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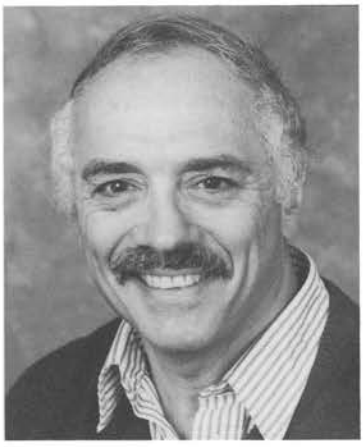

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For almost as long as the systematic study of human behavior has been a distinct field of scholarly endeavor, a dispute has been carried on between the proponents of two opposite points of view. On the one side are those who are convinced that, since human behavior is a natural phenomenon, no less natural than phenomena associated with energy and animate and inanimate matter, the same fundamental scientific attitudes and ways of constructing and testing theories should prove fruitful in the study of this part of nature as in the areas of what are known as the natural sciences. Members of this group are called *nomological social scientists* because they accept the view that social phenomena can be accounted for within a framework of rules. On the other side are the *antinomological social scientists,* who are just as certain there is something unique in human behavior which is inaccessible to the methods of science.

During the two and a half centuries that this debate has been carried on (from approximately the mid-seventeenth century to the present), many attempts have been made by each camp to demonstrate the correctness of its position. Entire fields of study have developed (anthropology, economics, history, political science, psychology, sociology); and while in each of these fields the nomological and antinomological viewpoints have almost always been in contention, usually one or the other has been dominant. Among economists, for example, with the exception of German economists prior to 1950, the view of the nomological social scientists has prevailed; until recently, among political scientists the antinomological social scientists have held sway. But the fight still goes on.

One thing is clear: Up to this time neither side has demonstrated the validity of its position. For nomological social scientists, demonstration of successful scientific theories of social phenomena, tightly logically constructed and empirically tested, would constitute support of their position. But this demonstration has yet to be given. In economics and psychology, the two most rigorous and quantitative of the human

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sciences, there are examples of tightly constructed mathematical or logical theoretical structures; however, they are built of concepts that do not refer to empirical matter in such a fashion as to permit empirical test. Moreover, in these two fields there are examples of empirical studies which reveal elaborate empirical regularities in a thoroughly systematic fashion. In other human sciences there have been very few empirical regularities discovered and very few attempts to account for such as have been found. Thus in the social sciences we find neither the construction nor the testing of powerful, tight logical structures of theoretical statements that have significant empirical content.

A frequently expressed concern of antinomological social scientists, such as Peter Winch,[1] is that important subtleties of meaning cannot possibly be specified in a formal system. Nomological social scientists claim that, to the contrary, subtle distinctions can be more readily drawn in a definitional system than in a system in which the terms are not plainly defined. Furthermore, distinctions in a definitional system are clear and precise because the components of the terms are visible rather than murkily stated, as is now generally the case. Winch's argument amounts, in essence, to the claim that understanding of social phenomena depends entirely on experiencing them, and that the responsibility of the social analyst is to be able to reproduce the phenomena. Thus, as Rudner has put it, "Winch's argument commits a rather subtle form of the 'reproductive fallacy.'"[2] Winch's argument, when unpacked, implies ruling out all means except experience (especially scientific and logical means) of gaining knowledge about social phenomena. This claim is rejected by nomological social scientists.

1. Peter Winch, *The Idea of a Social Science and Its Relation to Philosophy* (London: Routledge & Kegan Paul, 1958).

2. R.S. Rudner, *The Philosophy of Social Science,* Prentice-Hall Foundations of Philosophy (Englewood Cliffs, N.J.: Prentice-Hall, 1966), p. 83.

The case against the possibility of a science of human behavior would be made by the permanent failure of the nomological social scientists. That is, only with a proof of the impossibility of a scientific treatment can the antinomological social scientists' position be sustained thoroughly. This proof has not been offered, and no such proof appears to be in sight. And so the matter is still open.

This paper is an account of an attempt by two philosophers (the late Richard S. Rudner and Robert B. Barrett) and an economist (this writer), all nomological social scientists, to make an approach to the problem. The approach rests on the widely held view of a scientific theory as a linguistic entity. In this view a scientific theory is understood to consist of a set of two sorts of statements about a group of phenomena. The first sort are statements assumed to be true but not empirically tested. These statements are called *postulates,* or *axioms.* The second sort are statements which follow logically from the axioms. That is, they are implied by, or are deduced from, the axioms. These are called *theorems.*

For any reasonably rich set of axioms the total number of significantly distinct theorems which may be implied can be immense. Empirical test of a scientific theory as a linguistic entity can be carried out by attempting to verify that all those theorems which are known, and whose testing is feasible (assuming that the number of such theorems is not zero), are not inconsistent with observable data. If they pass this test, then the theory is supported and normally is accepted. If any of the known and testable theorems of the theory are *not* sustained, then so long as the scientific community trusts the test, the theory may be called

into question, dropped, or revised and tested again. Acceptance of a theory at a given time does not constitute a basis for its uncritical acceptance for all time thereafter. It is the hallmark of empirical science that all theories are, at all times, candidates for rejection or revision. Tests of theorems which were earlier untestable, as well as the discovery of (or new understanding of) phenomena hitherto unknown or understood differently, are all events which can lead to the questioning of hitherto accepted theories.

Since the components of this linguistic entity (the scientific theory) are statements, they must be constructed of *terms*. It is with terms that this enterprise is concerned. In effect, what my colleagues and I have begun to do is to construct a technical dictionary of terms out of which theories of the social sciences can be built. These terms are of two general sorts: The first are called *primitives*. These are not defined, but their meaning or content is clarified by example, by ostension (i.e., demonstratively). The second sort are the *definitions,* which are constructed in a very formal logical fashion. The building blocks of these definitions are the primitives and logical operators such as *and, or, not,* and *there exists a* ——; plus terms already defined in the field of investigation; as well as terms which are primitives in, or defined in, other fields such as logic, mathematics, or physics.

Richard Rudner and I met toward the end of 1956, shortly after we had both come to Michigan State University as young faculty members, he in philosophy and I in economics. We soon realized that we shared many intellectual attitudes and concerns, which, along with our complementary abilities, offered a basis for interesting work together.

In the spring of 1958 we began to ask ourselves why is was that the social sciences had not yet shown more signs of success as a scientific enterprise. Eventually we concluded that, while there were probably other factors involved, surely one had to do with the nature of the lexicon in which the social sciences—all of them—were conducted.

In each social science area, the lexicon consists of the natural language (i.e., the informal or everyday language) plus a sprinkling of technical terminology defined as in an ordinary dictionary. There is some variation among fields as to the extent to which mathematical notation and modeling are used. But the mathematics, whatever the extent of it, involves symbols whose interpretations are made in terms of the natural language.

These social science lexicons did not then, and still do not, display any of the definitional structure which we see in classical mechanics—and which has now become part of chemistry and portions of biology. As Bridgman has so clearly shown, all terms in classical mechanics, apart from names and mathematical terms, can in principle be constructed from a primitive base (i.e., a set of primitives) consisting of three terms: mass, length, and time.[3] One consequence of this is that there are only three terms in classical mechanics that are clarified primarily by ostension rather than defined. Thus all other terms are so related to those three primitive terms, and to each other through the primitives, that the process of measurement of each of the defined terms is logically related to the process of measurement of each of the three primitives—and thus through them to the process of measurement of all the others. This approach leads to greater clarity of meaning and less circularity of

3. P.W. Bridgman, *Dimensional Analysis* (New Haven, Conn.: Yale University Press, 1922).

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definition than is displayed in statements constructed of terms defined as in a natural-language dictionary.

These properties are characteristic of the lexicon of classical mechanics, but they are not found in the lexicon of any of the social sciences. There we find that successive uses of the same term by one speaker or writer involve slightly different meanings; the meanings differ even more if the terms are used by different speakers or writers. Measurement of one concept is not systematically related, through definitional connection, to that of another. We also find vast umbras and penumbras of unclarity. As Rudner and I rehearsed this state of affairs, we decided to devise a constructional framework for such a system of definitions—a framework appropriate for some field of the social sciences.[4] In the summer of 1958 and during the subsequent year, we developed our first set of definitions, based on nine primitive terms.

The area on which we decided to focus was the theory of organizational behavior. This was a political as well as substantive decision. Our reasons were, first, that organizational behavior was a field with which we were both familiar. Second, the field had been subjected to a fair amount of formal or quasi-formal analysis. Third, well-entrenched institutional interests and deep organizational commitments to particular methods were weaker in this area than in some others.

It was our belief (since borne out repeatedly by experience) that a proposal to adopt entirely new and strange methods and concepts would be resisted by practitioners in *any* field. New terms and methods are usually resisted until it becomes clear that their introduction may aid in the solution of an unsolved problem. Moreover, we expected that the vigor of such resistance would be in direct proportion to the degree of confidence of practitioners as to the good scientific standing of the field. Thus microeconomic theory, operant conditioning theory, learning theory, and demographic theory were ruled out as fields in which to introduce our proposals. Organizational behavior theory was territory that had been worked, but it was not one of the major demarcated areas of the social sciences. There were few departments of organizational behavior, few organizations or journals heavily committed to the field. It appeared then to be a good candidate for our effort, and it still seems so.

4. "The definitions of an uninterpreted symbolic system serve as mere conventions of notational interchangeability. . . . In a constructional system, however, most of the definitions are introduced for explanatory purposes. They may be arbitrary in the sense that they present a choice among alternative definientia *(definitions);* but whatever the choice . . . the definiendum *(what is defined)* . . . is . . . a familiar meaningful term. . . . A constructional definition is correct . . . if the range of application of its definiens is the same as that of its definiendum" [emphasis added]. N. Goodman, *The Structure of Appearance,* 3d ed. (Dordrecht, Holland, and Boston: D. Reidel Publishing Co., 1977), p. 3.

Given this decision, one question quickly arises: What are the terms to be defined? That is, what are the terms of which theories of organizational behavior, if there are any, are constructed? Indeed, our first concern was to look into the status of organization theory.

March and Simon begin their landmark survey of the field with the following paragraph:

> *This book is about the theory of formal organizations. It is easier, and probably more useful, to give examples of formal organizations than to define the term. The United States Steel Corporation is a formal organization; so is the Red Cross, the corner grocery store, the New York State Highway Department. The latter organization is, of course, part of a larger one—the New York State Government. But for present purposes we need not trouble ourselves about the precise boundaries to be drawn around an organization or the exact distinction between an*

> *"organization" and a "nonorganization." We are dealing with empirical phenomena, and the world has an uncomfortable way of not permitting itself to be fitted into clean classifications.*[5]

5. James G. March and Herbert A. Simon, *Organizations* (New York: Wiley, 1958), p. 1.

It is of interest to note that March and Simon, in this paragraph, express a much less sanguine view of the prospects for the conduct of nomological social sciences than they do elsewhere (especially Simon). Certainly their position is quite different from the one taken in this article. But what is particularly interesting in the quoted paragraph is the authors' assessment in 1958 of the definitional status of *organization.*[6]

6. In this respect there is no significant difference between the organization theory literature of today and that of 1958.

One important feature of their book, and the primary reason for referring to it here, is that it is a propositional inventory of organization theories. This inventory is organized, as the authors tell us, around three groupings of propositions:

> *1. Propositions assuming that organization members . . . are primarily* passive instruments. . . .
>
> *2. Propositions assuming that members bring to their organizations* attitudes, values *and* goals; *that they have to be motivated or induced to participate in the system of organizational behavior; that there is incomplete parallelism between their personal goals and organization goals, and that . . . goal conflicts . . . make power phenomena, attitudes and morale internally important. . . .*
>
> *3. Propositions assuming that organization members are decision makers . . . and that perception . . . is . . . central to the explanation of behavior in organizations.*[7]

7. March and Simon, *Organizations*, p. 8.

Propositions of the first sort are characteristic of the *structural approach* to organization theory (e.g., the scientific management theories of F. W. Taylor, Max Weber's theory of bureaucracy). Those of the second sort are characteristic of *motivational approaches* (e.g., the Hawthorne studies, the Michigan group-dynamics research). Those of the third variety are characteristic of the *decision-making approach* (e.g., management science, organizational decision-making studies). What was of special interest to us in the March and Simon work was the critical concepts in the sentences quoted—most especially those which the authors emphasized typographically. Other concepts were important as well, including some which were implicit in the authors' language such as features of organization structure, attitudes, values, goals, organizational goals, personal goals, decision, decision maker, perception, organization member, behavior, conflict, cooperation, and organization itself.

These concepts form a central core of notions to be found in all organization theories. My colleague and I understood their definition to be the central task in the construction of a system of definitions for organization theory.

But since these terms were used by a variety of organization theories, how could we be sure that they were always used in the same way—that they were to be understood in the same way by the reader? Indeed, our initial interest in this problem was, in part, motivated by the conviction that one of the troubles with social sciences was precisely this—that the same terms were, on different occasions, used differently. How could we deal with this problem of different intended meanings or, in the absence of clear intention, different loose understandings? Our

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approach has been to construct for each term a general definition which seems to cover most understandings (that is, a definition which covers far more territory than does any one usage) and then to apply qualifications to it in a careful, explicit fashion. Thus particular intended meanings were arrived at by a process of reduction of coverage through qualification.

These were the nine primitives we used: *time slice, producer or potential producer of, mechanical class, physical class, morphological class, functional class, event, probability,* and *belief.* Of these, the first six were borrowed from work by C. West Churchman and Russell Ackoff, two philosophers who attempted in the 1940s to formulate a definitional system for the field of personality psychology.[8] The work that Rudner and I did in 1958 and 1959 resulted in the appearance of a paper that incorporated a set of definitions—the primitives plus 100 definitions in four groups: preliminary (21 definitions), decision behavior (26 definitions), cooperation and conflict (21 definitions), and organization (32 definitions).[9]

This set of definitions was never applied to any body of theoretical discourse, and, after the publication of this paper, events interfered for a while with further activity. But in the summer of 1964, Rudner and I were able to begin work again. Rudner then suggested that we could profit from the involvement of Robert Barrett, his colleague in the Philosophy Department at Washington University. The three of us worked intensively together during the summers of 1964 and 1965, and on and off since then.

8. C.W. Churchman and R.L. Ackoff, "Psychologistics," mimeographed (University of Pennsylvania, 1947); and C.W. Churchman, "An Experimental Definition of Personality," *Philosophy of Science* 14 (1947):304–332.

9. R.S. Rudner and R.J. Wolfson, "Notes on a Constructional Framework for a Theory of Organizational Decision-Making," in *Decisions, Values and Groups* (London: Pergamon Press, 1962), 2:371–409.

At the beginning of this three-way collaboration we gave much thought to the differences between the *nominalist* and *Platonist* approaches to logic to which Nelson Goodman points.[10] Goodman asserts that severe ontological problems arise from the fact that, for purposes of constructing formal existential statements, Platonists (i.e., users of the calculus of classes) do not discriminate between individuals, classes of individuals, classes of classes, and so forth:

> *Use of the calculus of classes, once we have admitted any individuals at all, opens the door to all classes, classes of classes, etc., of those individuals, and so may import, in addition to the individuals purposely admitted by our choice of the special primitives, an infinite multitude of other entities that are not individual. Supposedly innocent machinery may in this way be responsible for more of the ontology than are frankly 'empirical' primitives.*
>
> *. . . The nominalistically minded philosopher like myself will not willingly use apparatus that peoples his world with a host of ethereal, platonic, pseudo entities. As a result he will so far as he can avoid all use of the calculus of classes, and every other reference to nonindividuals in constructing a system.*[11]

We found this argument persuasive, so we decided to formulate our definitions nominalistically. Consequently we used the calculus of individuals,[12] a nominalist form of logic developed by Goodman and Leonard which, in contrast to standard logic, permits existential statements to be made only at the lowest level of abstraction (the level of individuals)[13] and not simultaneously about abstract ideas. Thus state-

10. N. Goodman, "A World of Individuals," in *The Problem of Universals,* ed. I.M. Bochenski, A. Church, and N. Goodman (Notre Dame, Ind.: University of Notre Dame Press, 1956), pp. 13–31; Goodman, *Structure of Appearance,* pp. 24–33.

11. Goodman, *Structure of Appearance,* pp. 25–26.

12. H.S. Leonard and N. Goodman, "The Calculus of Individuals and Its Uses," *Journal of Symbolic Logic* 5 (1940):44–55.

13. *Individual* here means any tangible material object or collection of objects (*object* may refer to a living organism) or any part of an individual. An individual, if it is a collection, need not be homogeneous. That is, while a group of people, or a group of chairs, or a group of human legs (amputated or not), or a group of chair rungs (not necessarily separate from functioning chairs) can be an individual, so can a collection of a chair leg, five fingers (three still on a live hand), and a book.

ments like *There is a sweater* (a claim that a sweater exists) can be made in nominalist logic. But in nominalist logic one cannot also say *There is a heavy* (that is, that the weight of an object exists in any sense independently of the object).

Heavy refers to an idea or essence which approaches, but does not achieve, existence when it is an attribute of an individual. On the other hand, *sweater* refers to an object or individual. *This heavy exists* is not the same sort of expression as *This sweater exists*. In contrast to nominalist logic, standard logic admits treatment of symbols referring to abstractions in just the same way as it treats symbols referring to individuals. Consequently nominalists speak of standard logic as *Platonist logic,* in reference to the central place in Plato's philosophy of ideas or essences.

A byproduct of this decision to couch our definitions in nominalist terms is that the possibility was left open, although it was remote at the time the decision was made, of automatic proof of theorems. In principle, then, computing algorithms might be used to prove theorems.

In this second version of the system we reformulated our primitives, reducing their number to six and nominalizing them. Using these six primitives we have constructed 145 definitions. The new primitives are shown in Table 1, arranged against the set of nine which were used in the earlier version. In the second version the definitions fall into four groups which resemble the four groups of version 1: auxiliary (36 definitions), psychological (25 definitions), sociological (50 definitions), and organization (34 definitions).

The test of a primitive basis for a formal definitional scheme should concern itself with three questions: Can a set of terms referring to concepts concerning a major group of phenomena be defined out of this primitive basis? Does the above set of definitions naturally extend to concepts concerned with related groups of phenomena?[14] Is the set of primitives small in number relative to the number of concepts to be defined out of it?[15]

14. Connectibility of theories dealing with conceptually adjacent fields is desirable. It aids in the generalization of theories.

15. This question refers to the problem of simplicity of scientific theories. Until recently the problem has been dealt with in terms of the criterion of minimization of the number of axioms of a theory (usually referred to as Occam's razor). But some work of Nelson Goodman demonstrates that it is the minimization of the number of primitive terms (those terms of which are constructed the definitions used in stating the axioms) that is really the issue. See N. Goodman, "The Test of Simplicity," *Science* 128 (1958):1064–1069; and "Recent Developments in the Theory of Simplicity," *Philosophy and Phenomenological Research* 19 (1959):429–446.

There is reason to believe that both sets of primitives pass all these tests. In both versions the decision to stop constructing definitions came not because of the difficulty of construction but rather because it became clear that more definitions could be constructed when and as needed. Moreover we have found, when our interests have led us there, that not only have we been able to construct definitions for such terms as *decision, cooperation, conflict,* and *organization* but also we can easily see how we might formulate definitions for such notions as *investment* or *revolution,* for example. And the entire structure is built up out of six extralogical primitives (i.e., six primitives concerned with empirical substance and not with the apparatus of logical implication).

The decision to construct a formal lexicon for the field of organizational behavior entails a commitment to provide a lexicon in which one could express all theoretical statements of that field (i.e., all axioms or postulates and consequently, assuming full formalization of all theories, all theorems dealing with any portion of that field). That is, successful completion of this task should allow all formal scientific discourse in the field to be conducted in a lexicon consisting of defined terms and the primitives, plus all of logic and mathematics and other mature sciences,

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plus the names of individuals (i.e., elementary objects of the field), in place of the natural language.

Paul Feyerabend asserts that the meanings of terms used in the axioms of a theory are necessarily totally dependent on the theory itself.[16] The import of this claim is that the meaning of any term used in science is determined by the nature of the theory in which it is used. Thus if Feyerabend were correct (and this is a matter of considerable dispute), a given term could necessarily have different meanings in different theories.

16. P.K. Feyerabend, *Explanation, Reduction and Empiricism,* Minnesota Studies in the Philosophy of Science (Minneapolis: University of Minnesota Press, 1962), 3:28–97.

Feyerabend's argument is made with reference to terms which had not been the object of formal definitional analysis as they came to be used in science. In each theory in which they were used they were clothed (explicitly in some cases, implicitly in others) with meaning peculiar to the theory in question. But these events are matters of history, not of necessity. Nothing prevents a formal undertaking to the effect that term x, when used scientifically, in whatever theory, will always be understood in a certain way—perhaps in a way consistent with its definition in the lexicon here being discussed.

Moreover, to the extent that Feyerabend's point has real impact, it does so most of all in reference to lexicons arising in connection with fully formalized theories, and there are none in the social sciences. In any case, it could be argued that the problem is one not of confinement of a term to one theory but rather of translatability from one theory to another. But that is an issue which can be handled within the confines of such a definitional system as we are here describing. Moreover, to the extent that there are organization theories, we believe our definitions cover their major concepts and can be extended to cover the rest.

We set out to construct a formal lexicon which would cover the range of concerns of a field of study, rather than a theory. In this case we felt we had to cover individual decision behavior, cooperation and conflict between individuals, organizational structures, roles in organizations, and decision behavior in and of organizations. Thus there was a range of complexity—from the individual to the organization and its details—that we wished to span. In order to do this we needed to start with a primitive base which stood outside that range.

The six primitives of the current version of the definitional system (version 2), their readings and their clarifications (their presystematic understandings), are given below:

$x \, o \, y$ may be read as x *overlaps* y (*P*1)

This two-placed predicate holds between a pair of individuals just in case they have a part in common.[17] Note that it is with this primitive that we make our commitment to nominalism.

Suppose x is all of the left hands of all the presidents of the United States, and y is Theodore Roosevelt. Then the overlap of x and y is Theodore Roosevelt's left hand. In contrast, it could be the case that one of these individuals (x and y) is entirely a part of the other. Thus if x was Harry Truman and y his left hand, the overlap of x and y would be Truman's left hand; or if x and y were identical, their overlap would be the individual (x or y). If x and y had no part in common, they would

17. A predicate is a term that refers to an attribute of an individual or a relation among individuals. In the following five statements the italicized portions are predicates: The sweater *is red.* Bill *is the father of George.* This chair *is stronger than* that one. This set of diamonds, rubies, and paintings *is more valuable than* this tract of land. Jane *stands between* Pat *and* Bob. The first statement contains a one-placed predicate. (It is an attribute of an individual.) In the next three statements the predicate is two-placed, and in the last statement the predicate is three-placed. Predicates may be n-placed, with n being any positive integer, however large. A predicate can also be a term, which, being true of all members of some set, is the defining criterion of set membership.

have no overlap. In that case, the overlap would be nonexistent.

Es xy may be read as *x is an earlier time slice than y* (*P*2)

This two-placed predicate holds between a pair of time slices just in case the first-mentioned time slice is temporarily prior to the second mentioned. A time slice is an instantaneous cross section of the universe. That is, it is a photograph of the entire world and all the physical objects in it at an instant of time. It has extension in three spatial dimensions but no duration at all. *P*2 is the basic temporal concept of the definition system. It makes possible the introduction of such entities as time slices, as well as continuous sequences of time slices, which form time intervals. Moreover, with this primitive we are able to construct such notions as a slice part of an individual (that region of a time slice which overlaps an individual) and an interval part of an individual (that region of a time interval which overlaps an individual). To clarify a bit: A slice part of an individual is that individual's image at an instant of time. An interval part of an individual is that individual's existence during the interval in question. Finally, this primitive enables us to construct such temporal relations among entities as we may require.

Morph-id xy may be read as *x is morphologically identical to y*(*P*3)

This two-placed predicate holds between a pair of individuals just in case they satisfy all the same morphological predicates of the theoretical language. To say two objects or individuals are morphologically identical is to say that both have the same morphological predicates. That is, if one member of a pair of morphologically identical individuals has a morphological predicate, then so does the other.

For an individual to have a morphological predicate is for that individual to be partially described by being classified in a range. *Has an income of $100,000 a year* is not a morphological predicate, since it does not refer to a range. But *has an income of between $75,000 and $119,999* is a morphological predicate.

Suppose that all possible incomes were covered by a set of nonoverlapping income ranges. With each of these is then associated a morphological predicate, forming the set of morphological predicates: *low income, middle income,* and *high income* (associated with, say, incomes under $13,000; $13,000–$29,999; and $30,000 and over, respectively). Moreover let us assume that there is no other predicate in the theoretical language with respect to which individuals are classified into ranges (i.e., there is no other morphological predicate in the language). Then two individuals are morphologically identical if they fall into the same income range (i.e., if they both have the same predicate of *low income, middle income,* or *high income*).

Let us now consider a theoretical language which has these three income predicates and three more morphological predicates: *low IQ, middle IQ,* and *high IQ*. Individuals are said to have these predicates on the basis of their performance on the Wechsler-Bellevue intelligence test (score under 70; 70–130; above 130). If these are the only two scales in the theoretical language which generate morphological predicates, then two individuals are said to be morphologically identical if they

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have identical morphological predicates (and each would have exactly two). Otherwise they are said to be morphologically distinct. In general, two individuals are morphologically identical when one individual has every morphological predicate that the other has, and vice versa.

The predicate content of a theoretical language must include those predicates used in the theory constructed in that language. In order to keep morphological identity from being too restrictive a notion with respect to any theory, a theoretical language should contain only those predicates used in the theory under discussion. If more than one theory is drawn from a language pool that is largely but not entirely common to all, then the pool should be understood to be not a language but the collection of terms used in any of the theories under discussion. That is, the pool should be seen as the collection of terms in the language used in those theories. Within any given full interpretation of the system, *P*3 permits distinguishing those characteristics that the system counts as morphologically distinct from any other characteristics. It does this by enabling us to define *x is morphologically distinct from w.*

Con-x(p) may be read as *x conduces to p* (*P*4)

This one-placed predicate holds just in case *x* is an event such that some true sentence about it confirms an explanans of which *p* is the explanandum.[18] That is, event *x* conduces to statement *p* if, and only if, statement *p* is explained by an explanation which is confirmed by some true sentence about *x*. Hence, conducing here means that the investigator is persuaded, by a true statement about *x*, to accept statement *p* as true.

18. The explanans is the explanation. The explanandum is that which the explanation explains.

Let *p* be the following statement: *Other things being equal, the price level will rise.* Assume that statement *p* is explained by the pair of statements *L*1 and *L*2.

*L*1: Other things being equal, if the national debt increases, the quantity of money in circulation will increase.

*L*2: Other things being equal, if the quantity of money in circulation increases, the price level will rise.

Suppose the following is a true statement about event *x: The national debt has increased.* Then it is proper to say that *x* conduces to *p;* that is, the fact that the national debt has increased persuades the investigator to accept statement *p* as true. *P*4 is the basic confirmation relation of the system. Explications of such notions as *causation, influence,* or *function,* insofar as they enter into the system, are introduced by means of this primitive.

F-bel-(p) may be read as *Individuals which have F believe that p* (*P*5)

This sentence scheme is true for a given choice of statement *p* and, say, predicate *G,* just in case the predicate *F* is manifest and the fact that an individual satisfies it is accepted by the investigator as a sufficient condition for his believing that *p*.

Suppose the predicate *F* is understood as *genuflects upon entering a Roman Catholic church,* while *p* is the statement, *The pope is the vicar of Christ on earth.* Primitive *P*5 would now read, *Individuals who genuflect upon entering a Roman Catholic church are construed by the investigator to believe that the pope is the vicar of Christ on earth.*

This is a scheme which serves to define a particular belief (which we

usually understand to be a mental state) by associating statements about it with a manifest predicate. That is, in this theoretical language, genuflection upon entering a Roman Catholic church would be seen by the investigator as grounds for saying the individual in question believes the pope is Christ's vicar on earth. In another more complete language there might be several manifest predicates (e.g., in addition to *F,* perhaps *D, dips hand in holy water font upon entering Roman Catholic church; C, carries rosary into church*), any one of which, being satisfied, is taken by the investigator as grounds for saying the belief is held by the subject individual. All notions involving belief, such as the veridical-perceived distinction and the subjective-objective distinction, that are used in the definitional system are introduced by means of this primitive.

Index-(F)x may be read as *x satisfies an index of F* (*P*6)

This one-placed predicate is true of any individual for any choice of predicate just in case the individual satisfies an index of the predicate. An index of a predicate is another predicate satisfied by everything satisfying the former and by some other things besides, where there is some theoretical justification for holding that these other things would have *F* too, under appropriate circumstances.

Suppose *x* is a phonograph record and *R* is the manifest predicate *is being played.* Then *Rx* can be read, *Phonograph record x is being played.* Consider the phonograph record *y,* which is not being played: That is, *y* does not have the manifest predicate *R.* But if *T* is the dispositional predicate *is playable,* can we say on what sort of grounds we might be justified in saying that *y* has *T*?

We might examine the phonograph record and find that it has the following three manifest characteristics: It appears to be well made; it has a hole of the proper shape and size at the proper location; it is not obviously scratched. If we define predicate *C* as *has the phonograph record characteristics given in the preceding sentence,* then manifest predicate *C* can be taken to be an index of *T.* Or, instead of *C* we might accept manifest predicate *W, is warranted by a reputable manufacturer of phonograph records,* as an index of *T.* Thus there might be a multiplicity of indices of a given manifest predicate (each of which is itself a manifest predicate).

Index-(T)x could then be read as *phonograph record x satisfies an index of playability.* The grounds on which this is said could be the fact that phonograph record *x* has the manifest predicate *C;* or the fact that phonograph record *x* has the manifest predicate *W* or any other predicate taken by the scientist to be an index of dispositional predicate *T.* In general, in establishing an index of a dispositional predicate like *T,* it would seem most reasonable that an index itself be a manifest predicate. The role of *P*6 in the system is to permit projections from manifest predicates to dispositional predicates where such are required.

Of these primitives there is one which uniquely enables the system to deal with more than social physics (i.e., with more than an account of physical properties of social individuals and institutions). That is, it permits construction of definitions of concepts involving notions of goal, purpose, intention, knowledge (including self-knowledge), expec-

tation, and so forth. This is primitive *P*5, *Individuals which have F believe that p.* The other five primitives are concerned with physical or logical matters.

A central concept in our lexicon is that of purposive behavior (which we take to be identical to decision behavior). For us, purposive behavior is the proclivity to carry out one or more of a class of behaviors, each different from the others, but each producing the same or a very similar state of affairs. Thus an agent (i.e., actor) may display purposive behavior with respect to some goal states but not with respect to others.

We speak of someone displaying *K(FG)-for-Ha* purposive behavior, where *F* and *G* are predicates referring to different sorts of action, both of which belong to the class *K* of actions; any member of *K* will eventuate in a certain range of outcomes. *K* may be, for example, the class of actions that transport a person a few miles through urban traffic. Thus an individual may be a *K(FG)-for-Ha* purposive behaver, where *K(FG)-for-Ha* is the selection on a morning of one (*F* or *G*) of several kinds of urban transit *(K)*. The agent might ride in his own or a friend's car or on a bicycle so as to get himself *(a)* to his office on time *(H)*. But the same individual may not be an *L(MN)-for-Ja* purposive behaver, where *L* is the class of actions (e.g., *M* and *N*) involved in cooking for breakfast so as to provide the agent with a warm breakfast *(Ja)*. In this case, rather than doing *L(MN)-for-Ja,* the agent munches mechanically and absentmindedly on whatever he randomly reaches for in his pantry while reading the morning paper. It is the morning paper that has his purposive attention, not his breakfast.

What we are saying of purposive behavior is that it is not just action that leads to a specified state of affairs. Any simple automaton is capable of that. A thermostatic furnace or refrigerator control or toilet-flush mechanism does not perform purposive behavior or decision making. Rather, purposive behavior is action that is expected to result in the attainment of the specified state of affairs, while alternative means to that attainment are available and known to the actor. Moreover, one can infer (in ways which are specifiable as observational techniques are developed) that if conditions are unfavorable and a given strategy fails, one or more of the remaining alternatives will be selected, serially and appropriately; thus in most circumstances there is a greater probability of successful attainment of the goal than there would be if a smaller number of strategies were available. The use of *(FG)* does not mean there are only two strategies; *F* and *G* are specimens of the class *K,* which may be very large.

Moreover, we are saying that a purposive behaver is not necessarily purposive about everything. In the example above, the agent is behaving purposively about getting to his office on time but not about having a good warm breakfast. He is a *K(FG)-for-Ha* purposive behaver but not an *L(MN)-for-Ja* purposive behaver. In addition, it is possible for a nonhuman animal (e.g., bear, amoeba) or a computer running under the proper program to be a purposive behaver with respect to certain goals. Finally, an individual may be a *K(FG)-for-Ha* purposive behaver at one time and not at another. This is probably very common.

This notion of purposive behavior enables us to build up to cooperation (i.e., an individual acts so that his purposive behavior facilitates

activity associated with the purposive behavior of another purposive behaver); conflict (i.e., an individual acts so that his purposive behavior inhibits the purposive behavior of another purposive behaver); and teleologically neutral behavior, where neither of the above is true. Moreover, the structure of these definitions enables us easily to distinguish between, for example, cooperative behavior of a person; intentional and unintentional cooperative behavior of a person; and mutual and quid pro quo cooperation (where the desired states of the two agents are different). Thus we are able to build a great many nuances of difference into the lexicon—something that most antinomological social scientists claim is impossible in a formal lexicon.

One attractive set of distinctions which we are now able to make clarifies the differences between conflict, cooperation, and rivalry. *Conflict* is described above. *Competition* is mutual conflict over the same ends; that is, x and y are in competition with each other if x is in *K-FG-for-Ha* conflict with y, and y is in *K-FG-for-Hb* conflict with x, where the *K, F, G,* and *H* are the same for each of the expressions. *Rivalry* is a situation in which x and y are in competition or are disposed to be—and, in appropriate circumstances, would be. No ordinary-language dictionary which has come to our attention makes such clear distinctions as these.

Another interesting case has to do with the notion of revolution. But in order to discuss this, we must first look at the treatment of organization. Our definition of organization is quite complex and presents some difficulties whose elimination is still a concern of ours. However, it is clear that in essence it will entail considerations of the following sort:

An organization is a group of individuals divided into subgroups that are functionally distinct from one another (i.e., they perform distinct functions within the organization). Each subgroup has a goal or goals peculiar to it. Moreover, these goals, when attained, are instrumental for the attainment of the goal or goals of the organization. Certain aspects of the definition of *organization* are concerned with (1) goals—of the subunits and of the organization; (2) roles performed by members of the organization (e.g., treasurer, maintenance man, foot soldier, membership secretary); (3) structure of relations between roles, between functional subunits, and so forth; (4) incumbencies (i.e., role assignments to specific individuals).

A reorganization is any sort of change in any of these four aspects of the organization. Depending on which aspects change, there are at least four major types of elementary reorganization. Since a government is a form of organization, these comments apply to governments as well. If the reorganization takes place without the consent of those whose agreement is required for lawful reorganization, the reorganization is a *revolution.* There are at least four sorts of revolution: If incumbencies alone change, there is a coup d'état. If incumbencies and structures of relations change, there is a more sweeping sort of revolution. If these as well as roles change, the revolution is even more substantial. If all these, and goals, change, there is a true *social revolution.* Note the unlikelihood of goal change without role and incumbency change, and so forth. A goal-change revolution is likely to entain role, structure, and incumbency change. A role-change revolution may not entail goal

change, but it probably involves structure and incumbency change. A structure-change revolution may not entail change in rules and goals but probably involves change in incumbency. A coup d'état, however, probably does not go beyond incumbency change. Again, detailed examination and classification of this set of phenomena is surely facilitated by the clarity of formal definition.

We have devoted much thought to the question of how a definitional scheme can be applied. In general, there are two types of applications that present themselves. One is to use the system as a means for examining, and subjecting to logical test, theories in the social sciences which are not fully formalized. That is, we propose to examine any putative theory in the social sciences, since there are no fully formalized social science theories.

In this sort of application an extant body of theoretical material would be recast in terms of these definitions; and any claims as to logical connections among the statements comprising that body (e.g., logical independence of axioms) would then be examined. Further, possibly clearer, more interesting, and more powerful deductions from the axioms might emerge. Thus in this use the definitional system might seem a test-bed for infant theories in the social sciences. And it would in all likelihood aid in the further formalization and development of such theories.

The second type of application is hinted at in the preceding paragraph. Here the theorist would start from scratch in this lexicon instead of translating into, and then further developing, extant theories in terms of this definitional scheme. Thus the theorist would work in these terms as the theoretical physicist does in his, rather than start out in terms of humors or fluids, proclivities or tastes, as occurred in natural science before Newton and is too often the case in the social sciences now.

Thus far we have not attempted any full-scale applications. During the few years before Rudner's death in July 1979, we had several ventures under discussion. They included filling in some lacunae in the definitions, preparing a discussion in the metalanguage (i.e., the language in which the formal language is discussed) so as to simplify moving in and out of the formal language, and developing some applications of the system.

Another line of investigation (which we pursued after 1979) goes to the matter of mechanical (i.e., computer based) proof of theorems. Today, in contrast to the situation in the early 1960s, there is a body of increasingly powerful mechanical theorem provers which have been developed by the artificial intelligence community (J.A. Robinson, E.E. Sibert, and K. Bowen at Syracuse; R. Kowalski at Imperial College, London University; and others). Barrett and I are currently looking into the possibility of making use of this computer technology to manipulate the ferociously complex statements which would result from the use of this lexicon in recasting extant theories or in formulating new ones.

Rudner's untimely death seriously interfered, for a while, with our abiliity to carry forward some of our projects. However, Barrett and I now anticipate that a major piece of work will be ready for publication within two years. It will consist, first, of an extensive discussion of the philosophical and scientific considerations underlying the development

of a constructional framework for a system of definitions for the social sciences. A second major component will be a detailed explanation and exposition of the system itself, replete with illustrations and clarifications of the definitions. Finally, we will present a few small-scale applications of the definitional system to social science theory.

We believe that with the appearance of this work, social scientists will find that a new and powerful tool has become available, which should aid in the formulation, development, and logical analysis of social science theories. If we are correct, the social sciences will be in a better position than heretofore to become nomological sciences, with all the good and evil consequences, social and scientific, that may flow from such a development.

TABLE 1 TWO VERSIONS OF PRIMITIVES

Version 1	Version 2
.....	x overlaps y
Time slice	x is an earlier time slice than y
Morphological class.....	x is morphologically identical to y
Mechanical class.....	
Physical class	
Producer or potential producer	x conduces to p
Functional class.....	
Event.....	
Probability.....	(Seen as settled in logic and mathematics)
Belief	Individuals which have F believe that p x satisfies an index of F

NOTE: When x and y are individuals, F is any predicate and p is a statement.