldness of ultimate reality. Or in the words of the economist, von Hayek (1967, p. 90), "The crucial fact of our lives is that we are not omniscient..." Or in the words of Szent-Gyorgyi (1964), "The mind is not a bottomless pit..." Based on my own cautious study of Bertalanffy's writings and the understanding, derived from that study, of his intentions and concerns as a scientist, I have defined the GST epistemology as the belief that, relative to explicit cognizance of the spatiotemporal complexity and variety of the material universe, human cognitive capacity is distinctly inadequate.

Szent-Gyorgyi (1964) presented an enjoyable description of a symptom in science of the limitations of human cognitive capacity. He wrote in a recollection of time he spent at the Institute for Advanced Studies in Princeton,

> ...I revealed that in any living system there are more than two electrons,....the physicists...With all their computers...could not say what the third electron might do. The remarkable thing is that it knows exactly what to do. So that little electron knows something all the wise men of Princeton don't...

(Bertalanffy and other early systems writers often cited the three-body problem of physics as indicative of the weakness of reductionistic science.) The present author suggests that little electron knows something all the wise men can't. That something is how to be an electron; knowing, without calculating, pondering, or experimenting, what an electron should do next. It is appropriate that such an epistemology should be presented by scientists since it allows an examination of the nature, validity, and limits of empirically verifiable, and consequently, of scientific, knowledge.

EMPIRICAL KNOWLEDGE

Empirical knowledge may be defined as that which exists in the "experiencer" or "knower" due to its relationship with the "experienced" or "known", and, under the GST epistemology, would seem subject to a conceptual form of the law of the minimum. If the "known" phenomenon is functionally "simpler" than the cognitive capability of the "knower", then the knowledge of that phenomena is limited by the simplicity of the "known". On the other hand, if the complexity or immensity of the "known" phenomenon exceeds the cognitive capability of the "knower", then the knowledge of that phenomenon is limited by the cognitive capability of the "knower". There is, also, a third possible case. If the complexity of the cognitive capability of the "knower" is functionally equivalent to the complexity of the known phenomenon then, given no other information, the "knowledge-limiting factor" can not be identified.

Empirical knowledge, as defined here (above), implies the empirical existence of both the "knower" and the "known", i.e., the existence of a physical reality in which both "knower" and "known" simultaneously exist. It does not, however, provide the means for determining which is the "knower" and which the "known". For example, assume there is a live fish in water. Under the definition given above, since the water is displaced and altered by the fish one may state that the water has knowledge of the fish, that the water is the "knower". On the other hand, since the fish displaces and manipulates and may be displaced and manipulated by the water, one may say the fish has knowledge of the water. How then may one distinguish "knower" from "known"? And what does all this have to do with perspectivism?

Non-Purposive Empirical Knowledge

Distinguishing the "knower" from the "known" requires more information than is available in the stated definition of empirical knowledge. Two general types of knowledge, which are GST-epistemologically valid, can be distinguished on the basis of observable relationships between "knower" and "known". In the preceding fishy example, the water's knowledge of the fish exemplifies one of these types. One might call this type of empirical knowledge "passive", but for reasons which will become apparent, I will call it non-purposive knowledge.

Innumerable empirical observations by humans indicate that the existence of water is not dependent on the spatiotemporally simultaneous existence of fish, i.e., on water experiencing fish.[7] On the other hand, human empirical observations also lead us to believe that the existence of live fish is dependent on the simultaneous existence of water.[8] Or, restating, the existence of organisms recognizable as fish

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[7] Or, in the case of Szent-Gyorgyi's (1964) third electron: whatever electrons really are, they do not need science or scientists in order to know how to behave as electrons.

[8] The live scientist/electron case is not so empirically clear cut. Not because the relationship is necessarily different in principle, but

depends not only on those organisms knowing how to develop and maintain "fishy" shapes, colors, etc., but also on their knowing how to use water to accomplish that development and maintenance -- fish must necessarily "know" water exists, and how to manipulate it in order to move, eat, breathe, etc.[9] Hence, the second general type of knowledge, which I will call purposive.

Purposive Empirical Knowledge

Non-purposive empirical knowledge is that which exists in the "knower" as a consequence of its relationship with the "known" and which, though necessary for the existence of the "known", can not be empirically construed as having any purpose with respect to the existence of the "knower". Purposive knowledge is that which exists in the "knower" as a consequence of its relationship with the "known" and which has purpose in the sense that the material existence of the "knower" is dependent upon it. Thus, a purposive "knower" is necessarily (or "obligately") concerned with externalities while a non-purposive "knower" is not.

Therefore, to differentiate the "knower" from the "known", it is necessary to determine whether the knowledge of interest is purposive or non-purposive. Scientific knowledge must be construed as purposive knowledge since it develops in, and to the existential benefit of, the

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because the existence of electrons is not so readily or certainly, i.e. sensorily, demonstrable as the existence of the substance we call water.

[9] Water is an opportunity, but not an obligation, for fish to exist. More formally, the existence of water is a necessary, but not sufficient, condition for the existence of fish. Water can not be construed as the cause of fish and fish can not be construed as the cause of water.

"knower"; and empirical observation suggests -- hence definition requires -- that it cannot exist in and is of no existential benefit to non-purposive "knowers". Science then may be defined as a (methodical) pursuit of purposive knowledge and scientists, consequently, must be concerned with the nature of purposive knowledge.

Passively Acquired Purposive Empirical Knowledge

Two GST-epistemologically valid types of purposive knowledge can be distinguished on the basis of the means of acquisition by an individual "knower". The first type, passively acquired, is that knowledge an individual purposive "knower" must have a priori in order to exist. Such knowledge is fundamentally essential and consistently reliable in the knower's existential universe, and not available to the individual through active knowledge acquisition. Continuing with the fish as an example, the individual fish must know how to exchange gases with water ("breathe") in a manner appropriate for each stage of its development, from egg to adult. And it must possess such knowledge before, and implement it no later than, the moment it is needed because the delay associated with discerning the need for such knowledge, investigation, and development of appropriate knowledge of gas exchange and physiological options (i.e., with "thinking about it") -- not to mention implementation of the selected options -- would necessarily result in asphyxiation. Similarly, a fish must know when and what kind of appendages and body shape are appropriate to sustaining its existence in water; for failure to develop a streamlined body and fins at the appropriate time would result in starvation or falling victim to environmental hazards. That is, knowledge of what is functionally essential for existence in a fish's watery existential universe must be prior knowledge in any fish. If this were not so, then each individual organism would need to independently rediscover how to build and coordinate a morphological, physiological, and behavioral complex appropriate for existence in whatever existential universe it might find itself. And if this were so, then fish eggs might give rise to lizards or worms or humans or maybe even plants, according to where the individual developed. Empirical observation constrains us from such logical but outrageous inferences though, because, as an example, fish eggs either develop into fish or cease to exist -- die.

It should be clear at this point that, for living organisms, an individual's genetic and cytoplasmic inheritance may be construed as its passively acquired purposive empirical knowledge. The complexity and depth of passively acquirable knowledge is incomprehensible (or, perhaps more accurately, "indescribable") and its effectiveness wondrous, even in a bacterium, let alone in a fish or a human. The passively acquired knowledge possessed by an individual is nevertheless limited, and its expression a material obligation -- a fertilized fish egg can only become a fish or die, and nothing more or other than that. There is a further limitation on passively acquired knowledge that is critical to an understanding of perspectivism.

An individual must be able to sense and respond effectively to those events which are possible in its existential universe and which affect its material existence. The adequacy of passively acquirable knowledge in this regard is dependent on the condition that sensory and response capabilities which were existentially adequate for its progenitors will also be effective for the extant individual. Hence, as long as the chemical properties of water do not change, the passively acquired knowledge of, say, gas exchange functionally adequate for a fish's progenitors will remain adequate. Likewise, as long as the physical properties of water do not change, passively acquired knowledge of a mechanically effective morphology could remain adequate. However, the existential universe of a fish includes far more than a collection of water molecules in the liquid state. There are other material events, not so consistent as the existence and properties of water, which must be effectively dealt with as well.

Actively Acquired Purposive Empirical Knowledge

Consider, for example, a fish's need to acquire food. If the fish is predatory it will at times in its life prey upon others and, probably, be subject to being preyed upon by others. If reliably abundant prey, when pursued, always attempted to evade the predatory attacks of the fish's ancestors by, say, turning abruptly to the left, then a reliably successful attack behavior could be part of the fish's passively acquired knowledge. Difficulties arise in practice, however. If the left-turning prey should begin to turn right, then the fish's pre-programmed attack pattern would be inadequate -- in fact, a complete failure, and the behaviorally inadequate predatory fish would starve. It is existentially essential therefore that the individual possess more than a functionally adequate passively acquired understanding of the historically consistent phenomena which occur in its existential universe. The individual must also possess a functionally adequate means of dealing with existentially relevant phenomena which were unknown to its ancestors or not sufficiently consistent in its ancestral history for functional knowledge of them to have become part of its passively acquired knowledge. Hence, the second type of purposive empirical knowledge: actively acquired.

Actively and passively acquired knowledge differ in that while passively acquired knowledge defines internal capabilities (structure and function) in terms of external phenomena which are (presumed for historical/empirical reasons to be) temporospatially universal consistencies, actively acquired knowledge defines external phenomena which are temporospatially local consistencies in terms of internal capabilities (or "consistencies"). Passively acquired knowledge assures the functionality of the individual in terms of phenomena which are reliably consistent throughout the individual's existential universe -- that is, assures that the individual makes sense in terms of its existential universe. Passively acquired knowledge defines the limits of the individual's experiential capabilities and, consequently, the potential limits of its experiential universe. Actively acquired knowledge, in contrast, is concerned with the functionality of locally consistent phenomena in terms of the existential needs of the individual -- i.e., allows the individual to experientially "make sense" of (experienced) external phenomena with respect to its existential needs. Which knowledge an individual <u>might</u> actively acquire is limited to that of its existential universe, that is, that of the portion of the ontological universe in which the individual is equipped, by the knowledge passively acquired from its progenitors, to exist. Which knowledge it <u>does</u> actively acquire is limited to that of its own experiential universe, i.e., that portion of its existential universe with which it has experience. Considering the fish again, it has passively acquired knowledge that it must periodically replenish its internal energy and nutrient supplies -- it knows when and how "to get hungry" -- that adequate food supplies (prey) exist in its universe, and how to use -- digest and metabolize -- food once ingested, but consistently reliable knowledge of such universally inconsistent phenomena as where and when prey occur, how to efficiently capture different prey, or when, while pursuing prey, "discretion is the better part of valor", can not be passively acquired. By way of clarification through definition, the experiential universe is that portion of the existential (epistemological) universe in which the individual exists, and to which its actively acquired knowledge is limited. The existential universe is that portion of the material (ontological) universe in which the individual is prepared by its passively acquired knowledge to exist and which it is prepared to "know".

The Ability to Actively Acquire Knowledge Cannot Be Actively Acquired

Because it is knowledge of external phenomena which are locally instead of universally consistent, the actively acquired knowledge which permitted (an individual's) progenitors to exist may not enable the (individual) progeny to deal adequately with "new" existentially relevant externals, or even new combinations of "old" externals. Further, empirical observation suggests that every individual is faced with the need to develop temporospatially appropriate actively acquired knowledge and that lack of the ability to meet that need will result in existential failure just as certainly as an inability to breathe, digest food, etc. Hence, the ability to actively acquire knowledge that is functionally relevant and reliable in the individual's existential universe is as fundamentally essential as, say, knowing how to breathe.

I suggest, for two reasons, that the fundamental knowledge of how to actively acquire knowledge can not itself be actively acquired. The first reason: Either one has the ability to "learn" or one does not. If one does, then one will learn. If one does not have the ability to learn, then one can not learn, hence, one can not learn to learn. The second reason: most purposive knowers constantly face challenges which, if these knowers are to maintain their existence, require material demonstration of the existential validity and effectiveness of their knowledge. Hence, it is unlikely they have the opportunity to concern themselves with how they acquired the knowledge that permits them to exist. Therefore, the ability to actively acquire knowledge is not functionally available to the individual as actively acquired knowledge, and therefore must be passively acquired -- that is, "it is the nature of the beast".

PERSPECTIVISM AS A CONCEPTUAL METHODOLOGY

This, then, is a fundamental concept underlying perspectivism: Any extant individual purposive knower must inherently be able to reason and learn adequately to maintain itself in a specific existential universe, the limits of which are defined by the knowledge passively acquired from its progenitors; for it can not maintain its existence with less than adequate knowledge and there can be no empirical verification -- and therefore should be no assumption -- that it possesses more than adequate knowledge. This last point is subtle but crucial, for it lies at the heart of the fundamental concern of this discussion, and bears directly on any consideration of science, scientific methods, or the limits of scientific knowledge. Scientists have failed to recognize that over-zealous application of an anti-anthropomorphic doctrine implies an anthropocentric attitude which is, because of its subtlety, an even greater threat to our inherently weak human objectivity than the more easily diagnosed anthropomorphism (Berger and Berry, 1988). Perspectivism is an unorthodox conceptual methodology with a "more modest view" than the more conventional reductionism or less workable holism. Perspectivism holds, for example, that a fish can and must think, but as a fish and only as a fish; just as a human can and must think, but as, and only as, a human (Bertalanffy, 1968, p. 245-248). Those who take a more conventional (reductionistic) view would likely agree with the doubly biased -- anthropocentric and quantity-minded (atomistic) -- statement that while the ability of fish to actively acquire knowledge would not be existentially adequate for humans, human active knowledge acquisition capabilities would be more than adequate for fish. To those taking a perspectivistic view, the capabilities of fish are simply not appropriate to human existential needs, as human capabilities are not to the existential needs of fish.

Not A Futilitarian Relativism

It is important to emphasize at this point, Bertalanffy's (1968, p. 239-240) appropriate concern that perspectivism should not be construed as another form of futilitarian relativism. For, although it is relativistic, it does not hold that knowledge has a "purely conventional and utilitarian character", and should not give rise to an "emotional background of ... ultimate futility". A piece from Bertalanffy's writing on this point succinctly summarizes the concepts presented in the last few paragraphs.

> As far as direct experience is concerned, the categories of perception as determined by the biophysiological organization of the species concerned cannot be completely 'wrong,' fortuitous, and arbitrary. Rather they must, in a certain way and to a certain extent, correspond to 'reality' ... Any organism, man included, ... has to react to stimuli coming from outside, according to its innate psychophysical equipment. There is a latitude in what is picked up as a stimulus, ... However, its perception must allow the animal to find its way in the world. This would be impossible if the categories of experience, such as time, space, substance, causality, were entirely deceptive. The categories of experience have arisen in biological evolution, and have continually to justify themselves in the struggle for existence. If they would not, in some way, correspond to reality, appropriate reaction would be impossible, and such organism would quickly be eliminated by selection.

The Priority of Qualitative Knowledge

As a conceptual methodology, perspectivism gives priority to qualitative, rather than quantitative, aspects of the ability to actively acquire knowledge, and in so doing, diminishes the anthropocentricity -- increases the objectivity -- of human observation and description. For example, to the perspectivist, the fish brain may, in some quantitatively measurable ways, be different from the human brain, but the relevance of organ size or numbers of neurons and synapses to the quantitative aspects of existentially necessary knowledge can not be reliably inferred. Gould and Marler (1987) discuss research results which support this position. An example, experiments where seed-caching chickadees with their "tiny" brains could "remember the locations of hundreds of hidden seeds, whereas human beings begin to forget after hiding about a dozen." Perspectivism suggests that the more general recognition that this is, say, a fish brain while that is a human brain takes epistemological precedence over the quantitative recognition that this brain has a mass of less than 1 gram while that has a mass of over 1000 grams. Perspectivism prohibits presumption of an association of quantity with quality. Generalizations such as "If a little is good, a lot is better", "bigger is better", or "small is beautiful" are not epistemologically valid -- let alone empirically supported -- because they confuse the qualitatively and quantitatively describable aspects of the observable phenomena. While it would be perspectivistically meaningful to state that fish brain "A" is materially larger than fish brain "B", it would be redundant to state that fish brain "A" is smaller than human brain "C" because such a size difference is implicit in the qualities "fish" and "human". Further, even if some strange excess of growth should occur in a fish so that its brain mass approximated that of humans, there is no reason to believe that its brain would be -- at best, assuming no deformative growth -- anything other than a quantitatively unusual ("large"), qualitatively consistent ("fish") phenomenon (brain).

This should not be understood to mean that perspectivists regard quantitative data as (ontologically) "second-rate" information. Bertalanffy (1968, p. 238) is again explicit,

> Much harm has been done in science by playing one aspect against the other and so, in the elementaristic approach, to neglect and deny obvious and most important consequences; or, in the holistic approach, to deny the fundamental importance and necessity of analysis.

Perspectivists, for epistemological reasons, must consider quantitative determinations as secondary since such the ability to make quantitative determinations presupposes a qualitative awareness of the existential (functional) relevance of the subject phenomenon, i.e., the cognitive accessibility of the phenomenon. One of the principles of perspectivism -- a simple principle of which any effective scientist is implicitly aware -- may be simply stated as, "One must know where one is looking before one can know what to look for, and what to look for, before one can hope to determine how much of it there is."

A word of caution: It is easy to misconstrue "existential functionality", as used here, as synonymous with "relevance" as used in the current popular sense. This would imply that pure academic research should not be pursued. This is clearly a misapprehension of the concepts involved because "existential functionality" is apparent as a universal human-relative quality synonymous with "cognitive accessibility" not by the majority, but by any human, because cognitive accessibility for any human mind necessarily implies potential existential, but not necessarily socio-economic, relevance for each human being. Bertalanffy also speaks to this.

The Problem: Existing Without Being Omniscient

Over the full range of qualitatively different purposive knowers, there is no empirical evidence that there exists, or has ever existed, any purposive knower with complete actively acquired knowledge of its experiential universe -- much less its existential, or, even more ambitiously, its ontological universe. There is a functional limit on the quantity of knowledge any spatiotemporally finite individual can possess. No individual can know the current status of all, or even most of, the separate material factors and relationships which have the potential to affect that individual's material existence. And apparently all empirically observable purposive knowers are faced with this quantitatively insurmountable problem. Still, they do exist and their existence necessarily implies that all extant purposive knowers --

not to mention their ancestors and descendants -- somehow overcome this problem. That is, at least one solution to this problem of surviving in a universe of infinite manifoldness through the use of only limited knowledge and cognitive capabilities must exist, and every extant purposive knower must be presumed competent in the functional application of such a solution.

The Solution: A Single, Reliable, Fully General Approach

This conclusion has three possible consequences. First, there may be an immense number of solutions -- giving rise to the need to consider the possibility that each purposive knower might discover its own. Second, there may be a unique general solution -- giving rise to the need to consider the possibility that all purposive knowers exist as a consequence of functional competency in the application of that solution. Third, the first and second consequences may both be true. It is the third, most complicated of the three consequences which seems most compatible with the empirical information that is available within human cognitive constraints. However, the third consequence is resolvable, because the three consequences are not necessarily mutually exclusive. The existence of a unique general solution (second consequence) would not preclude, and in fact would require, the development of an immense number of special solutions (first consequence) through application of the general solution to an immense number of special cases (third consequence). Hence, we arrive at the conclusion that although there must be an immense number of special solutions, there might be a general solution and, if there is, that all purposive knowers could be expected to be functionally competent in it.

There is another reason to consider that there might be a general solution to the problem of existing while equipped with only limited knowledge and cognitive capacity. To argue that there might not be a general solution is equivalent to arguing against the estimates that it would take more time than the present material universe has been in existence for randomly sequencing amino acids to become organized into self-reproducing structures (Madore and Freeman, 1987). In fact, there are reasons to believe that the development of such knowledge by random observation and apprehension is probably even less likely than the development of self-reproducing structures from randomly sequencing amino acids. One such reason is that once knowledge that is not passively acquired becomes necessary, it must be acquired in short order or it will not be effective; i.e., the purposive knower in question will cease to exist. Another reason is that there is an immense number and variety of potentially existentially relevant phenomena in an individual's experiential universe, and specific phenomena relevant today may not be tomorrow. Still another is that, if there were no general solution available, then the individual knower would have to "start from scratch" each time a new phenomenon is encountered. So, the ubiquity of the ability to actively acquire existentially adequate ("effective") knowledge despite limited cognitive capacity; and, the unlikelihood that each individual purposive knower has the ability or sufficient opportunity to discover its own general solution to the problems which arise as a consequence of the obligate limitations on its cognitive capacity; lead to the conclusion that the general solution is inherent in -- i.e., part of the passively acquired knowledge of -- each existentially adequate purposive knower.

Consideration of the principal tenet of GST epistemology and some aspects of its associated conceptual methodology, perspectivism, suggests then, that there is a general solution to the problem of surviving in "the infinite manifoldness of material reality" with only finite cognitive capabilities. Further, it suggests that all existentially effective purposive knowers inherit competency in application of that general solution to a peculiar range of existentially relevant phenomena; that range being defined by their "biophysiological organization", which is itself the result of passive application of that general solution by their progenitors. It is the opinion of the present author that the idealized objective of GST is to discern the character of this general solution and present it, to the extent possible, in a form compatible with transmission as actively acquired knowledge and thus bring this general solution as far as possible into the realm of explicit, conscious application. The remainder of this presentation is directed toward that objective.

The Language: Mathematics or Logic

Bertalanffy (1968, p. 37) held that in its elaborate form GST "would be a logico-mathematical discipline, in itself purely formal but applicable to the various empirical sciences." Checkland (1981, p. 93) stated that Bertalanffy and the other founders of the Society for General Systems Research shared "Bertalanffy's unchanging view ... that there would arise as a result of work in different fields a high-level meta-theory of systems, mathematically expressed", but that "The general theory envisaged ... has certainly not emerged". Bertalanffy (1968) wrote in the 1945 article in which he introduced GST that GST "in its developed form, would replace what is known as 'theory of categories' by an exact system of logico-mathematical laws. General notions as yet unexpressed in the vernacular would acquire the unambiguous and exact expression possible only in mathematical language." Three important points may be extracted from these statements. First, it is reasonable to infer that throughout his lifelong interest in GST Bertalanffy held the view that no mathematically expressed meta-theory of systems that might satisfy the objectives of GST had yet "emerged". Second, not only is it clarified that in its essential form GST would be a logico-mathematical discipline, but that it would replace (or supersede) "theory of categories". Third, emphasis is given to the importance of a language capable of unambiguous and exact expression, in Bertalanffy's view a "mathematical language". It is my opinion that Bertalanffy was correct in suggesting that in its developed form it would replace "theory of categories", but in error regarding the lack of a formal logic capable of replacing "theory of categories" and compatible with GST or the natural sciences. I suggest that this error was the consequence of a conflict within Bertalanffy between a traditional scientist's loyalty to mathematics, and an intuitive sense that the requirements of GST could not be met by mathematics. An observable symptom of this conflict is the preservation of ambivalence about languages other than mathematics and ambiguity in use of the words "mathematics" and "mathematical".

"Mathematical" is defined as that of or pertaining to mathematics, "the science of numbers and their operations, interrelations, combinations, generalizations, and abstractions ..." (Webster's New Collegiate Dictionary, 1981) or the science "treating of the exact relations between quantities or magnitudes and operations..." (Webster's Collegiate Dictionary, 1946). One also finds a second simpler, less restrictive, definition: "rigorously exact: precise". There is, then, an inherent ambiguity in the word "mathematical", and consequently in the word "mathematics" as well. GST writers generally have not given any indication of which meaning they intended for these words. The following considers the meaning of these words as used in the GST literature, whether GST writers, including Bertalanffy, have used "mathematics" to mean "the science of quantities...", and whether this meaning is the appropriate one for use in efforts to describe GST.

The Bertalanffian view: mathematics

In the GST literature in general it is contextually difficult to consider that "mathematical" might have been intended to mean anything other than that which is related to "the science of numbers" and "mathematical language" anything other than the "rigorously exact" language of that science. Bertalanffy held the exactness of the language of mathematics in high regard, but, also held throughout his lifetime that GST must accept the use of languages more ambiguous than that of mathematics. For example, in his 1945 introduction of GST (Bertalanffy, 1968) one finds reference to

> "...phenomena where the general principles can be described in ordinary language though they can not be formulated in mathematical terms."

In 1967 (ibid.),

> This does not mean that models formulated in ordinary language are to be despised or refused... A verbal model is better than no model at all, or a model which, because it can be formulated mathematically, is forcibly imposed upon and falsifies reality... Models in ordinary language therefore have their place in systems theory.

And again in 1972, the year of his death,

> 'Verbal' descriptions and models are not expendable. Problems must intuitively be 'seen' and recognized before they can be formalized mathematically. Otherwise, mathematical formalism may rather impede exploration of very 'real' problems." (p.163).

But he never seemed to seriously consider that there might exist any "mathematical", i.e., any rigorously exact and unambiguous, language compatible with GST -- or, for that matter with science generally -- if not the language of the science of numbers. His belief in the primacy and power of mathematics was most apparent in 1972 when he wrote,

> "...even though the problems of "systems" were ancient and known for many centuries, they remained 'philosophical' and did not become a 'science.' This was so because mathematical techniques were lacking...",

and in the same paper,

> "The goal obviously is to develop general system theory in mathematical terms (a 'logicomathematical field,' as this author wrote in the early statement ...) because mathematics is the exact language permitting deduction and confirmation (or refusal) of theory."

So, although Bertalanffy continued to hold throughout his life that there is a place in GST for other languages, the only reliable language for GST (and science) was, in his view, the language of the science of numbers.

Bertalanffy never (as far as I have been able to determine) expressed any misgivings about the power of "the language" of mathematics and its role in GST. However, he never offered any more justification for this "faith" than a simple default to the traditional regard for mathematics as "the exact language permitting rigorous deduction..." from quantitative observation. Such unsupported invocations of convenient traditional views to support efforts to establish the validity and acceptability of GST may have been the basis of GST opponent Naughton's (1979) claim that GST "seems to be benefiting from the prevailing climate of intellectual permissiveness." (Or, perhaps, after some forty years of exposure to impressive innovations in mathematical and numerical techniques and of "surviving" within traditional science, the quantity-minded ghost of atomism had returned to haunt Bertalanffy, or, at least, to reassure him of the legitimacy of mathematics as "the language" of mature scientific work which is ultimately no different, for, as I will attempt to demonstrate, once the domain of its language is defined so is the domain of a discipline.) Regardless, the GST literature neither justifies the acceptance of mathematics as "the" GST language, nor does it consider, except for resignations to practical necessity like those in Bertalanffy's statements quoted above, whether some other language might be more appropriate for GST, and, hence, for description of an effective procedure for active acquisition of reliable knowledge.

The Inadequacies of Mathematics

Can the acceptance of mathematics as the appropriate language for GST, as a language capable of supporting active acquisition of existentially relevant knowledge, be justified? Bertalanffy (1955, in 1968, p. 24) indicated the points which he apparently considered to justify the acceptance of mathematics in this role: "unambiguity, possibility of strict deduction, and verifiability by observed data". Consideration of the nature of mathematics and the intent of GST reveals an unresolvable contradiction between Bertalanffy's aspirations for GST and his presentation of GST as potentially fully expressible in the language of the science of numbers. Bertalanffy and many of his successors considered GST a new, powerful, and truly general "paradigm". Now if GST is such, then only a truly general language could be used to adequately describe it or reliably render its purported power into a methodology. Mathematics is neither a truly general nor a very powerful language. Some of the weaknesses of mathematics (particularly with respect to the descriptive needs of science, and certainly, then, GST) have been pointed out in prose by such individuals as the economist, Hayek (1967), and the mathematician, von Neumann (1966; Berlinski, 1976); in models such as that of Solomonoff (Chaitin, 1975); and even mathematically demonstrated in works like those of the mathematical logician, Goedel (Chaitin, 1975; Nagel and Newman, 1956; Wang, 1986).

Consider, as an initial demonstration of the inadequacy of mathematics as a truly general language, the entity called a sphere. An effective definition of a sphere can be provided in ordinary English: that three-dimensional geometrical surface which consists of all points equidistant from a specified point. In mathematical language the sphere is defined by the equation:

$$(x-a)^2 + (y-b)^2 + (z-c)^2 = r^2.$$

But mathematical language will not do to describe material objects -- the subjects of science and concern of any individual concerned with maintaining its existence in a material universe. What, for example, is the equation that defines a "red sphere"? There is none, because neither objects nor colors -- indeed no material objects or sensual perceptions of them -- are mathematical concepts, and consequently can not be described, ambiguously or otherwise, in the language of mathematics. Because of the importance of establishing the limitations of the language of mathematics, and because the preceding demonstration (of the futility of expecting the language of the science of numbers to serve as the language of a truly general GST) is not altogether satisfying, it is worthwhile to consider the shortcoming of mathematics in more detail.

The complaint is, then, that mathematics can only unambiguously and explicitly describe the conceptually sound constructions which can be formed from the colorless, odorless, tasteless, massless, mutually penetrable, and generally indistinguishable entities -- specifically "points" and "numbers" -- which are the building blocks of the universes of mathematics. There is a traditional counter-complaint that this is little more than a strict formalist argument which could be extended to interfere with the use of mathematics in any area of intellectual endeavor. To make this counter-complaint is to miss the point. It is not the purpose of this discussion to suggest that mathematics is not an extremely useful conceptual tool. The purpose is to consider what the fundamental limitations of mathematics might be, what types of problems might lie outside the domain of mathematics, and what artifacts might arise from application of mathematics to such mathematically inaccessible problems.

Mathematics is idealistic

Mathematics is incapable of describing GST, or of adequately expressing a protocol for a perspectivistic procedure, because it is an idealistic, atomistic, and, at least as regarded by most scientists, including Bertalanffy, an absolute language. Mathematics is idealistic, consequently its domain is restricted to universes that contain "objects" and "relations" which exist only as ideas, and which are not constrained by the "natural order" that affects "real" objects in the material universe -- those existentially relevant objects with which purposive knowers must successfully deal if they are to maintain their material existence.

Mathematics is atomistic

The atomism of mathematics follows from its idealistic foundation, because every mathematics (at least insofar as I am aware) is based on at least one "undefined" term. To be more specific, every mathematics is based, directly or indirectly, on the existence of an irreducible abstract idealized entity which is referred to as a "number" (to contest this statement would be inconsistent with acceptance of the definition of mathematics as the science of numbers). It is a necessary consequence of being undefined that the fundamental "number" entity is functionally irreducible. Hence, mathematical universes are composed of ideal mathematical "atoms" called numbers, and consequently such universes are inherently atomistic.

Mathematics is absolutistic

The absolutism of mathematics follows from its idealism and atomism. Only those things which are deductively provable are permissible in the mathematical universe and, consequently, every mathematically extent "thing" can be fully and absolutely described in terms of the fundamental "number" entities of which everything in that universe is constituted. That is, any and every entity that can "exist" in a mathematical universe can be reduced to quantitatively specifiable relationships among idealistically absolute irreducible entities. Hence, mathematics is an absolute language. Proofs of mathematical theorems are, in fact, demonstrations of the reducibility of such theorems to tautologous truths about the mathematical universe, such as an identity, 1 = 1, for example.

The perspectivistic view: Mathematics and the nature of ideal universes

Mathematics is absolute, however, only in a relative way. Consider in this regard an implication of the irreducibility (atomicity, if you will) of the "number" entity. The abstract mathematically fundamental entity which we may unambiguously denote with the symbol 0.00001 is neither part of, nor smaller than, nor larger than, that to which we may unambiguously attach the symbol 1 or 0, or for that matter 10 000. However, the symbolic designations 0, 0.00001, and 1 are unambiguous only if the entity designated by each mathematical symbol is always distinguishable from all other such entities. Yet these entities are undefined, massless, colorless, tasteless, odorless, formless, and fully interchangeable; that is, they are by definition indistinguishable. How, then, does the mathematician determine which is which?

Meaning depends on form

Finding a satisfactory answer to this question is no simple matter, because if the mathematical universe is the collection of all formless mathematically fundamental entities, then the universe is itself a formless entity. It is, therefore, impossible not only to distinguish one irreducible number entity from another, but also, to distinguish an irreducible number entity from the ideal universe which contains it. Consequently, postulating the existence of infinitely many such distinct formless entities, and the existence of an idealistic universe comprised of all such irreducible entities, is the same as postulating one idealistic, All-Formlessness -- and we encounter the difficulty presented by adopting a holistic ontology. The entire language derivable from such an idealistic, atomistically absolute mathematical universe is contained in the one truth, "Formlessness is." Beyond this there are no other truths derivable from an idealistic absolute atomistic mathematical universe ultimately composed only of formless "number" atoms. Obviously, such universes are of little interest to scientists, or, more generally, to purposive knowers concerned with actively acquirable existentially relevant knowledge; in fact, they are not even of interest to mathematicians . The language of mathematics becomes meaningful, then, only when the mathematical universe which it describes is not formless.

Internally defined form: the real world -- One can speak of only one type of formless universe, i.e., formless. One can, on the other hand, speak of two types of structured universes: those the form of which is internally defined, and those with externally defined form. Internally defined form arises as a consequence of the form(s) of the fundamental entities of a universe. I will venture to state that all material universes in which purposive knowers might exist may be considered to be of this type. That is, for all cognitively limited individuals, all realistic universes are of this type. In such universes each and every entity, fundamental or otherwise, is uniquely distinguishable in so far as its form is unique. Such universes present at least two practical difficulties for mathematics, however. Either such universes must contain no identical entities or mathematics must be ambiguous with respect to such entities. This is a relatively minor practical problem, though, compared to the second. To unambiguously designate non-identical entities in such a universe would require a statement of how each entity is different from all other entities in the (same) universe, that is, to be unambiguous every description would require a complete catalog of all of the characteristics of each and every non-identical entity in the (same) universe. Hence, for such a universe, every designation of non-identical entities would be completely unambiguous, but, at the expense of being infinitely long, presenting the ultimate difficulty in adopting an atomistic ontology. I would not presume to state that infinitely long statements are never interesting, but they are of little help to the cognitively limited purposive knower in need of reliable, existentially relevant knowledge; to scientists looking for "natural laws"; to mathematicians looking for "provable" theorems.

<u>Externally defined form: describable worlds</u> -- The form of a universe may also be externally defined. Externally defined form arises as a consequence of form outside the universe. All idealistic universes are of this type. Such universes are of particular interest to mathematics. Indeed, they are the only universes of interest to mathematics, because the form of the fundamental entities which comprise the universe need not be known and, consequently, can be left undefined. In fact, the fundamental entities of such universes must be formless because if the fundamental entities had form, then such universes would also have form and universal structure could not be externally imposed. For universes with externally defined form, entities can be unambiguously distinguished and designated on the basis of where they reside in the relevant universal structure, provided two requirements are met. The first requirement is a means of unambiguously designating relative position in the universal structure. All that is necessary to

enable such unambiguous designation is the existence of a universal reference point. In mathematics this is provided by the concept of "the origin" or "zero". The second requirement is that one and only one entity occupy each relative position in the universal structure. Again a single condition makes such unique associations possible: impenetrability of fundamental entities. In mathematics, this condition makes it possible to state definitively that $0.999999999999999999999999999999999999999999999999999999999999\ldots \neq 1$.

These two conditions together give rise to such mathematical curiosities as the numerical indeterminacy of such sensibly determinate relations as that of the circumference of a circle to its diameter, that is, "pi"; and the problems that arise when numerical methods are applied to analytical mathematical models of material systems, i.e. chaos theory (Jensen, 1987) and attempts to explain episodes of chaotic behavior in such numerical models. The number of 9's in the left argument of the preceding inequality represents the number of binary digits used by digital computers in double-precision processing of numerical information and is of interest with respect to "chaos theory". It may be that 60 digits of precision is not sufficient to prevent numerical models (generally based on classical analytical mathematical models) of even the simplest natural "systems" from generating chaotic results. Ultimately, both the power and inadequacy of mathematics may reside in its dependence on the ontological validity of (the materially unaddressable concept) infinity. With even the most powerful computers, numerical methods are subject to a considerable loss of power relative to classical, analytical mathematics because numerical methods must ultimately relinquish the opportunity to functionally invoke infinity, which ultimately is the mathematician's device for dealing with that which is materially apparent but quantitatively unverifiable.

Further, imposition of these designation-enabling conditions does not alleviate all the problems inherent in the exploration of ideal universes. For example, there are problems with respect to the distinction between that which is (ontology) and that which can be known (epistemology) or, perhaps more correctly, that which can be designated. The form of an ideal universe is only defined relative to form outside the universe. In order to make formless entities distinguishable within externally defined structure -- that is, to enable unambiguous designation -- it is necessary to impose certain conditions, such as "zero" and impenetrability. As soon as such conditions are imposed the universe behaves as though its fundamental entities, indeed every entity in it, had form. And that behavior reflects the aspects of the external form that were implicit in the imposed designation-enabling conditions. Hence, ideal universes can be formed and conditionalized to reproduce the behavior of any, but never all, forms present in the external defining form. Never all, because, if this were possible, then the extrinsic-form-defined ideal universe would no longer be distinguishable from the extrinsic form relative to which it was defined, implying complete and absolute knowledge of the defining form on the part of the selector of the designation-enabling conditions. This implication violates the initial empirically based premise that the selector of the form-defining conditions has only limited cognitive capabilities.

The limits of mathematics and conflicts in the Bertalanffian presentation of GST

In light of these considerations it seems reasonable to state that mathematics is not a truly general (absolute) or unambiguous language. Its generality and freedom from ambiguity are relative to the extrinsic form of the ideal universe from which mathematics was derived. Consequently, mathematics only speaks unambiguously about entities, fundamental or complex, within its ideal universe, and those entities in the extrinsic form of its universe to the extent that the forms of the extrinsic entities are implicit in the designation-enabling conditions (imposed on the ideal universe of mathematics). So, I suggest that mathematics is inherently atomistic and reductionistic; and, contrary to a tradition of modern science and Bertalanffy's view, I suggest that even in its ultimate form, mathematics will be neither absolute nor fully unambiguous. Hence, mathematics is not an appropriate language for the expression of GST. I propose, further, that no finite, externally unreferenced language can speak unambiguously about anything. And, as a corollary, that no finite language that can speak about everything can speak unambiguously about anything.

Now these conclusions bring the limits of GST under Bertalanffy, not to mention the limits of traditional science, into view. Earlier in this discussion I raised three questions about GST. The first question regarding the purpose of GST as perceived by Bertalanffy has already been answered: GST "is a general science of 'wholeness'" and its subject is the formulation of principles that are valid for wholes (systems) not understandable by investigation of their isolated parts. The conclusions just reached regarding the effective domain of languages allow consideration of the two remaining questions. Why did

Bertalanffy's efforts to present GST appear redundant and ineffective? And, where was the inadequacy or inappropriateness of his means?

Unreasonable expectations

Bertalanffy's efforts appeared redundant and ineffective because apparently he did not himself appreciate the necessity of establishing an adequate philosophical foundation for GST. He bore a latent anthropocentric intellectual loyalty to an absolutist, atomistic, ontologically based science while trying to stimulate within that science development of a "new" epistemological foundation that was incompatible with his ontological loyalty. Consequently he continually jumped the fence between idealism and realism, which allowed him to hold unreasonable and contradicting expectations of GST. For example, if anything from an atom to a whale to a galaxy to the material universe can be a system, and if GST is "a theory...of universal principles" applicable to "generalized systems or their respective subclasses, irrespective of their particular kind, the nature of their component elements, and the relations or 'forces' between them" (1968, p. 32), then his expectation that GST would ultimately be explicitly expressible in the "exact language" of mathematics is unreasonable. The inadequacy of his means, then, may be seen to lie in his dedication, expressed and implied, to the science of numbers and quantities as the ideal science.

Ontological issues avoided

He avoided discussion of the ontological issues begged by his discussions of GST and the confusions and contradictions those discussions contained. In his discussion of "system ontology", for example, he suggested that "real systems" exist independently of the observer who may perceive or infer them from observation, but that "interactions of the component elements" in a system "are never directly seen or perceived; they are conceptual constructs." When, then, are conceptual constructs not to be considered as inferred from observation? And, if inferred from observation, then when are they just "conceptual constructs" and not to be understood to exist independent of the observer?

Ideality and reality confused

His willingness to jump from ideal to real properties reveals a more profound difficulty within Bertalanffy's GST. Unsolved this problem leaves GST without a philosophical foundation, disables perspectivism as a reliable conceptual tool for science, and, consequently, invalidates the perspectivistic approach as a theoretical description of the general solution to the problem faced by cognitively limited knowers in an immensely variable universe. Bertalanffy proposed that to explain the behavior of a whole it is necessary to know the quantities, qualities, and interactions of the component parts of the whole. Now, if the interactions among the component parts are conceptual constructs, then how does one know if one is investigating an "ideal" whole which is only a conceptual construct (probably not even of the investigator's own conceptual construction) or a "real" whole? How does one avoid the humanistic risks which so rightfully concern the opponents of GST and the "systems movement"? Bertalanffy answered by appealing to the "humanistic concern" of GST as he understood it. He apparently wished to suggest that science under GST would have more effective recourse to "humanistic concern" than traditional science when he stated that (Bertalanffy, 1972, p. 167),

> If reality is a hierarchy of organized wholes, the image of man will be different from what it is in a world of physical particles

> governed by chance events as the ultimate and only "true" reality. Rather, the world of symbols, values,...is something very 'real'.

Though his first sentence here is clearly a reasonable statement, it is tautological, and provides no evidence that science under GST would have more effective recourse to "humanistic concern" than traditional science, and ignores the philosophically difficult issue of whether any science should ever have recourse to "humanistic concerns" at all. The second sentence presents the ideal/real ambiguity again. Are symbols and values "real" or are they "conceptual constructs"?

An implicit principle contradicted

Finally, Bertalanffy violated the conclusions implicit in the perspectivism he himself proposed. He apparently gave quantitatively describable observations higher epistemological priority than qualitatively describable observations. This is a necessary consequence of his faith in atomistic mathematics as an absolute, universal language. Consider in this regard his expectation that GST would lead to quantification of

> Concepts like those of organization, wholeness, directiveness, teleology, and differentiation ... General system theory is, in principle, capable of giving exact definitions to such concepts and, in suitable cases, of putting them to quantitative analysis.

Such a belief implies a fundamental belief in the validity of atomism, hence, belief in the universal validity of quantitative descriptions; and, despite his sincere desire to the contrary, prohibits science based on GST as presented by Bertalanffy from being anything other than traditional science in more emotionally appealing vestments. In fact, the case can be made that the limits of science under GST as presented by Bertalanffy are either narrower than those of traditional science or are broadened at the risk of reduced reliability of conclusions, something traditional science is already capable of.

Ambiguities unresolved

GST under Bertalanffy ultimately succumbs to the same weakness that plagues science under atomism: dependence on an ideal, atomistic, absolutist language incapable of unambiguous description of material reality. Bertalanffy's presentation of GST failed because it bore too many ambiguities. I treated some of the more benign ambiguities earlier in this presentation, but I have found no treatment for the real/ideal ambiguity apparent in Bertalanffy's faith in the language of mathematics as the one truly exact and unambiguous language. There is an interesting and important lesson here. Bertalanffy's undoubting faith in mathematics as the only language capable of exact and unambiguous expression carried two implications which imposed a bias on his "perspective". First, since he saw mathematics as the one truly exact and unambiguous language, it would have appeared futile to him to search for other more powerful or less ambiguous languages. Second, since if he saw any language other than mathematics as inherently ambiguous, then he needed not be too concerned about avoiding or clarifying ambiguities in his use of such languages, particularly verbal languages. Also, since he saw mathematical truths as universal, and since only quantitative observations can be fully rendered into mathematical language, he must have regarded universal material truths as obtainable through quantitative observation.

Language and Science

The limits

That the limits of science are the limits of the language of science, and that the limits of traditional science are reflected in the constraints on Bertalanffy's views of GST, is suggested by the following, from articles on science and the scientific method by Bridgman and Holton (1987):

> A prerequisite to nearly every science is a suitable method of description of its subject matter. The language of such description must be capable of reproducing or recalling the subject matter with precision and uniqueness. If the description is of an object, there should be only one corresponding object, which it should be possible to reproduce or reconstruct from the description; or, given an object, it must be possible to check whether it does or does not satisfy the corresponding description...Fundamentally, measurement amounts to description by the use of numbers...The numbers obtained...may be subjected to mathematical analysis, and mathematical regularities revealing the operation of various laws of nature often can be discovered and made the basis of theoretical understanding. It is regarded as an ideal of science that it be capable of mathematical analysis, and the more highly developed the science, the more susceptible it is of such analysis...

Bertalanffy's contributions

Because Bertalanffy did not relinquish his traditional scientist's faith in mathematics, he could not do more than present intuitive impressions of the essential tenets of GST. Nevertheless his efforts did present them and that is Bertalanffy's contribution. His discussions provided insights on four principle points. (1) It is unreasonable to consider the limit of (human, scientific) knowledge to be defined by the "infinite manifoldness of ultimate reality", and as a consequence, that the pursuit of knowledge should not be based on a presumption of the nature of ultimate reality. (2) Realistic propriety requires the limit of knowledge and the method of pursuit of knowledge be viewed as defined by the capabilities of the pursuer of knowledge. (3) Only a realistic, qualitative, relativistic, infinitely reference-transferrable and extensible method, which he envisioned as "perspectivism", would provide finite "knowers" with a means to obtain existentially reliable knowledge. (4) Empirically distinguishable "wholes" and "parts" can serve as an epistemologically sound basis for a realistic perspectivism (Bertalanffy preferred the word "system" to "whole", but he defined the term "system" too restrictively.) Bertalanffy, then, provided insights into the "why" and "what" of GST, but it is necessary to look beyond his efforts for the "how".

Bertalanffy's error

Bertalanffy was not totally without insight into the nature of the "how" of GST. As mentioned earlier he was of the opinion that GST would replace logical "theory of categories", but did not seem to believe that a formal logic capable of replacing or superseding "theory of categories" had yet been developed. Bertalanffy's error in this regard suggests the possibility that he may have overlooked logic in general as a discipline where at least the rudiments of a formal GST might be found. The lack of mention of specific logical theories (other than by indirect reference, like that to "theory of categories") in his writings support this idea. Further, the possibility that a single discipline might already have a formal structure for GST would have conflicted with Bertalanffy's expectation that formal GST would arise from work in different fields.

There is also, however, the possibility that Bertalanffy's error was the consequence of his holding what is -- if the present author's experience with other scientists is correct -- a common view among traditional scientists regarding the functional relationship between science and logic. Logic, the study of forms of reasoning without regard for content, is generally regarded by scientists as "all form and no substance" (recall Berlinski's complaint about GST), something on which they can not reasonably afford to spend time. Many logicians hold a complimentary view that was well summarized by Lejewski (1986), "Traditionally, logicians have distinguished between deductive logic, whose principles are used in drawing new propositions out of premises in which they lie latent, and inductive logic, which ventures conclusions from particular facts that appear to serve as evidence for them. But this division is obsolete, because the problems earlier subsumed under induction are now apportioned to the methodology of the natural sciences." According to the logicians, then, inductive logic is the tool and domain of natural scientists. But among interested scientists and philosophers of science the conclusion has been drawn that there is no such thing as inductive logic (Medawar, 1969; Ackermann, 1976; also, recall Naughton's statement regarding GST). From this view the success of science is seen as depending almost solely on the effective use of an essentially deductive procedure known as "the scientific method", of which there are a variety of (incomplete) descriptions such as the hypothetico-deductive method. The invocation of the hypothetico-deductive or other such "scientific" methods is rather unsatisfying, however, for these methods in effect avoid the question of how one might formulate effective hypotheses -- note that in science hypotheses may be "effective" without being "correct", while for an existentially challenged purposive knower correctness and effectiveness often are not different -- about previously unknown phenomena with which science or scientists have no prior experience, or, and perhaps presenting even more difficulty, about previously known but misunderstood phenomena. Any science that would depend on idealistic deductive logic for innovation is ultimately no different from science that would depend on mathematics as a universally valid language. Science based solely on deductive logic, as, for example, hypothetico-deductive science, ultimately must fall into a routine of deductively testing relatively unexciting variations on dogmatic themes, and awaiting serendipitous events, those rare "lucky" selections of experimental conditions which lead to unexpected but interpretable observations and satisfying new explanations of observable natural phenomena. Consequently, under deductivistic approaches, science falls back onto Bertalanffy's belief that no formal logic compatible with the inductive needs of science or descriptive of effective active acquisition of empirical knowledge (GST) has emerged. More explicitly, that there is no such thing as a reliable inductive logic. That is, most logicians are no longer concerned with induction, and most scientists function disinterested in logic or denying that any logic could adequately describe effective inductive process.

Further, it seems not to be generally appreciated that the disinterest of scientists in inductive logic is likely due to their acceptance of the fundamental tenet of atomisitic-reductionistic science that there are absolute material "truths" which can be found out "scientifically". To be logically consistent, anyone who functions, knowingly or unknowingly, under an absolute materialistic, atomistic, and mechanistic ontology must reject the possibility of an inductive logic. This is so because, if the material universe were demonstrably mechanistic (consistent in an Aristotelian deductive logical sense) and ultimately composed of simple, indivisible, indestructible "atoms" (atomism); and if physical matter were the only true reality and everything in the material universe could be explained in terms of physical laws (materialism); then it would necessarily follow that (1) as soon as one universally valid law were known, all valid laws would be mechanically deducible, but (2) as long as no universally valid (absolute) law were known, nothing reliable would be known, and consequently (3) induction, by definition concerned with drawing effective conclusions from reliable but incomplete (relative) knowledge, can not be undertaken unless something is reliably known, but under the ontology in question as soon as something is reliably known; everything is deducible and induction is pointless. That is, scientists who function, knowingly or unknowingly, under an absolute, materialistic, atomistic ontology implicitly presume either that they are endowed with or will eventually develop infinite cognitive capacity (presumably as a consequence of the intellectual exercises necessary to their investigative activities), or achieve divine insight (presumably the result of having successfully investigated every materially permissible phenomenon), either of which would eliminate any need for inductive reasoning.

The Logic of Wholes and Parts

The obscurity of Lesniewski's work

Despite logicians' acceptance of the "obsolescence" of inductive logic (this attitude seems to be changing as a consequence of interest in "artificial intelligence", see Waldrop, 1987) and the deference of scientists to deductive methodologies, it still seems a little difficult to understand why no one, Bertalanffy or any other, interested in the GST debate (at least according to the literature I have seen) seems to have picked up on Lesniewski's mereology, a formalized "general theory of the relationship between part and whole" (Bird, 1986). Perhaps it is simply a question of the obscurity of Lesniewski's work. Lesniewski originally presented his logic in 1916 in Polish, a language not widely spoken in the world academic community. He then worked intensively for years formalizing his logic and its own mathematical language, but refrained from publication because his work was not in as perfect a form as he considered desirable. In 1927 he began presentation of a series of papers that illustrated the main lines of his theories of logic and mathematics -- even then, he apparently only undertook publication because the works of his colleagues, which were dependent on his results, were awaiting publication. Those publications brought worldwide recognition to the Warsaw school, but before it reached its height, Lesniewski died suddenly, just prior to World War II, most of his findings still unpublished. All his manuscripts were destroyed during that conflict. After the War, many of the results of his work were made known through the work of his students, but nevertheless seem to remain an obscurity even among logicians, let alone scientists.

Relevance to GST and science

The misfortune for GST, and for science, that is represented by the loss of Lesniewski's manuscripts and the obscurity of what remains of his work is apparent in Luschei's (1962) The Logical Systems of Lesniewski (which I will quote extensively in the following pages as an, perhaps the, authoritative English language reference on Lesniewski's logic), even though that author makes no mention of GST. Lesniewski acquired from his teacher, Twardowski, an "insistence on rigor and clarity...based on precise definition and analysis". He was concerned with the paradoxes which have afflicted deductive logics since the time of Aristotle, and felt that such were the result of ambiguities in the formal language or metalanguage implied by those logics. Hence, he did not trust formal logical languages during his early years, and empathized, apparently throughout his life, with the attitude of most natural scientists toward formal deductive logics. This empathy was apparent, for example, in his emphasis "that equivocal use of terms...makes it unclear whether theses are in or about the system in question, and remarked that such ambiguities discourage those who do not derive the same delight" from the manipulation of formal patterns "as 'devotees of meaningless mathematics'...but want to know what they are doing and why, and what the formations and transformations mean" (Luschei, 1962). "Lesniewski was openly critical of pure formalism that would consider logic and mathematics as nothing more than a game of symbols... he maintained that a theory ultimately must be judged for its accord with reality" (Bird, 1986). And, according to Lejewski (1967), "the conceptual apparatus of Lesniewski's theories was intended by its originator to be used in philosophical or scientific practice at any level of lower generality."

Foundations: The use and limits of language

The value of Lesniewski's work in the present consideration of the active acquisition of purposive empirical knowledge arises from his appreciation of several points. He appreciated linguistic expressions as generalizable communicable conceptual descriptions of cognitively accessible phenomena. Anticipating the work of Goedel and others as well as its implications, he appreciated that the validity of linguistically expressible conceptual descriptions is relative and limited to the level of the "object" language, but can only be meaningfully discussed at the metalanguage level. Like Aristotle and Russell, Lesniewski recognized that in "natural" languages, including prior formal logical languages, relative consistency was achieved through the use of "systematic ambiguities resting on systematic analogies". Such usage increases the "power" of language by permitting expressions of the same form to carry different meanings, the intended meaning clarified by the context in which the expression occurs. And the consequent that

> To ensure consistent determinacy of meaning in an adequate logical reconstruction [one which will provide a formal language the expressive power of which approaches that of natural language], it is essential to treat systematically ambiguous logical...schemes as representing unbounded hierarchies of different but systematically analogous and true theses ranged in tiers above the basic theses of lowest level (Luschei, 1962, p. 86).

Problem: Inconsistencies in theoretical foundations

Like Bertalanffy who was troubled by inadequate development of general theoretical foundations in the natural sciences, Lesniewski was troubled by inadequacies in the predominate theories of his field, philosophy of mathematics and symbolic logic, or as it was more widely known in his day, mathematical logic. Lesniewski, however, unlike Bertalanffy and probably most modern scientists, was more directly appreciative of the profound, fundamental import of languages to the natural sciences. In fact, he held views which differed from those generally accepted by the logicians and mathematicians of his day, views which many scientists now share. For example, he rejected the view that logic or mathematics is "nothing more than a game of symbols" the significance of which seems to arise out of mathematicians' or logicians' chance encounters with materially interpretable symbolic expressions. He held that realism, intuition, and common sense are of primary importance to logic, mathematics, and the natural sciences, and would almost certainly have rejected the currently prevalent belief that scientists should be distrusting, perhaps even "neglectful, if not contemptuous, of man's naive and basic intuitions of the way things are".

Diagnosis: All linguistically expressible truth is relative

It is interesting in this regard, that the theories and successes of modern physics, Einstein's Theory of Relativity in particular, are invoked by modern scientists as implying a general validity for an idealistic, anti-common-sense view of reality. Such invocation is due in large part to misinterpretation and over-extension of Einstein's results (Pais, 1988; Popper, 1980). Further, a presumption of the universality of the mathematical language is implicit in the Theory of Relativity. Overly enthusiastic reception of the theories and successes of modern physics, the inherent dependence of science on language, and an unquestioning "faith" in the universality of mathematics may prevent most scientists, as they probably prevented Bertalanffy with respect to his own GST ideas, from appreciating the more general implications of Einstein's work: (1) that experimental verifiability (reproducibility) requires scientific "truths" to be linguistically communicable, and hence that scientific "truths" are contextual and language dependent; (2) that the "truthfulness" of statements in any language is relative; and (3) that only a portion of reality can ever be accurately described through the use of any semantically closed symbolic language. The relativity of any linguistically expressible "truth" did not escape Lesniewski whose "study of semantic antinomies convinced [him] that in any 'universal' language" closed so that it contains its own rules for interpretation of symbolic expressions "the laws of classical logic cannot consistently hold" (Luschei, 1962, p. 34).

Cure: Exploit the relations between whole and part

Lesniewski took a direct approach to dealing with the problems which arise out of the relativity of linguistically expressible "truths", the result being his theories which "for him...consisted of interesting though extremely general propositions true of reality as we know it from experience." Interpreting this into the terminology of the present discussion Lesniewski developed what, in the aggregate, could represent a theoretical description of the process of active acquisition of purposive knowledge. A detailed presentation of Lesniewski's work is beyond the scope or needs of this exploration of GST. It is sufficient here to state that he developed, from his intuitive insight into the use and limits of formal and informal language, and the relationships among whole and part, a logic which he formalized

> ...completely, combinatorially on a finite basis, and in extensional terms. It is...distinguished by its 'constructively nominalist' and 'contextualist' character; its basic grammar of semantic categories; its rigor, generality, and power of expression; its demonstrable relative consistency; its universal validity; and its logical purity, economy, and elegance. It consists of three axiomatic deductive systems in hierarchic order: <u>protothetic</u>, <u>ontology</u>, and <u>mereology</u>...Protothetic and ontology together form a unified system of logic comparable in scope and power to <u>Principia</u> <u>Mathematica</u> as a foundation for classical mathematics and for any other axiomatic theory, such as mereology, in a deductive hierarchy...Mereology is an extremely general extralogical theory based on the two logical systems. Together with them, it provides a mathematical [in the sense of formal, "rigorously exact: precise"] basis for spatiotemporal theories of topology, for geometries such as Tarski's axiomatic geometry of solids...and for scientific description of reality...(Luschei, 1962, p. 28)

Lesniewski's accomplishments

Lesniewski was a philosopher, professor of the philosophy of mathematics at the University of Warsaw. His success in the field of logic arose out of fascination with the contradictions and paradoxes that afflict mathematics and symbolic logics. He developed the fundamental principles of his logic over a period of 11 years beginning in 1911. During that period he worked inductively and intuitively. By 1913 he had logically refuted "the conception of 'general objects'" (Luschei, 1962, p. 27). His linguistic insights and study of various logical traditions enabled him to diagnose the cause of specific paradoxes as failure to distinguish between collective (relative to the "whole") and distributive (relative to the "parts" which make up the "whole") interpretations of linguistic expressions about "classes". Having diagnosed the affliction of classical logics, and equipped with his material insightfulness and logical expertise, he began in 1914 development of mereology, his theory of the relations between wholes and parts, the logic embodying the interpretation of collective class expressions. He recognized that mereology implied certain "logically prior theories". He apprehended those theories and by 1922 had developed them as: ontology, his "general theory of what there is", based on the one undefined term "is" and embodying the distributive interpretation of class expressions; and, protothetic, "the most comprehensive theory yet developed of the relations between propositions", based on the functor of equivalence as the only undefined term; completed his grammar of semantic categories (theory of logical categories) about the same time; and, by 1931, had developed his comprehensive and rigorous directives for definition, substitution, and extensibility.

His philosophy: a definitive foundation for his logical theories

Lesniewski's logics may be considered a rigorous extension of his philosophy the character of which is indicated by his insistence that

> feeling for reality...ought to be preserved even in the most abstract studies. Logic...is concerned with the real world just as truly as zoology, though with its more abstract and general features....The sense of reality is vital in logic, and whoever juggles with it...is doing a disservice to thought." (Lesniewski quoted in Luschei, 1962, p. 51).

This statement indicates the depth of Lesniewski's realistic beliefs, in his belief in the sensible logical (but not necessarily mechanistic) consistency of the material universe. It also clarifies that the concern of logic under Lesniewski is the same as the concern of GST under Bertalanffy. Lesniewski apparently regarded material reality as the "primary standard" of logical consistency. And, at least partially as a consequence of his realistic orientation, he decided that if linguistic expressions are to accurately represent aspects of reality, then logic should refer "strictly to expressions as concrete spatio-temporal objects, not to forms of expression in abstraction from their instances in context" (Luschei, 1962, p. 4).

Lesniewski was not only an affirmed realist, but also an avowed intuitionist, believing the natural logical consistency of the material universe to be more directly accessible through disciplined, realistic intuition than through systematic exploration of idealized descriptions:

> ...one has only to stop using words for a moment to recognize that reality does not arrive neatly tailored and dressed in words or verbal categories (p. 14)...Lesniewski regarded the consistency or inconsistency of [other logical] systems 'quite irrelevant' to the 'reality-directed intellectual torment' of intuitive compulsion to believe presuppositions "true" and inferences "correct" that taken together lead to contradiction, [such contradictions] thus representing antimonies which can be resolved only by intuitively undermining their sources. For 'Mathematics [meaning symbolic logic as well as the science of numbers] without intuition cannot effectively remedy the maladies of intuition. (Luschei, 1962, p.78)(see also von Neumann, 1966).

Lesniewski's work and writings indicate he accepted intuition as cognitively primal. Apparently his studies of various logical traditions and his own linguistic insights enabled him to recognize that direct, immediate cognition of the metalinguistically describable whole is materially and logically prerequisite to materially adequate linguistic, hence scientific, description of the distributive qualities of the parts of a whole. Luschei (1962, p. 34-35) relates in this regard that

> Lesniewski was the first (at least in modern times, to Tarski's knowledge) to attain, express clearly, and appreciate the consequence of certain fundamental insights, anticipating Russell, Ramsey, and Goedel. Recognizing that semantic concepts are relative to the "object" language or theory discussed, which may not coincide with though it may be part of the "metalanguage" in which it is discussed, Lesniewski stressed the distinction between these correlatives...He concluded that in a language not constructively stratified and relativized but supposed to be universal, ideally completed, and semantically closed to incorporate all its own semantics, the laws of classical logic cannot consistently hold.

Reasonable expectations

Restating, Lesniewski accepted that if a symbolic logic or a language is to be truly general, then it can not be concerned with ultimate reality, which is the domain of direct cognition, perception, intuition. As a consequence of his insights into the limits of logic and language, and hence, science -- insights others later formally demonstrated to be valid -- Lesniewski accepted that all that can be expected of even a truly general language is expression of reliable, logically consistent, and testable descriptions of certain aspects of reality (because formulation of valid expressions about entities extant at the "object" language level requires cognitive apprehension at the metalanguage level of the collective whole which is comprised of those objects) (Luschei, 1962, p. 105). Under these premises he developed a logic which comprises a universally valid, ontologically ambivalent set of directives which enables the construction of qualitative, relativistic, infinitely extensible languages in which "individual aspects of reality can be described without 'referring to abstract entities' at all" (ibid., p. 80). The value of such a grammar to science is obvious; but, and more importantly here, it provides (in my view) the best (and apparently only) theoretical description ever developed of the general solution to the problem faced by all cognitively limited purposive knowers. I will cite specific points derived by Lesniewski (or others using his logics) in the following presentation of the general principles of the perspectivistic approach.

The General Principles of the Perspectivistic Approach

The first general principle: _Materially reliable generalizations must be discovered._ The wording of the first principle is intentionally ambiguous, allowing two interpretations.

The first interpretation: In order to exist, cognitively limited purposive knowers must reduce the amount of specific knowledge they are materially required to handle. This may be accomplished by classifying individual entities on the basis of equivalence with respect to material criteria which are existentially relevant, therefore cognitively accessible, to the classifying knower. Recall that equivalence and "is" were the only undefined terms necessary for Lesniewski to develop a relativistically consistent, infinitely extensible "general theory of what there is" (Lejewski, 1967). Reliably effective perspectivistic generalization (or, valid Lesniewskian generalization) involves qualitative classification of entities according to their existential relevance to the classifying knower; not according to their relevance to some ideal, hence materially unreliable, entity. For, although "equating logically different objects" is "one of the most powerful and efficient methods in mathematics" (read the science of numbers), relying on materially or logically unjustified generalizations would likely prove fatal for most purposive knowers. It follows then that: all materially reliable generalizations (including those which comprise the passively acquired knowledge) must be inducible from specific experiential knowledge. Properly developed perspectivistic generalizations have full epistemological, hence relativistic material, validity; but only partial ontological validity.

The second interpretation: The first principle stipulates <u>the primacy of direct cognition</u>. Direct cognition is otherwise variously described by such terms as perception, intuition, insight, inductive reasoning. Direct cognitive access always precedes and provides the reference necessary to effective description, and, consequently, to the communicability and applicability of traditional scientific methodology in the search for new knowledge. Previously unrecognized materially reliable generalizations are not encountered (at least not with existentially adequate speed) through directed searches of, or intentionally "designed" and "constructed" from parts selected out of the warehouse of, previously acquired specific knowledge. Each previously unknown materially reliable generality must initially be grasped as a whole consistent with, and part of, or inclusive of the whole that is the totality of knowledge previously acquired by the classifying knower. Initially only one such entity will have been encountered. Hence, the cognitively "new" whole is unique and therefore only qualitatively describable, since multiplicity, which is a necessary condition for quantitative description, is by definition not a quality of any unique thing. Once a clear perception of the collective material whole represented by the generalization has been achieved, that is, once an adequate description of this "whole" is available; its parts become unambiguously distinguishable and describable, even though, as

Lesniewski pointed out, they may not be discrete and consequently, again, only qualitatively describable. (Recall the previous discussion of distinguishability of entities in ideal universes with externally defined structure.) Such qualitative generalizations represent primitive entities and may be considered to have the desirable qualities, in the Lesniewskian sense, of logical simplicity and purity; and were well described in one of the poetic essays of Pope (1711),

> Those rules of old discover'd, not devis'd
> Are Nature still, but Nature Methodized;
> Nature, like Monarchy, is but restrain'd
> By the same laws which first herself ordain'd.[10]

The second general principle: <u>Specific experiential knowledge is obtained only through specific experiences with material entities that are sensorally accessible as individuals</u>; that is, which are functional material "wholes" with respect to the existential needs, or experiential "range" of the knower. Consider again my hypothetical fish, this time encountering, say, a worm. The fish perceives a whole worm, not a collection of worm parts. The fish smells to the full extent of its olfactory capabilities the complex of chemicals released by the whole worm, not just those of the worm's intestinal contents, or blood, or epithelial cells, or.... It sees the physical form and movements of a

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[10] The reader might also consider Luschei's (1962, p. 103) comment that without meaningful generalizations, "human knowledge would be incommunicable and non-cumulative, amounting at each moment, for each isolated individual, to little more than the momentary content of his conscious awareness, since even individual knowledge becomes generalized in being communicated. And generalizations may be at least as important as their individual instances. It may for example be as important to know that there is danger (or none) in the field one is about to explore as to know individual names, kinds, numbers, locations, and dispositions of bulls, mines, or what-not. The mere existence of nuclear bombs has proved as disturbing as their unknown total....

whole worm, not of an assembly of worm tissues, organs, cells, cytoplasm, genetic material,.... Just as a human apprehends an automobile as an automobile, not as an assembly of sub-assemblies of sub-sub-assemblies of...car parts which are assemblies of sub-assemblies of...various chemical assemblies which are assemblies of.... For as Lesniewski discerned from usage in natural language, the individual entities which are parts of a cognitive whole are not necessarily discrete. That is, the same valid generalization may be inducible from specific experiential knowledge of materially different individual entities.

Speaking in more general terms, which entities are sensorally accessible to an individual knower is determined by its sensory capabilities. The individual's sensory capabilities are determined by its passively acquired knowledge and limit the knower's sensory access to only those entities which display material qualities ancestrally established as characteristic of existentially relevant entities; that is, entities that did (do) something that was (is) materially relevant to the survival of this individual whole.

An individual knower's sensory limits may be temporarily extended -- when an astronomer uses a telescope, for example -- but this does not alter the sensory capabilities by which this individual (as a collective whole) might be distinguished from other (distributive) members of its collective class (say, its taxonomic biological species). Similarly an individual's cognitive effectiveness might be temporarily enhanced, but not its fundamental cognitive capabilities. Regardless, the most general statement that can be empirically justified is that all materially extant knowers exist equipped with quantitatively limited cognitive capabilities. One interpretation of the empirical evidence -- indeed, in my opinion, the only interpretation based on reasoning that is not viciously circular -- is that they accomplish this by compensating quantitative inadequacies through exploitation of the extensibility of qualitative cognition.

Built, as Lesniewski's logics, upon a foundation of verifiable material equivalence and recognizability of patterns among the parts of cognitively accessible wholes; qualitative cognition enables classification of incomprehensibly large numbers of entities into a comprehensible number of related, existentially relevant wholes. The recognized infinitely extensible validity and descriptive power of Lesniewski's logics provide formal symbolic ("mathematical") justification for the proposed biological importance of qualitative cognition based on pattern recognition and material equivalence, the most fundamental and general pattern being that of whole and part. Existentially effective qualitative cognition is ultimately the ability to decide whether, with respect to the knower, a collection of cognitively accessible entities is an existentially meaningful collective class (metawhole) or simply a distributive collection of discrete entities with no collective material function existentially relevant to the knower -- even though some or all of the individual entities may have existentially relevant material functions.

The second general principle further implies the requirement that no entity is part of a collective whole (a "system" with emergent properties, a Bertalanffian "system") unless the entity has at least one cognitively accessible, sensorally verifiable, material function with respect to, that is, material effect upon, at least one other entity which is part of the collective whole. This in turn, implies that Bertalanffian relationships, those "conceptual constructs...never seen or perceived...", among the parts of a whole are not materially reliable, and dependence upon such ideal connectors is too risky to provide an adequate basis for active acquisition of purposive knowledge.

This material function requirement addresses the limits of quantitative and qualitative cognitive accessibility, and would cause considerable difficulty were it not that the perspectivistic approach is based on recognition of those limits and stipulates that meaningful, linguistically describable observations can only be made from the metalevel of the subject under observation. If the entities which are parts of a whole function distributively, and those distributive functions are to be observed; that is, if the individual parts of the whole are functionally discrete; then direct, materially reliable, even quantitative, observation can be carried out from the level of the whole, one level above the level at which the parts are functional collective wholes in their own right. On the other hand, when the entities which are parts of a whole function collectively, the function of the individual part entities simply can not be directly, meaningfully observed; because each distributive entity's collective function is uniquely contextually defined and not observable outside the collective whole in which it is part. The member entities in a collective whole are functionally non-discrete, hence not quantitatively describable. Cognitive accessibility and sensory verifiability of the material function of an entity in a collective whole can only be established qualitatively by observing that lack or malfunction of the entity alters the collective function of the whole. That is, that an entity is a (functional) part of a collective whole can only be established by observation from the metalevel of the whole, two levels above the level at which the parts are collective wholes in their own right. Observation at this (2 X higher) level can materially establish only the qualitative knowledge that an entity is (or is not) a functional part of the collective whole.

It may be observed at this point that wholes comprised of distributively functional, hence, discrete parts are the most satisfying for traditional scientists -- materially reliable generalizations representing such wholes can be expressed in the language of the science of numbers, i.e., in quantitative terms. However, such wholes are of limited existential relevance to the knower with limited cognitive capacity; if for no other reason, then because the number of entities in each such whole can, for no immediately apparent reason, quickly expand to exceed the quantitative cognitive limits of the knower, becoming cognitively inaccessible, and leaving the knower no means by which to decide when and how to respond to such wholes, or even if such wholes are the ones which should be responded to.

Collective wholes are more existentially important to purposive knowers of limited cognitive capacity. As already discussed, the greater importance of such materially functional collections of non-discrete parts arises from the fact that they can provide the knower with an existentially adequate means of cognitively simplifying the material universe in which it exists, enabling it to select effective responses to a quantitatively incomprehensible variety of environmental hazards. Also, as has already been mentioned, collective wholes and collective functions can only be qualitatively described -- they either materially occur or they do not. It is of interest to add here that even a knower with a relatively very limited cognitive capacity can build, retain, and use complex cognitively accessible hierarchies of collective wholes rapidly. This is so because, while there is no assurance of functional connection among quantitatively describable assemblies of quantitatively describable distributive wholes, in perspectivistically developed hierarchies of relativistically materially reliable collective wholes, no whole is functionally unrelated to any and every other whole within a hierarchy. Such hierarchies require analog representation; they can not be validly represented by quantitative or digital symbolic representation. Hence, those generalizations which are included in passively acquired knowledge are present in a wholistic analog expression, the passively acquired knowledge. The individual's initial store of existentially useful knowledge, and the individual's existential effectiveness, may be increased by the acquisition of relativistically valid, materially reliable generalizations. The "emotion" associated with direct cognition of (sudden insight into) a valid generalization may be the result of the brain encountering an organization of representations, a pattern, that is compatible with the whole that is the collective totality of the knower's acquired knowledge, active and passive. This view of valid generalizations as analog representations that are wholistically acquirable only through direct cognitive access; well described in Gertrude Stein's statement about modern art

> It looks strange and it looks strange and it looks very strange and then suddenly it doesn't look strange at all and you can't understand what made it look strange in the first place. (quoted by Luscher in Medawar & Shelley, 1980)

was shared by von Neumann (1966), but is contrary to the expressed view of Bertalanffy (1968), and the cyberneticist Ashby (1966).

Finally, the second general principle serves not only to describe effective cognitive process for knowers with limited cognitive capacity, but also serves a clear theoretical purpose. It prevents deductions based on epistemologically sound, hence relativistically materially reliable, generalizations from degrading into excessive or divergent idealistic logical regresses which, although formally correct in the sense of non-Lesniewskian logics, lead to the inference of specific entities without adequate or appropriate epistemological justification and no ontological (material) reliability. Further, as already mentioned, the first and second principles in conjunction imply that if any valid, materially reliable generalization can be expressed, then cognitive access to the next higher level has already been achieved. This implication leads to the third general principle.

The third general principle: If there is more than one cognitively accessible class of individual material entities in the knower's experiential universe, then there are at least two reliable generalizations. And, if there are at least two materially reliable generalizations (about specific entities in the knower's universe), then there is at least one materially reliable generalization about the generalizations, that is, there is at least one valid generalization of a "higher level". But, if all materially reliable generalizations must be inducible from specific experiential knowledge; and if specific experiential knowledge is obtained only through specific experiences with entities which are cognitively accessible as individual "wholes";

then a higher level generalization implies that there are specific entities from which the higher level generalization may be induced. That is, each materially reliable generalization at any level implies one cognitively accessible material whole. The third principle is the perspectivistic equivalent of Lesniewski's refutation of ideal general objects, and implies, as does his refutation, that no materially reliable generalization ("logical general object") represents more than one individual object (Luschei, 1962, p. 27). Further, in conjunction with Lesniewski's refutation, it implies that the limit of expressible (quantitatively describable for traditionalist science), hence scientific, knowledge is (at least) one semantical category, one logical type, one cognitive level, one existentially relevant arrangement of material "wholes", below the level of current qualitative cognition. This epistemological implication of "Goedel's proof" (of the incompleteness of closed logic systems) was understood and exploited by Lesniewski fully a decade before Goedel presented his proof.

The fourth general principle: "There is no such thing as nothing". This principle arises directly from the realistic, material foundation of actively acquired purposive knowledge. The perspectivistic procedure is presented as a theoretical description of the general, materially effective, cognitive process which permits existentially effective active acquisition of purposive knowledge. The cognitive process described is proposed to be passively acquired by all existentially effective, cognitively limited, purposive knowers. That is, it is proposed to have arisen necessarily as the effective means by which cognitively limited purposive knowers deal with an inexpressibly complex and variable material universe (environment). The described cognitive process evolved out of existential need to deal with "concrete", "real" entities. If the evolutionary, material origin of cognition is accepted, then it follows that such "things" as "nothing", "zero", or "the null set" which are not material things at all, do not represent cognitively accessible entities. From a Lesniewskian logical, or perspectivistic viewpoint, the usefulness of the traditional idealistic concepts represented by these terms is related to the usefulness of the realistic concept represented by the term "randomness". Traditionally these concepts are discussed and used as though they refer to some ideal "thing" which would be convenient to have around even though it has not even an ideal "concreteness" and is not in any sense cognitively or materially accessible. For the perspectivist these terms refer to those cognitively apprehensible material things which are not cognitively accessible as related to the class of entities under discussion (being generalized about); or as Smith (1980, p. 41 in Medawar & Shelley) put it, those things among which

> there is no pattern,..., or, if there is, it would be better to ignore it.

It also follows from the fourth principle that, if purposive knowers are cognitively equipped to deal with entities which exist under the constraints of material reality, then logical (linguistic) entities ought to be conceived as though they were specific material entities, subject to the same constraints as material objects. That is, in order to be as cognitively accessible as possible, they should have the characteristics which enable cognitive access to material entities; namely, cognitively accessible individuality and inducibility from specific knowledge. Hence, Lesniewski's strict reference "to individual [logical or linguistic] expressions as concrete spatiotemporal objects" (Luschei, 1962, p. 4).

The fifth general principle: All existentially effective generalizations, even about collective classes with potentially infinitely numerous member entities, must be validly inducible from a finite number of experiences with entities cognitively accessible as collectively related individuals. If this were not the case, then, in order to discover effective generalizations, it would be necessary not only to have but to use an infinite amount of information; and no knower with limited cognitive capacity could meet such a requirement. Luschei (1962) provides a Lesniewskian view on this point,

> To establish a generalization "about members of a class" it is not always necessary to "comprehend all individual members" and confirm all substitution severally....Nor is this even possible when individual members or instances cannot all be enlisted or checked in a register, as when their number is indefinite or unknown, generalization being useful just when they cannot all be summoned, mustered, and called to attention for intuitive inspection, so that only inference or conjecture can provide more or less reliable information about them all, or about arbitrary members not present for roll call.... Lesniewski's terminological explanations and directives refer strictly to individual expressions of finite length in spatiotemporal context, not to forms of expression in abstraction from their occurrences, much less to "corresponding extralinguistic entities or to expressions infinite in number or length". He [Lesniewski]...asserted that he would not consider a "collection consisting of an infinite number of words" an expression at all.[11]

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[11] Recall the previous discussion of unambiguous, but infinitely long descriptions required for ideal universes with internally defined form. Lesniewski's work re-examined the ancient idea that language and reasoning are inextricably intertwined. Lesniewski, though, had a deeper appreciation than most, recognizing that natural languages co-evolve with knowledge and that their accumulation of expressive power shadows the accumulation of knowledge. As previous knowledge is superseded or expanded, it is meanings not expressions that become

The Cognitive Primacy of Wholes

These five general principles of the perspectivistic approach -- interpretations of empirical observations of the behavior of cognitively limited knowers -- provide a complete foundation for the development of a perspectivistic procedure. However, practical application of the general principles underlying the perspectivistic approach can not be traditionally undertaken, for it is ultimately a call to develop and exploit the natural intuitive, inductive capabilities implied by the existential success of purposive knowers. Unlike the traditional reductionistic approach, the perspectivistic approach does not pursue knowledge of entities (which possess properties of existential interest) by serially dissecting entities into subentities, subentities into subsubentities,...and then attempting to reconstruct a "new and improved version" of the original entity. The perspectivistic approach is not a Bertalanffian approach; it does not pursue knowledge of specific quantifiable interrelationships among the various parts in wholes of existential concern; it is not naively unconcerned that the number of

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outdated, and consequently the meaning of the same expression can differ in time or space. Which meaning of an expression is intended is established by the context of that expression.

> Lesniewski exploited the dependence of meaning on context,...the meaning of an expression in canonic language L [any language constructed according to Lesniewski's grammar], as in unformalized languages, depends not on its form alone but also on its use in propositional context. But whereas in unformalized languages context usually <u>reduces</u> ambiguity inherent in homonymy [uses of the same expressional form with different intended meanings], or different but analogous uses of expressions of such forms as 'is', 'exists', 'unique', or 'the', in canonic language L rigorous general conventions assure that context altogether <u>eliminate</u> indeterminacy of significance... (Luschei, 1962)

Establishing meaning contextually has the interesting effect of limiting the number of forms necessary for clear expression of even highly complex ideas.

interrelationships in a whole probably exceeds, in most cases by far, the number of isolable parts in the whole, that is, if there are any validly isolable parts in the whole. The perspectivistic approach focuses, instead, on recognition of the primacy of the whole as the simplest means of reliably identifying the entities that possess all the functions of existential concern at any given time or place.

A SEARCH FOR SIMPLICITY

It is important to recall the proposed existential function of the perspectivistic approach, and, hence, of any valid perspectivistic procedure, before considering a protocol for any perspectivistic examination of natural phenomena. The perspectivistic approach is proposed to be the cognitive approach selected during the evolution of purposive knowers because it enables cognitively limited knowers to select, from among an incomprehensible range of actions that could be taken in response to each of an incomprehensible number of existentially threatening situations, one of a few appropriate courses of action. The perspectivistic approach is a search for existentially relevant and effective, materially reliable, simplicity. Excerpts from Rapaport's "Search for Simplicity" (1972) summarize well the purpose, power, and weaknesses of the perspectivistic approach, (and reveal the previously mentioned ignorance of Lesniewski's work characteristic of supporters of GST):

> A strong case can be made for the search for simplicity as an activity rooted in a survival mechanism. A simple, predictable environment is easier to adapt to than a complex, capricious one....Science is clearly a systematized search for simplicity, a method of making the world predictable [understandable].... Understanding the world and controlling it are logically separable....Understanding the motions of the planets does not confer the power to control them. Nevertheless, there is an

> undeniable connection between understanding and control. Understanding the nature of the world can confer power over a portion of it....curiosity has probably antedated rapacity (the obsession with power) in the development of human psyche, since familiarity confers the survival-enhancing ability to predict, independently of the ability to control....In short, all understanding stems from perceived analogies -- recognition that something is like something else....In contrast to the mathematical concept, which defines a system as a set of relations among variables that are defined or postulated, the organismic concept [Bertalanffy's concept equivalent to Lesniewski's earlier concept of the collective whole] depends on an act of intuitive recognition....this ability does not depend on any conscious selection of variables and of relations among them: it is simply given to us, as it is to other animals....The question now is, how far can this recognition be stretched? What else besides organisms can we get to recognize as "systems"?...And what is a "theoretically fruitful" analogy anyway?...The quest for simplicity stems from a conviction that underlying apparent wide dissimilarities are profound similarities, which, when one perceives them, make order out of chaos, hence simplicity out of complexity...In pursuing investigations of this sort, it is well to keep in mind that most of them will lead to disappointments. We do not really have any serious "system laws" on which to build a grandiose theoretical edifice comparable to the edifice of mathematical physics.

Lesniewskian Linguistic Analysis

Rapaport (1972), as all the GST proponents I have read, appears not to have known of Lesniewski's logics which might be the serious laws on which a theoretical edifice could be constructed. One of the beauties of Lesniewski's logics in this regard is that there is no need to exhaustively redevelop a language developed during previous efforts to scientifically describe reality -- a prospect as disheartening as having to become fluent in German in order to understand Bertalanffy's GST vocabulary -- in order to have the ability to induce valid conclusions with respect to the relationship of material whole and parts of current existential concern. For, if a scientific language is materially reliable, i.e., consistent; then it can be presumed to be valid in a

Lesniewskian sense. Consequently, the individual need not concern itself with, for example, the validity of prior material observations because all materially related observations jointly may be presumed valid up to some Lesniewskian linguistic level, and that level can be identified by determining the highest level up to and including which descriptions of related material observations lead to consistent conclusions. This highest level of consistent description is one level below the current level of cognitive access, the level relative to which all currently consistent descriptions are defined.

A Perspectivistic Procedure

If the complete function of current existential concern is not consistently inferrable as a quality of one or more classes of functional collective wholes consistently describable on the presently describable level, then induction of the functional metawhole of existential concern is called for. At this point an inductive search for meaningful patterns among the inconsistently describable observations must be undertaken. Success in this undertaking is, as Rapaport (1972) pointed out, dependent "on an act of intuitive recognition", and not "on any conscious selection of variables and of relations among them". The capability of carrying out such inductions is primitive, "simply given to us, as it is to other animals", or, in the opinion of the present author, to all life forms. To be efficient in such inductive efforts, the mind should not be fettered with expectations based on previously acquired quantitative knowledge, but guided by the "sense" of meaningfully qualitatively describable wholes. In other words, under a Lesniewskian linguistic analysis approach; or on the one-level-higher, purely cognitive scale of the perspectivistic approach; it is invalid and debilitating, to attempt to define a priori, based on experience with quantitatively describable entities, the whole or type of whole responsible for the function of current concern, because valid identification of the (at this point) only qualitatively describable metawhole will be replaced by the self-realizing prophecy of a previously designated whole that is quantitatively describable and therefore not of an adequately high linguistic/logical level or material function.

Because of the primitive nature of the perspectivistic approach, a procedure is short, and specified by the form of the preceding discussion of a Lesniewskian linguistic analysis approach.

1. A function of material concern to the knower becomes apparent.
2. Efforts based on previously acquired knowledge fail to control the function or to identify appropriate responses to its occurrence; i.e., consideration of previously known functional wholes lead to contradictory conclusions regarding the identity of the functional whole of concern.
3. All entities which are validly describable at the currently highest logical/cognitive level are qualitatively identified -- quantitative descriptions are to be avoided, in order to avoid adulterating the knower's "cognitive innocence".
4. Those entities which can be experimentally demonstrated, or, on the basis of materially reliable previously acquired knowledge, concluded to have no effect on the function of concern are eliminated from consideration as parts of the whole of concern.
5. The candidate part entities are collectively submitted for intuitive consideration. Since inductive reasoning is not a conscious activity there can be no procedure for this step. Indeed, if the work conditions are correct, i.e., not excessively fixated on quantitative data collection, or overly demanding on the functional capacity of the mind, then this activity is probably occurring continuously. This is important since all the entities necessary to the induction of the whole of interest may not be known at the time of preparation of the list of highest-qualitatively-describable-level, candidate part entities.

6. The whole of interest, once induced, is not itself subject to quantitative analytical experimental verification. But its parts, which were previously entities of the highest-qualitatively-describable-level or other entities which were not consistently quantitatively describable will, if the induction was correct, now be consistently quantitatively describable. If they are not, if the function of concern is still not controllable, or if appropriate responses to the function can not be consistently selected, then the induction was invalid and should be abandoned (though this is not easily mentally accomplished) and the procedure re-initiated.

In the body of this dissertation I present the results of the application of the perspectivistic approach to examine the current predicament of agriculture as the consequence of a failure to diagnose traditional practices as materially unreliable (mutually contradictory) with consideration to the collective whole within which agronomic crop production is a major and essential function. Chapter 3 suggests that the logical/linguistic/cognitive level of the induced whole(s) currently used as the relativistic reference(s) for the description of the behavior of plant/soil systems is not adequately high, and attempts to present the inductive identification of an appropriate level. Chapter 4 presents a model of soil structural development compatible with the plant-control model (whole), induction of which was begun in Chapter 3. In conjunction, the plant-control hypothesis and soil structural development model form a higher (metawhole) level model of control and self-regulation in plant/soil systems. Under this higher level model, previously inconsistent quantitative descriptions of parts of the plant/soil system become consistent without loss of the practical usefulness of those descriptions. Chapter 5 presents demonstrations of the effectiveness of the high level model in enabling consistent quantitative descriptions where only inconsistent descriptions had been available before, and in predicting and interpreting the consistencies among observations made independently by different observers in different locations.

A CLOSING REMARK

There are several other relevant matters which I have not discussed, but this text must end somewhere, so I offer these closing thoughts. My time (all too long) in graduate school has inclined me to believe there is one very important thing that is strikingly difficult to find in science and academics today: sincere intellectual humility -- a virtue which seems almost impossible to develop or maintain by intent. And another is all too easily encountered: the elevation, in practice, of easily stated and apprehended logical or mathematical principles to the status of universal or natural law. As mentioned earlier in this appendix, Einstein's theory of relativity has been a victim of such unwarranted intellectual and social sanctification. Another victim is the principle of logic known as Ockham's razor and often interpreted as requiring that, among competitive explanations, simple explanations be accepted over more complex. A powerful and important tool in the realm of closed logic systems, its reliability with respect to accurate description of the material universe, the natural sciences, or any effort to deal with non-ideal universes is dubious. So I offer in closing this quotation from Rapaport (1972) (part of which appeared earlier in this appendix) as it reflects my own view about the role of science, humility, and simplicity in this my present effort and those of scientists, and human beings in general:

> A strong case can be made for the search for simplicity as an activity rooted in a survival mechanism. A simple, predictable environment is easier to adapt to than a complex, capricious one....Science is clearly a systematized search for simplicity, a

method of making the world predictable [understandable]....The search for simplicity, however, is seductive. It is easy to delude oneself into thinking one has discovered a great universal law, and delusions of grandeur of this sort are -- alas! too frequently -- apparent in the work of scientifically or mathematically semiliterate cranks...the line between creative and destructive, or self-defeating, effort is thin....So it is with the search for simplicity. The catharsis of insight is exhilarating, but the distinction between a genuine insight and a self-induced illusion is not clear. There is, however, one test to which one can put one's insights if one has the courage....If it [one's insight] only opens the mind to further, more tantalizing questions, if it makes one more humble than proud, it may be genuine. Insights derived from speculations instigated by perceived analogies function somewhat like education: they reveal to the intelligent and conceal from the stupid the extent of their own ignorance....Therefore, seek simplicity and distrust it.

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