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A LOOK BACK AT THE AMERICAN NAME SOCIETY

Is a Theory of Names Possible?

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A theory is needed in any systematic study to guide those working in the field. Theories are of two types: strong or scientific, which contain an experimental procedure for potentially disproving the theory, and weak or general, which are incapable of disproof but are still useful guidelines for study. Onomastics, like linguistics generally, is capable of only a weak theory. Seven desiderata for a theory of names are suggested.

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Those of us who study names usually do so in the same spirit and much the same way that stamp collectors treat postage stamps.

There are, to be sure, great differences among collectors, whether of stamps or of names. Some collectors gather their specimens randomly, as they happen to find them, and keep them in an old shoe box to bring out on appropriate occasions for the interest and amusement of friends. Others set about to gather methodically all the specimens of a given kind from a given place, to study those stamps or names in as minute detail as possible, and to classify them according to the generally accepted categories of philately or of onomastics. Between those two extremes there is a great continuum of other collectors.

A diversity of philatelists and of onomatologists is a good thing, because collectors of all kinds are useful. Amateur collectors enjoy the hobby, provide a base for more advanced and professional studies, and may turn up specimens that even the most advanced collector needs to pay attention to. Professional or "scientific" collectors discover the best way to go about collecting, provide an overall view of the activity, and are resources to whom the beginning collector can turn for guidance. Thus there is room for everyone and for all degrees of seriousness in collecting stamps or names.

Advanced or professional onomatologists may occasionally wonder, however, whether there is not, or ought not to be, something more to their enterprise than just collecting and sorting and displaying the results, however meticulously and professionally the collecting-sorting-displaying is done. In the deep hours of the night,

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what onomatologist has not sat bolt upright in bed, with the question ringing in a sleep-befuddled brain: "Is a theory of names possible?"

To be respectable nowadays in academia, one must be scientific. And to be scientific, one must be theoretical. Pity the dry-as-dust old physicist who busies himself with weights on an inclined plane, when string theory is where it is at. It is hardly surprising that linguists and onomatologists also hanker after a theory for their disciplines. But, to understand what a theory of names might be, it will be useful to think briefly about the history of theory in science and about what is meant by theory, in general.

The high prestige of theory reflects a real change in the way scientists and other scholars have come to think of themselves and of their activities. When Francis Bacon was promoting the new science of the Renaissance, he had to struggle against a view of scholarship that assumed the basis of all knowledge was already available in the form of general principles enunciated by classical philosophers like Aristotle, and elaborated and harmonized with Christian revelation by theologians like Aquinas. To escape the trap of a closed mind, to cease to worship the idols of the mind, as Bacon called them, he stressed the need for inductive as opposed to deductive thinking.

Deductive thinking begins by assuming the existence of general principles and deduces from them consequences that have to be true; observation is irrelevant to it. For example, if we assume that God made the world, that everything God does is done perfectly, and that the perfect geometrical figure is the circle, we may conclude (as some early astronomers did) that the planets must all have perfectly circular orbits. If observation suggests that they have rather elliptical orbits, then so much the worse for observation. Given the choice between a beautiful principle and an ugly, contradictory fact, the deductive way to save the principle is to ignore the fact.

Bacon rejected such dogmatic deduction by stressing the need for inductive thinking. In the Baconian view of induction, we begin by observing, by collecting as many facts about our subject of study as we can find. Then we sort those facts to see what it is they have in common or how we can best explain them. Thus we induce general principles from the observation of many particulars.

To a great degree, the study of names today is Baconian. And it shares the weakness of any purely Baconian approach to science. The problem with Baconian induction is that those who practice it tend to spend all their time in collecting data, and seldom get around to explaining much of it. Explanations or general principles do not arise spontaneously out of a mass of data, however great the mass may be. Indeed, the bigger the mass, the bigger the mess. Ironically, the more particulars one has, the harder it is to find a general principle lurking among them.

The opposition of pure induction to pure deduction creates a paradox. If we are purely deductive, facts are irrelevant, but facts are what science is about. On the other hand, if we are purely inductive, we can never rise above particular facts, although the aim of science is to explain particular facts as instances of universal principles. The way out of this Catch-22 is to get theoretical.

Theories involve hypotheses and experimentation. A hypothesis is a general principle that is held tentatively, not as a matter of divine revelation or of self-evident truth, but as a possible explanation for particular facts. Neither are hypotheses derived from sifting masses of data; they do not arise by spontaneous generation out

of the mess of fact. Indeed, no one knows exactly where hypotheses come from. They are lucky hunches or intuitions. Their existence is the link science has with mysticism, though few scientists care to acknowledge the link. For the scientist, the important thing is not where the hypothesis came from, but how it can be experimentally tested.

The scientist asks, "If this hypothetical principle is true, what particular facts can I deduce from it?" That part of the activity is deductive. Then the scientist asks, "Are the facts that I can actually observe in the world the same as the facts that I have deduced from this hypothetical principle?" That part of the activity is inductive, since someone has to make some actual observations and gather some real facts to answer the question.

The process of testing the hypothesis — testing being the combination of deduction and induction just described — is experimentation. When experiments show that the predicted facts and the observed facts are different, the hypothesis has to be thrown out, however beautiful or appealing it may be. When experiments show that the predicted facts and the observed facts are the same, the hypothesis is tentatively confirmed and has then become a theory. It is the unhappy lot of hypotheses and theories to be subject to disproof, but never to be capable of proof, since at any time new facts may be observed that are inconsistent with the predictions based on the hypothesis or the theory.

When a theory has been subject to repeated testing and it has been consistently confirmed by that testing, it may be promoted to the status of a law. Laws are thus general principles that have survived the prolonged rigors of extensive experimentation. They are still not proven principles, since proof is in the nature of science an impossibility, but rather laws are principles that a lot of very determined scientists have not been able to disprove. They may still be wrong.

Laws, however, are hard to come by. A usual situation in science is that experimental evidence may generally confirm, but occasionally will conflict with a theory or hypothesis. In such cases, the scientist may decide that the experiment was flawed and therefore should be ignored, or that the theory is incomplete and therefore needs tinkering with. Only when a great deal of disconfirming evidence mounts up is a scientist likely to throw out a previously established theory — and then only if some better theory is at hand, that is, one whose predictions are better matches with observed fact.

Thus, as the concept has developed in the history of science, a theory is a generalization that correctly predicts observations, at least most of the time. That is what we might call a "strong" concept of theories. There are, however, other concepts and other ways of using the term *theory*.

Kenneth Pike likes to point out that theories are like windows — they afford us a view (Pike, 1982: 5–9). The word comes to us from the Greek noun *theoria*, meaning "an act of viewing, contemplation," based upon the root of the verb *theorein* "to look at, contemplate, consider." A theory in this more etymological and general sense is a way of looking at the world and of ordering the impressions we have of it. It is a viewpoint we adopt for contemplating reality. In this "weaker" sense, a theory is a set of categories by which we order our experience and relate one experience to another within some intellectually and perhaps esthetically pleasing whole. In this

sense of the word, we can appropriately talk about a theory of art or of morality, or of various other things that are quite outside the sphere of science, such as postage-stamp collecting. "Strong" theories are scientific, "weak" theories are not.

This second sense of *theory* is "weak" only in that the theories it denotes contain within themselves no technique for disproving themselves. Such theories are not self-testing, but have to be evaluated and to be accepted or rejected in other ways. "Weak" theories may be concerned with and judged by values (economic, moral, esthetic, etc.) or by purely pragmatic considerations (how well the theory serves a particular purpose under a particular set of circumstances). If two "strong" theories conflict with one another, one of them eventually has to give, or else both have to be superseded by a new hypertheory that reconciles their conflicts. Two "weak" theories may conflict with one another and yet continue to coexist, being used for different purposes or under different conditions.

The question is, if we are to have a theory of names, whether that theory will be a "strong" or a "weak" one. Is onomastics a science or something else? That question is related to the question of whether linguistics is a science or not, because finally onomastics is a branch of linguistics.

To assert that onomastics is a part of linguistics is not to deny the obvious fact that many persons who study names are little interested in traditional linguistic matters but are rather very much interested in such other disciplines as geography, cartography, history, genealogy, sociology, anthropology, psychology, and literary criticism. Disciplines obviously cross over and interconnect. Persons in many disciplines have a natural interest in names, since the proper names of persons or of places impinge upon so many other aspects of life. And thus there are onomastic geographers, onomastic psychologists, onomastic literary critics, and so on. That is all quite as it should be.

Nevertheless, if we consider names apart from the things they name, apart from the circumstances in which they are given and used, and apart from their users, that is, if we focus on names per se, it is clear that they are a kind of word. And words are the basic features of language. So basically, onomastics would seem to be a part of linguistics, albeit a part generally ignored by linguists (a good example of linguistic theory reaching out to accommodate onomastic facts is provided by Sullivan, 1977). To make such a statement is not to deny that onomastics can and doubtless should also be considered as an autonomous discipline that has affinities with linguistics, as it does with geography, literary criticism, and many other disciplines. It does imply, however, that whatever limitations linguistics has as a scientific discipline, onomastics will share.

If onomastics and linguistics are scientific, they must also be predictive. Without predictions, a theory cannot be tested and is therefore not scientifically relevant. Beginning in the nineteenth century, historical linguists tried very hard to find universals of language change. Those efforts are continuing today, especially with attention to language typology. If firm universals of a certain kind can be discovered, they might be used as a basis for predicting. For example, if all languages that order their objects after their verbs (instead of before them) also use prepositions (instead of postpositions) and put their adjectives after their nouns (instead of before them), we can say that a change in any one of those order features should result in a change in

the others. That would be a theoretically relevant prediction. The results of all such study seem to be that, although there are universal tendencies in languages (such as the ordering features just mentioned), they are just that — tendencies, not the basis for reliable predictions. Historical linguistics is apparently not a predictive science.

During the last half of the twentieth century, an effort was put forth to make of descriptive linguistics a scientifically theoretical discipline. The form of linguistic theory known as "generative grammar" attempted to write a grammatical description of a language that would predict which strings of words are grammatical sentences of the language and which are not. If a generative grammar could be written for any natural language, it would be a predictive theory of that language, in the strongest scientific sense. Despite years of work, we are a long way from having a real, as opposed to a postulated, generative grammar of English or of any other natural language.

Indeed, Charles F. Hockett argued that such a grammar cannot, in principle, be written (Hockett, 1968). The aim of a generative grammar is to predict whether any given string of words is or is not a member of a particular set, namely, the set of grammatical sentences of the language for which the grammar has been written. That aim presupposes that the set of grammatical sentences of a language is well defined. Sets may be either well defined or ill defined. The set of the capitals of the fifty states of the United States is a well-defined set. Everybody of normal, rational mind can be gotten to agree on which cities belong to that set and which do not. On the other hand, the set of the fifty most livable cities of the United States is an ill-defined set, because there are no generally accepted criteria by which "livability" can be measured and consequently normally rational-minded persons will disagree on what cities belong to the set.

Are grammatical sentences like the capitals of the fifty states, or are they like the fifty most livable cities of the United States? There are, of course, many strings of morphemes that every normal speaker of English would unhesitatingly accept as grammatical (for example, *She likes pie*). And there are others that every normal speaker would reject as ungrammatical (for example, *Pie like shes*). But both experiments and everyday observation convince us that there are many strings of words that we disagree about among ourselves or of which we are uncertain. These are not merely the sort of sentence that usageasters quibble over (*Everyone has a right to their own opinion*) or that is a matter of dialect diversity (*Alabama is/are first this week*). They are sentences of the sort whose very possibility is doubtful.

For example, is the following sentence grammatical? *Someone poisoned the cheese that the mouse that the cat chased ate*. Some speakers of English maintain that, although such a sentence is a bit hard to understand and is improbable, it is quite grammatical. Others say that it can occur only in the twisted imagination of a linguist and is impossible in real language. Or, as a subtler example, is the sentence *Coincidentally he is at the same time the blacksmith and Thomas Smith* grammatical or not? Can one coordinate common nouns and proper nouns? Some linguists have said not, but have offered no evidence. Indeed, such examples raise the question of whether grammaticality is a categorical matter or one of variable probability. The latter possibility raises additional problems for predictions.[1]

It is an indisputable fact that we cannot get agreement about the grammaticality of many putative sentences of English (or of any other natural language), and the lack of agreement is evidence that the set of grammatical sentences is ill defined. That being so, the aim of a generative grammar is unattainable. Generative grammars can, of course, approximate their aim, but they can never achieve it. And if the aim is in principle unachievable, then generative grammars have no more predictive value synchronically than typological theory does diachronically. Because linguistic theory cannot predict in the way that scientific theories must, linguistics is not properly a science at all, but an art.

One consequence of linguistics being unscientific is that onomastics, which deals with a subset of linguistic facts, is also unscientific. That conclusion must be accepted until such time as someone proposes an onomastic theory that can predict onomastic facts. The day of such a theory does not seem imminent. Such onomastic theory as we can reasonably hope for is not likely to be a predictive theory. It is rather going to be the unscientific sort of theory that offers a view of the field for onomatologists to work within.

What, then, may be the aims of such a more modest and "weaker" onomastic theory? The following suggest themselves:

1. The first aim of any study ought to be to know what it is about. And therefore onomastic theory needs to consider what is to count as a name, and thus as an object of onomastic study. What we mean by *name* is the first question to be answered. And the answer should include a characterization of proper names that distinguishes them from other words in a language.
2. The theory should provide us with a descriptive framework for sorting out different kinds of names and for distinguishing the various processes to which names are subject and the various ways in which names are used. Although *taxonomic* was for a time used as a slur word by one school of linguistics against other schools, every theory has to include a taxonomy of its subject.
3. The theory should be capable of dealing with both synchronic and diachronic facts of its subject. In onomastics the sharp division between historical study and descriptive study of an *Jtat de langue*, which has characterized twentieth-century linguistics, has never been made. In naming, more than in other aspects of language, users are aware of history. For that reason at least, a distinctive approach to the synchronic/diachronic dichotomy is called for.
4. The theory should rethink the distinctions of Saussurian parole/langue and of Chomskyan performance/competence as they apply to names. Names may be more distinctly a part of parole and of performance than is any other aspect of language. As names are private and practical in their nature, the individual use of names may form an important part of the theory of onomastics.
5. The theory also needs to distinguish itself from other theories, especially from theories of allied disciplines. So an adequate onomastic theory should tell us how onomastics differs from linguistics, geography, literary criticism, philosophy, and so on, especially as they deal with names.
6. At the same time that it distinguishes onomastics from other disciplines, the theory should be capable of showing how an onomastic system, including name invention and name use, relates to other aspects of human life. Kenneth

Pike called his magnum opus *Language in Relation to a Unified Theory of the Structure of Human Behavior* (Pike, 1967). We need a theory that treats naming in relation to some unified theory of human behavior.

7. The theory should also aim at discovering such universals of naming as may exist. It seems likely that such universals will be tendencies rather than categorical distinctions, but even tendencies can tell us a good deal about names and human nature.

There are doubtless other desiderata for a theory, even a "weak" theory, of names, but seven is a number hallowed by tradition. If a theory of names is possible, it will do well to meet even these seven modest aims.

Note

[1] Hockett's argument is that no physical system is well defined and thus generative grammars that describe what an ideal speaker-hearer knows are not adequate for dealing with a real language. The language of logicians, of mathematicians, or of generative grammarians is a restricted language that lacks the full richness, and indeterminacy, of real language. Much the same thing can be said of the concept of the proper name as it has appeared in logic. A generative grammar that incorporated variable probabilities in its predictions might answer the need. But before we can be sure, someone must write one.

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