2004

Ritual Syntax

Carl Andrew Seaquist
*University of Pennsylvania*

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Abstract
Frits Staal developed a theory of ritual in the late 1970s that is well-known to ritual studies scholars, though consistently misunderstood. Most attention has focused on Staal's claim that rituals are meaningless, since this seems contrary to most theories of ritual, and to professed beliefs of ritual practitioners. I show that Staal's treatment of meaning is more subtle than most readers allow, and I demonstrate that other theorists make similar claims, but my main focus is on another part of Staal's theory: the claim that rituals have the same formal structure, or syntax, as natural languages. Staal adapts to ritual an approach originally developed for language by Noam Chomsky, to the effect that ritual structure is sufficiently complex that it can only be modeled by what is known as a Context Sensitive Language. Seen in this light, Staal's theory is really a cognitive theory of ritual, in other words it is a theory of the mental qualities that are necessary for a person's actions to count as ritual actions. My final chapter therefore considers this theory in the light of recent, cognitive approaches to religion, especially the work of Dan Sperber and Pascal Boyer. Most work in the cognitive science of religion relies on methods from cognitive psychology, and Staal's theory is unique in presenting a computational model of ritual structure. It focuses our attention on the sequential ordering of the elementary actions that compose ritual sequences, and in the process it opens up a wide range of research programs for ethnologists and historians, as well as for ritual studies theorists. Staal's theory is based on data from the Vedic ritual tradition, especially the fieldwork he pursued on a performance of the Agnicayana in South India; in the process of examining Staal's theory, I consider a variety of topics relating to South Asian and Vedic ritual and grammatical theory, and I supplement this with a look at exempla from other ritual cultures. In addition to a thorough analysis and critique of Staal's theory, I provide the foundation for what I call a "distributional" study of ritual structure.

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RITUAL SYNTAX

Carl Andrew Seaquist

A DISSERTATION

in

Religious Studies

Presented to the Faculties of the University of Pennsylvania in Partial Fulfillment of the Requirements for the Degree of Doctor of Philosophy

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Se enim soni verborum quibus loquimur, pro tempore commutantur, cademque res aliter enuntiatur facienda, aliter facta, sicut ista ipsa duo verba quae dixi, facienda et facta, nec paribus morarum intervallis, nec iisdem vel totidem litteris syllabisve sonuerunt: quid mirum si alis mysteriorum signaculis passio et resurrectio Christi futura promissa est, alis iam facta annuntiatur; quandoquidem ipsa verba, futurum et factum, passurus et passus, resurrecturus et resurrexit, nec tendi aequaliter, nec similiter sonare potuerunt? Quid enim sunt alius quaeque corporalia sacramenta, nisi quaeam quasi verba visibilia, sacrosancta quidem, verumontamen mutabilia et temporalia?

Augustine, *Reply to Faustus the Manichaeans*, XIX, 16

Al, it's a grammatical world. I just live in it.

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My parents have been a constant source of support throughout my studies, and though they might have wondered whether I would ever make it out of graduate school, they can perhaps let out a sigh of relief now that the end is finally in sight.

This dissertation would not have been written without the help and support of all of these people, and it is with great appreciation that I acknowledge their help.

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ABSTRACT

RITUAL SYNTAX

Carl Andrew Seaquist
Guy Welbon

Frits Staal developed a theory of ritual in the late 1970s that is well-known to ritual studies scholars, though consistently misunderstood. Most attention has focused on Staal's claim that rituals are meaningless, since this seems contrary to most theories of ritual, and to professed beliefs of ritual practitioners. I show that Staal's treatment of meaning is more subtle than most readers allow, and I demonstrate that other theorists make similar claims, but my main focus is on another part of Staal's theory: the claim that rituals have the same formal structure, or syntax, as natural languages. Staal adapts to ritual an approach originally developed for language by Noam Chomsky, to the effect that ritual structure is sufficiently complex that it can only be modeled by what is known as a Context Sensitive Language. Seen in this light, Staal's theory is really a cognitive theory of ritual, in other words it is a theory of the mental qualities that are necessary for a person's actions to count as ritual actions. My final chapter therefore considers this theory in the light of recent, cognitive approaches to religion, especially the work of Dan Sperber and Pascal Boyer. Most work in the cognitive science of religion relies on methods from cognitive psychology, and Staal's theory is unique in presenting a computational model of ritual structure. It focuses our attention on the sequential ordering of the elementary actions that compose ritual sequences, and in the process it opens up a wide range of research programs for ethnologists and historians, as well as for ritual studies theorists. Staal's theory is based on data from the Vedic ritual tradition, especially the fieldwork he pursued on a performance of the Agnicayana in South India; in the process of examining Staal's theory, I consider a variety of topics relating to South Asian and Vedic ritual and grammatical theory, and I supplement this with a look at exempla from other ritual cultures. In addition to a thorough analysis and critique of Staal's theory, I provide the foundation for what I call a "distributional" study of ritual structure.
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INTRODUCTION

This dissertation focuses on a theory of religious ritual that Frits Staal began formulating in the 1970s and has since continued to develop. I believe that this theory is the most creative and potentially fruitful contribution to the theoretical study of religion that has been proposed in recent decades, and while it is always mentioned in surveys of all the recent trends in ritual theory (for example in Bell 1997, the best current survey of the state of the art now available), it has never been given the reception it deserves. Richard Payne and Roy Gane, two of Staal's own students, are among the only scholars who have attempted to actually use the theory in their own research. Unfortunately, Payne's application demonstrates that he does not understand the theoretical value of the theory; he uses it simply as a device for graphically outlining the sequential acts that constitute rituals, rather than as an analytical tool or as a theory with empirical implications, which thus can be proved or disproved. And Gane's contribution to this topic is limited to his unpublished dissertation and one journal article.

My dissertation is not an intellectual history: its goal is not to take Staal's theory as an object of study in its own right. Rather, the goal is to present a version of his theory, with which Staal might or might not approve in every respect himself; it seeks to preserve all the many, worthy insights of his theory while making the argument clearer and more consistent. Let me explain why such a study is necessary. Staal originally proposed his theory in two 1979 articles, "The Meaninglessness of Ritual" and "Ritual Syntax". These contained the core of the theory, but Staal continued to expand on these initial statements in books and further articles. In 1989 he published Rules Without Meaning, which was intended to be a relatively complete and comprehensive survey of his theory and its theoretical and empirical underpinnings.

But Rules Without Meaning is, I believe, a hard book for many people. It is long and yet arguments for individual claims are often presented in a cursory and incomplete manner; its tone makes it sound like an apology for its main thesis, whereas it really is a scholarly and studied approach to ritual; the first sixty pages range widely over philosophical positions that are probably neither familiar nor of interest to many of the intended readership (which primarily consists, I think, of those in
religious studies, Indology, and anthropology). Further, it is more a collection of earlier articles, partly rewritten to present a continuous argument but still structured largely like its original sources, and thus not a very novel presentation of arguments that had already been made in earlier publications. The book therefore does not fill satisfactorily the purpose for which it seems to be intended: to present its theory in a clear and accessible way to those grounded in any one of the many disciplines on whose terrain it impinges.

No doubt the version of the theory presented here will suffer from some of the same failings as *Rules Without Meaning*, despite my best efforts, and probably from others as well. I do make a concerted effort, however, to formalize an argument that I believe is at the core of Staal's view of ritual, but which is largely hidden beneath a surface of complex details. Staal presents a number of arguments for his position, but they are not unified into one overall pattern of argumentation; rather, they are built up almost at random, without any good indication of how they relate to one another. In this dissertation, I have tried to collect the various arguments that I find in Staal's work. Then I judge their relative merits, determine which to present as key arguments, and which to cast in supporting roles. My dissertation therefore presents the coherent pattern of argumentation that I have found lacking in *Rules Without Meaning*.

Let me now clarify some of the terminology I have already started using: I hold that Staal is proposing a *theory* of ritual, which can be formulated by three interrelated *theses*, each of which are substantiated by *arguments*; these individual arguments (plural) are organized to form one, overarching argument for the theory as a whole. *Empirical support* for the theory comes primarily from Vedic ritual, though empirical data could be used by other scholars either as *applications* of the theory (i.e., to illustrate the claims that Staal makes), or to help *confirm* or *disconfirm* his claims. This terminology should clarify how the various parts of the theory relate to each other, for example which arguments support which theses. Staal seems to treat his theory as a unity, assuming each argument supports all parts of the theory. I suspect this is just a function of style and rhetoric, but it has still proved damaging to the reception of his views. Appropriately, perhaps, his theory has thus been received as a unity, and this is perhaps one reason why it has been treated so harshly: when critics have taken issue with one
aspect of the theory, they have summarily dismissed the whole of it. (This claim will be supported
below, in my survey of some critical reactions to the theory from scholars in several different fields.) I
myself have severe reservations about what I refer to as the "ethology thesis", and I propose that the rest
of the theory can do without it. If I treated the theory as a unity, the problem with this one thesis would
of course present a much greater problem for the entire theory.

Also, I refer to the theory I am presenting as Staal's theory (sometimes simply the theory),
even though Frits Staal might himself disagree with some of what I say. This is to emphasize that I am
not trying to present a new and original theory of ritual, but am clarifying and supplementing a theory
that is already out there for scholars to use and study. So by "Staal's theory" I mean not the theory that
Staal himself intended to present, but rather the theory that I find in his writings. An analogy would be
the theory of Kripkenstein, so called because Saul Kripke found it in the writings of Ludwig
Wittgenstein (Kripke 1982), though he admits that it might not be identical to Wittgenstein's own views.

My major contributions are, I believe, four: first, the systematic presentation of Staal's theory,
including the analysis of the theory into three distinct but related theses; second, my presentation of a
typology of ritual meaning; third, my criticism of Staal's work in ritual syntax; and finally, the
connection I draw between Staal's theory and other cognitive theories of ritual. As the title of my
dissertation indicates, I view the last two as the most parts of my project, though the first two may have
the most immediate impact on ritual theory. I will attempt to state the basic theory in Chapter 1, with
additional clarification, evidence, and arguments being held back until later chapters.

Towards the end of the dissertation I focus on prospects for future research. Briefly, I see
three general questions coming out of the present study. To answer these questions will require the
assistance of specialists in different religious traditions. From the perspective of historians and
ethnographers of religion, these are questions that can be taken into the field, or that can be asked of
written texts. From the perspective of the theoretician of ritual, it is only when specialists in diverse
traditions weigh in that we will know how powerful this theory really is, because only then will we
know how well it covers relevant empirical data. So I see Staal's work as the beginning of a long and
complex research project, which will require the assistance of scholars in many different fields. My
hope is that by clarifying the nature of his argument, I will facilitate the sort of dialogue that will be required for this fieldwork to be done. (1) The first question is: How much of ritual is syntactic? This question introduces theoretical problems that I seek to study further in future work, and the present dissertation therefore lays the ground for such inquiries. But this dissertation should provide enough guidance for specialists to begin to ask this question, at least in a preliminary way. (2) The second question asks: Could ritual meaning be compositional? Compositionality is implicit, I believe, in what Staal has written, and I make it a major element of my presentation of the theory. (3) The third question derives from the fact that the claims made by the theory are primarily psychological or cognitive. If Staal is right that a study of ritual syntax tells us about general human cognitive abilities, then the theory might be of use to scholars outside of religious studies, for example ethologists or computational linguists. The third question, then, is: How general are the cognitive abilities utilized in ritual thought and action? Theoreticians of ritual are more likely to deal with this third question directly, but the researches of specialists will be of great use to them. It is my hope that these questions, or at least the first two, will become a standard part of the toolbox in ritual studies, and will be institutionalized in sources like Notes and Queries.

If there should be any concern whether it is appropriate for a young academic's first monograph to attempt to improve on the work of such a notable scholar as Staal, I can only refer to the "Preface" of Staal 1989: "My efforts are not completed but there is enough for others to continue, with or without me" (xiii). Hopefully the current study will provide the impetus for people to take a fresh look at a theory that has now become a received part of the discipline, and it will encourage both Staal and others to dispute or correct aspects of my version of this theory so that we might better understand some general properties of certain types of ritual.

As for the structure of the dissertation, the core of the theory is presented in Chapter 1. The Meaninglessness Thesis is considered in more detail in Chapter 2, and the Syntax Thesis in Chapter 3. The final two chapters round out the discussion by considering problems that recur throughout the earlier discussion, such as the role of linguistic models and the nature of rules in the construction of ritual systems. The Conclusion focuses on prospects for future research. I hope this approach helps the
reader to see that Staal's theory is really fairly simple and interesting, and to appreciate its potential. I will make use of examples to help clarify individual points, which hopefully will make the going easier for those in religious studies and anthropology.

As for the rest of the Introduction: first I give a very brief overview of the theory, with the understanding that a fuller explanation will be found in Chapter 1. Then I present an argument that is intended to serve as something of an intuition pump: it is neither deductive nor inductive in form, but rather presents a plausible case for the theory to be considered in the rest of the dissertation. It is an argument that I think was very persuasive to Staal, and it therefore helps to ground the stricter arguments to follow. But since it is not, strictly speaking, an argument for the theory, I present it here rather than in the body of the dissertation. Consider it, if you will, as a reason to think at the outset that the theory itself is a plausible one.

After this I will consider the reception that Staal's theory has already received. Although I am explicitly focusing on Staal's treatment, implicitly the history of its reception will occupy the place of silent interlocutor throughout my dissertation, and I think a few words about this reception will therefore be helpful at the outset. I will wait until the end of Chapter 1 to discuss the work of scholars who, in my mind, have appreciated some of the power and subtlety of Staal's theory, so my initial review of the reception of Staal's theory will focus on cases in which the theory was misunderstood. It is my hope that the present dissertation will help to counteract such misinterpretations. The Introduction concludes with a discussion of definitions, for those readers who think any theoretical treatment of ritual should begin with a definition of ritual.

0.1 The Theory in a Nutshell. Let me present now just a very brief summary of the main argument of Chapter 1, to tide the reader over until a proper exposition can be given. I view Staal's theory as consisting of three claims --- I will only consider the first two here, since the third is less important. First, rituals have a formal structure similar in important respects to the formal structure of natural language at the level of the sentence; thus we may speak by analogy of a syntax of ritual. Second, there is an important sense in which rituals are meaningless; this is the meaninglessness of ritual. The bulk of the present dissertation explains in more detail these rather cryptic remarks.
Criticism of this theory focuses primarily on Staal’s notion of meaningfulness, and, as I will demonstrate shortly, though this criticism is quite pervasive it is generally skin-deep. Staal speaks of rituals as being for themselves, and his approach is consistent with many scholars’ intuitions, reacting against the more traditional alternatives, that rituals are about religious doctrine, or social structure. In a loose and untheoretical sense, any talk of rituals as having function or significance, or as representing ideals described explicitly in statements of doctrine, is really talk that attributes “meaning” to rituals. The alternative to this is to say that rituals are for themselves. Staal explains this idea by comparing ritual to dance --- just like people dance because they find value in the act of dancing, so people perform rituals for the sake of performing rituals, and not for other reasons (ceteris paribus).

It is important that the second claim depends on the first: it is precisely because ritual has a syntax that we should ask whether this syntax determines a meaning. In natural languages, it seems clear that sentences get their meanings due in part to the formal relations between the words they contain, so the syntax of the sentence contributes to the resultant meaning. Thus, the two sentences “Chaucer predated Shakespeare” and “Shakespeare predated Chaucer” contain the same words but have different meanings. In English, word order indicates sentence structure (or syntax), and it is precisely this difference that accounts for the difference in meaning. It seems reasonable, if rituals also have syntax, to ask whether this syntax contributes to meaning the same way natural language syntax does. Thus Staal’s theory draws attention to two issues that generally go unrecognized in discussions of ritual: first, how rituals are done is very important to what they are. It happens, however, that the logic that governs what constitutes correct performance of a ritual is not well understood. In fact, this problem has hardly ever been examined in general treatments of ritual. Staal’s theory connects the problem of meaning to the problem of form: what parts go into rituals and how those parts are put together determines the significance of those rituals. Staal is not clear as to what he means by “for its own sake”. In fact, he argues that it is not necessary to clarify the notion. I propose that we understand it in one particular sense, namely that the meaning of a ritual is not a compositional function of the meanings of individual ritual elements.
The second issue that Staal's theory draws attention to is the problem of how people can be ritual agents. This problem may be stated as a question: What is it that allows us to perform an action so that it is a ritual? Staal's theory lends itself to a psychological answer to this question: just as successful language production requires us to follow the rules that constitute the grammar of a language, so successful ritual production requires us to follow the rules that constitute the grammar of a ritual system.

0.2 Vedic Ritual Specialists and Sanskrit Linguists. Since Staal's theory is based on his study of the Vedic ritual and grammatical traditions, some background is necessary at the outset.

0.2.1 Staal's Initial Argument. Staal presents a number of different arguments in support of his key claims, and the main goal of this dissertation is to disentangle these arguments and chart their interrelations. One of them is not a strictly deductive argument, but rather what I have recently called an intuition pump: it gives the reader some confidence that the argument that follows may be worth the effort that will shortly be expended on it. I present what I will call "Staal's initial argument" in the following terms:

(1) Modern linguistics is based on a scientific theory of language developed in ancient India, by a group I will call the Sanskrit Grammarians.

(2) At around the time of the Sanskrit Grammarians, another group of scholars existed, whom I will call the Sanskrit Ritualists. They developed a theory of ritual that was just as sophisticated and just as scientific as the linguistic theory of their peers.

(3) The treatises of the Sanskrit Grammarians and Ritualists make use of similar formal devices, which implies that language and ritual have certain common features, since both types of treatise present accurate descriptions of their subject matter.
(4) Modern theories of ritual are not nearly as sophisticated as modern theories of language. Perhaps scholars of religion would therefore profit from looking at the work of the Sanskrit Ritualists, and seeing if they could develop a general theory of ritual based on the work of their Indian predecessors.3

It should be noted that the Sanskrit Grammarians did not exactly develop a general theory of language. What they developed is a precise description of the Sanskrit language, which may be studied to reveal an implicit theory of language generally. Similarly, what the Sanskrit Ritualists produced was a set of manuals describing the practice of Vedic ritual.4 But Western linguistics in the late eighteenth century, when European scholars were first introduced to the Sanskrit Grammarians, was scarcely more advanced than it had been one or two thousand years earlier, whereas the last two hundred years have seen extremely rapid growth in our understanding of language. Unlike in the basic natural sciences, which began taking their modern form primarily as a result of developments within the Western tradition, it seems clear that the encounter with India was the catalyst in the development of modern theories of language. So the precise description of the Sanskrit language by the Grammarians may be seen as crucial to the development of more general theories of language, and there is the promise of similar developments in ritual theory. Of course, the only way to see if this promise can be fulfilled is to attempt to develop a modern theory of ritual based on the work of the Sanskrit Ritualists: there is no way to guarantee the success of this method at the outset. And this is precisely what Staal proposes to do.

In other words, Staal's initial argument gives us a reason to consult the Sanskrit Ritualists in hope of finding the inspiration for a general theory of ritual. Since ritual theory is currently in a very fragmented state, Staal's proposal could prove very beneficial, but clearly the value of the theory can only be determined as it develops. It gives us only hope, but not confidence, at the outset. The initial argument is relatively simple, and the thrust of the argument should be clear already. A little more discussion of the historical issues that this argument raises would perhaps be helpful: for those unfamiliar with the history of linguistics, this will provide essential background for later chapters; and
for those with some knowledge of the relevant history, it will help to clarify these brief remarks and head off misunderstanding. In the process of considering historical issues, I will also introduce some theoretical problems that sit at the heart of the Staal's overall theory.

It is worth mentioning that one previous attempt has been made to develop a general theory of sacrifice based on the Vedic sources, namely Mauss and Hubert 1964/1898. I view their work more as an historical curiosity than a currently vital theory. In any case, their approach is quite different from Staal's, and despite any virtues that Mauss and Hubert's work may possess, they are not particularly relevant to the current project.

0.2.2 The Historical Problem. Staal's initial thesis holds that the Sanskrit Ritualists might be able to provide modern ritual studies with the same kick-start that the discovery of Sanskrit and the Sanskrit Grammarians did for modern linguistics. The case Staal wants to make is dependent on two factors. First, it is not entirely clear whether the Western encounter with Sanskrit literature was sufficient inspiration for the development of modern linguistics, or whether the Sanskrit Grammarians in particular played a crucial role. Second, modern linguistics appears to have a strong rupture, roughly dividing the nineteenth from the twentieth century, and modern linguistic historiography says that the realizations that led to twentieth century linguistics were the results of internal advances within the Western tradition. Now, Pāṇinī implicitly recognizes a number of sophisticated meta-theoretical principles about language, which were only fully understood in the West in roughly the last half century. So one of two options is true. Either Western linguistics rediscovered these principles on its own, or else Pāṇinī continued to influence more recent developments in Western linguistics up to fairly recent times. Historians of linguistics currently hold to the first of these options, but I have seen some indications that perhaps the second is closer to the truth. I will therefore outline the historical context as briefly as I can, and give a few bits to evidence to indicate that Pāṇinī's influence on modern linguistics may have been longer and deeper than most people acknowledge. This digression is important to an understanding of Staal's theory for several reasons. First, it helps us understand his initial thesis, which in turn motivates all that follows. Second, it introduces the genres of Vedāṅgic texts that we will
encounter at a later point. Finally, it introduces the methodological issues that these texts introduce, issues which will be very important for our understanding of ritual. So this digression on the history of linguistics serves the same purpose as this Introduction generally: it introduces some of the issues that will recur at later points in my discussion.

0.2.3 Historical Context. It is an unobjectionable fact that the study of language in the West up until the late eighteenth century was primarily based on a foundation laid by ancient Greek and Roman scholars; further, it was the British "discovery" of India that began the rapid growth of linguistics as a discipline in the nineteenth and twentieth centuries. But what it was about traditions in India that proved so inspirational is not immediately clear.

Nineteenth century linguistics was basically just historical linguistics, and the commonly accepted story is that historical linguistics began with William Jones' realization that Sanskrit was historically related to Latin and Greek. Once Westerners began looking at it in this light, they began seeing systematic relations between words in the three languages, and therefore systematic relations between the sounds composing those words. Perhaps the greatest advance of nineteenth century linguistics was the discovery of sound laws, culminating in the Young Grammarians' credo (rather an overstatement, as it happens) of die absolute Ausnahmlosigkeit der Lautgesetze, "the absolute exceptionlessness of sound laws". When, over long stretches of time, a language changes in different places in which it is spoken until it has stopped being the language it once was, and has turned into several related "daughter languages", then the sounds in corresponding words of the daughter languages will correspond in a systematic fashion. This is the idea of sound laws. Thus, if we have a given Latin-derived word in French and Spanish, and if we know that that a descendant of that Latin word exists in Italian, we can guess fairly accurately at how it will be pronounced in Italian. Of course, it is a contingent fact whether the word has fallen out of use in Italian, perhaps replaced by a loan word from another language. But if the word does exist, we can predict the probable form it will take --- hence generalizations about sound correspondences between related languages are called laws. But to tell this story about the history of linguistics, we only need to mention the European discovery of the Sanskrit language. It is not necessary to discuss the Sanskrit Grammarians to tell this story.

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As for Indian linguistics: in perhaps the fifth or fourth century B.C., Pāṇini wrote a
codification of Indian thought on language up to his time. Though speculation about the nature of
language had gone on for centuries at that point (going back to the padapātha on the Rg Veda, perhaps
a half a millennium earlier), Pāṇini’s work was so successful that we have little direct evidence for the
work of his predecessors. After Pāṇini, however, thought on language continued, and other scholars
including Kātyāyana and Patañjali continued to make original contributions. The grammatical tradition
in Indian vernaculars is less well studied than the Sanskrit origins of the tradition, but recent articles in
the handbook Geschichte der Sprachwissenschaften (de Gruyter, 2000) by Shapiro and Chevillard
provide accessible summaries of the later Hindi and Dravidian traditions. It is Pāṇini and those
successors who concerned themselves with the linguistics of the Sanskrit language whom I call the
Sanskrit Grammarians.

It is well-known that the earliest European scholars of Sanskrit learned the language in the
traditional way, from Indian pundits and with the help of Indian grammatical texts. Also, all the major
nineteenth century "Orientalists" knew the work of Pāṇini. But historians of linguistics tend to tell the
story of twentieth century linguistics as constituting a great rupture with the past. If the nineteenth
century was an age of diachronic (historical) linguistics, the twentieth century was a time of synchronic
linguistics, leading up to Noam Chomsky’s Transformational-Generative Grammar (TGG). And the
keys to this rupture can be found in two tendencies in American linguistics: the study of Native
American languages, which provided the same impetus for theory change as the discovery of Sanskrit
had a century earlier, and the adoption of mathematical formalisms. The former tendency derived from
the work of anthropologists, notably Edward Sapir and Franz Boas, and linguists such as Leonard
Bloomfield; whereas the latter is particularly associated with Zellig Harris, Chomsky's teacher and the
founder of the first modern linguistics department in the United States, at the University of
Pennsylvania. If we wanted to find the influence of Pāṇini on twentieth century linguistics, we would
have to reexamine this crucial period leading from purely descriptive anthropological linguistics to the
theory-heavy work of Chomsky. Unfortunately, the history of American linguistics in the first half of
the twentieth century is poorly understood. In part this is because the period is still very recent and the methodological issues introduced then have not yet been fully resolved, and in part because the history we have is largely written by the "winners" of the major debates of the period, in particular by Chomsky and his partisans (for a discussion of how Chomsky's recollections of his own intellectual development changed over the years, see for example Murray 1994, 225-237).

Let us simplify the story and consider only Bloomfield and Chomsky to develop a model of what a history of twentieth century linguistics should look like. The author of the seminal 1933 book entitled simply *Language*, Bloomfield is widely recognized as the chief figure of developed American anthropological linguistics in the first half of the twentieth century. When Chomsky entered university in the 1940's, Bloomfield represented the orthodox tradition, against which the new generation would define itself. Bloomfield's book is divided into two parts: the first treats descriptive, or synchronic, linguistics; the second, historical linguistics. In a sense it can be viewed as a transition between historical linguistics and TGG. It tries to look back to the past in its second part, but in its first it considers notions like Immediate Constituent Analysis (IC-analysis) that would serve as foundations for linguistics in the latter half of the century. Chomsky, in turn, is best known for several things: his mentalism (i.e., his rejection of behaviorism); his notion of a generative grammar; and the formalisms of his early works, especially the notion of the transformation. None of these is entirely original to Chomsky, and the theory for which he is best known did not come fully-formed at once --- it evolved over a period of perhaps two decades. What was important about Chomsky's work was the way he put various pieces together into a system, and the way he built a school around himself. A proper (though simplified) history of this period would have to determine the essential elements of Bloomfieldian and Chomskian linguistics (I have indicated some of these), determine which elements of Chomsky's thought are directly dependent on Bloomfield's, and then account for the remainder in terms of either secondary strands of influence or creativity on the part of Chomsky. We can look for traces of Pāṇini's influence either in Bloomfield's work or in secondary influence on Chomsky.

As it happens, we now have some idea of the influence of Pāṇini on Bloomfield. Rogers 1987 provides the best general discussion. Emeneau 1988 adds a likely hypothesis concerning when
Bloomfield might have first studied Pāṇini; previously, Rocher 1975 had reviewed evidence for the view that "Pāṇini's example was largely responsible for Bloomfield's interest in synchronic and descriptive linguistics" (26). Suffice it to say that Bloomfield was originally trained as an Indo-Europeanist, but later became interested in Malayo-Polynesian languages thanks to an encounter with a speaker of Tagalog, and eventually turned to American languages, especially those in the Algonquian family. His theoretical contributions therefore came in the application of skills developed in the study of Indo-European languages, but he applied those skills to languages that had scarcely been studied by linguists. Traditional grammar, based on the Graeco-Roman tradition, presumed certain parts of speech and certain grammatical relations between those parts; it therefore indirectly implied that all languages were structured on the model of Indo-European languages. Pāṇinian grammar, though similar to the Graeco-Roman tradition in being based just on one type of language, differed in that it contained more insights regarding the nature of language in general. A case could be made that it is these insights that helped Bloomfield escape the straightjacket of historical linguistics, and that without his knowledge of Pāṇini he would not have laid the foundations for later developments in synchronic linguistics.

Despite Bloomfield's knowledge of Pāṇini, it has not yet been proven that Pāṇini served as Bloomfield's inspiration for, as an example, IC-analysis. Seuren 1998 finds the origins of IC-analysis already in the European tradition in Thomas of Erfurt (c. 1300). Thus it remains an open question to what extent modern synchronic linguistics is indebted to Pāṇini through Bloomfield.

As it happens, though, Bloomfield was not alone in his knowledge of Pāṇini, so a full understanding of Pāṇini's influence must consider a wide range of early twentieth century linguists. Consider Chomsky's mentor Harris, and in turn his mentor James Montgomery. Both Chomsky and Harris were educated entirely at the University of Pennsylvania, and Harris remained there his entire professional career. Harris is credited with having founded Penn's Linguistics Department in 1947. Previous to that year, linguistics had been taught in two departments: Oriental Studies and Indo-European Philology. Both departments offered courses in historical linguistics. Harris was trained in Semitic Philology by Montgomery, and his earliest publications were in Northwest Semitic historical
linguistics, especially Ugaritic and Phoenician. But in 1940 Harris began offering a course, cross-listed with Anthropology, in the "Problems and Methods in the Study of Unwritten Languages". By 1945 this one course had transformed into a small series of courses in general and descriptive linguistics: "Analysis of a Near Eastern Language Learned from a Native Informant", "Research in Descriptive Linguistics", and "The Techniques of Linguistics". At this point linguistics courses were still part of the Oriental Studies Department, but by 1948, when Linguistic Analysis was a separate department (though drawing its staff still from philological departments) such courses as "Linguistics and Mathematical Logic" and "Linguistic Analysis of Culture" had been added to the curriculum.

Further, "Analysis of a Near Eastern Language Learned from a Native Informant" had been transformed into the more general "Informant Work" course. While Harris was indeed the instigator of the new program in linguistics, and both he and Chomsky began their studies in Semitic languages¹⁰, its faculty was from the beginning drawn from Oriental Studies (E.A. Speiser), the former Indo-European philology program (W. Norman Brown, George Seiver, Alfred Senn, Otto Springer), and Anthropology (Frank Speck). The close connection of Indo-Europeanists and Semitic scholars both before and during the early history of Penn's Linguistics Department makes it likely that Harris and Chomsky could have encountered Pāṇini themselves, albeit indirectly through discussions with W. Norman Brown or Roland Kent, who taught the Indo-Iranian languages for many years. Brown did not publish on the Sanskrit Grammarians himself, but both he¹¹ and Kent were readers of Isidore Dyen's 1939 dissertation, The Sanskrit Indeclinables of the Hindu Grammarians and Lexicographers. This work treats not only Grammarians including Pāṇini, Patañjali, and Candragomin, but also earlier linguistic thought, collectively called "Vedic Studies" by Dyen, such as the Prātiśākyas, the Nirukta, and the Brhaddevatā. Chomsky was awarded his B.A. only 10 years after the publication of this dissertation, in 1949, so it seems not unreasonable that discussion of the Sanskrit Grammarians was in the air among linguists at Penn during his time. Like the influence of Pāṇini on Bloomfield, this is not definitive proof, but it does indicate the possibility of a connection stronger than most linguistic historians will allow.
0.2.4 Pāṇini. Having discussed the modern tradition in linguistics and having looked at its possible debts to the Sanskrit grammatical tradition, I now survey the work of Pāṇini as the chief representative of that tradition\textsuperscript{12}, and then in the next subsection briefly compare this work to the ritual sutras. According to Staal's initial argument, it is the success of and similarities between grammarians and ritualists that underwrites our use of the latter as models for our own study of ritual --- and the development of a modern ritual theory is the ultimate goal of this whole inquiry. Therefore in this Introduction I outline some of the key features of grammatical and ritual theories of ancient India, discuss some of the similarities between them, and consider the significance of these similarities for our purposes.

In the words of Kiparsky 1994, 2918, "modern linguistics acknowledges [Pāṇini's grammar] as the most complete generative grammar of any language yet written, and continues to adopt technical ideas from it." The core of this grammar is a set of about 4,000 rules that define the combination of sounds into words and words into sentences\textsuperscript{13}. In other words, the grammar defines what combinations of elements form acceptable strings\textsuperscript{14} within the Sanskrit language. Appended to this Āṣṭādhyāyī are two lists of elements --- the Śivasūtra or Aksarasamānnāya, which contains phonological segments; and the Dhāipāṭha, which contains the verbal roots of the Sanskrit lexicon, with annotations indicating the morphological and syntactical properties of each root --- and a list of items that are associated with particular operations, the Gaṇapāṭha. Thus the subsidiary lists provide the elementary units of the language, and the sutras of the Āṣṭādhyāyī provide the rules that allow these units to form longer, meaningful strings.

I should perhaps begin by explaining the term 'sūtra', which I have just used. A sūtra is a literary genre, a form of expression known for its extreme concision. To give one example, rule 8.1.1 of the Āṣṭādhyāyī translates literally "of the whole, two" (sarvasya āve). This means that in Sanskrit there is a class of words that can be repeated for various effects, which will be described in more detail in rules immediately following. Thus\textsuperscript{15} sā pacati pacati (literally, "That [woman] cooks cooks") means
"That woman is constantly cooking"; and pari pari trigartebhyyo vrṣṭo (literally, "Around around Trigarta [it rained]") means "It rained all around Trigarta [but not in it]." Katre 1987 explains 8.1.1 by translating it as "two expressions occur in the place of a whole sequence." In other words, in Sanskrit the repetition of a word in the syntax of a sentence can indicate repetition of the act or idea indicated by the word. To get this meaning out of "of the whole, two" requires that the reader understand not only the language in which the Āṣṭādhyāyī is written, but also the relevant genre conventions. These conventions include: (1) a rule like this introduces a series of subsequent rules, to all of which it applies as a general heading. (2) The case endings of words contained in rules (for example, -asya in sarvasya, which in normal Sanskrit indicates the genitive or possessive case, i.e., "of ___", in Pāṇini's technical language indicates substitution) indicate stereotypical functions. In this case, the genitive ending followed by the nominative indicates that two words (in this case, the doubling of one word) can be used to represent the whole content of an idea. (3) Technical terms refer to their meanings, whereas non-technical words refer to their form. In this example, both words used in the rule count as technical words: thus, 'whole' means the whole expression. In rule 8.1.9, ekam bahuvrīhitvat, bahuvrīhi is a technical term, but eka (one') is not. This rule says that when the word eka is repeated, it functions like a bahuvrīhi compound in form but not in meaning. Thus the word eka (one') is used in the same way as described under 8.1.1, but as 8.1.9 tells us, its form is different than what we would expect from 8.1.1. Thus ekam + ekam + akṣaram + pāṭhati is written ekaikam akṣaram pāṭhati (the two eka-s are written like one single, compound word rather than separately) but the meaning is as stated before in 8.1.1: "he recites each and every syllable."

It should be clear from this example that often a longer, more explicit version of the original rule cannot quite describe the sense of the rule. In order to explain the rule to a modern audience, Cardona and Katre resort to longer statements of the original rule, examples, and explicit references to other rules that are implicit in Pāṇini's text (in this case, Katre explains that this rule applies to all the rules through 8.1.15). It is in this sense that sūtras are highly concise. Because of this concision commentaries are often necessary, and when authoritative commentaries are not available, even
scholars can disagree how to interpret particular statements. In the case of the Sanskrit grammatical
texts, the chief commentators lived several centuries after Pāṇini, and it is possible that the
interpretations they give are not always quite in line with the original intent of particular rules.

It should also be clear that there is really no way to discuss the grammatical sūtras without
going into some detail regarding the technicalities of the Sanskrit language. Readers unfamiliar with
the grammar of Indo-European languages will probably find the examples I gave above to be rather
difficult to follow. This is a problem for which there is really no solution, since the grammatical
treatises are highly technical. The same situation will obtain with the ritual sūtras, because the Vedic
ritual system was itself highly complex and in many respects unlike, for example, the ritual systems of
Judaism or Christianity. I hope that even if particular examples are difficult, the basic idea will come
out from the examples in combination with my explanations of them. In the case of rituals, I will be
able to give examples from different and perhaps more familiar traditions, and this should help matters.

Pāṇini’s grammar, then, consists of lists of basic elements and rules for combining these
elements. The rules themselves are of four major types: definitions (saṃjñā), metarules (paribhāṣā),
headings (adhikara), and operational rules (ūrtti). We saw an example of a heading in Aṣṭ. 8.1.1, which
both serves as a heading and states an operation. Aṣṭ. 8.1.16 (padasya, "Relating to Padas:"") just serves
as a header. It is the operational rules that do the real work of the system. They perform operations on
strings: in other words, Pāṇini’s grammar provides rules for changing one string of sounds under certain
conditions, so that a different string results. Kiparsky identifies four types of operational rules
(replacement, affixation, augmentation, and compounding); Cardona treats two others (single remainder
and doubling) as distinct types. In any case, operative rules are used to transform one string into
another string. Unlike in TGG, an attempt is not made to treat synonymous expressions as mutual
transformations of one another. Instead, operative rules are used to transform elementary units so that
they may be combined into strings.

Of the other basic rule types, headings are important for a text that was designed for oral
transmission, to aid in communicating the overall structure of the text. Key terms are defined within the
work, and terms that are not defined are taken as commonly understood. Metarules, however, are particularly important, because they allow the operational rules to be grouped into types --- and this has implications for our understanding of the object of these rules (the Sanskrit language) itself. Consider, for example, Ast. 1.3.10 and 1.1.50 (see Cardona 1988/1997, 52-54). The former rule asserts that if two lists with the same number of elements are given, a rule that relates the two lists will take the first element of the first list as relating to the first element of the second list, the second to the second, etc., in a one-to-one relation. For example, the following lists could be used in such a rule:

Class G: \text{ i u r l }

Class D: \text{ y v r l }

This rule is relevant, for example, in discussions of the phenomenon of external sandhi (Ast. 1.1.45). Consider the words: \text{ tasmād ity uttarasya}. The three words contained in this sentence are \text{ tasmāt, iti, and uttarasya}. When a word ending in word final -i is followed by a word beginning with initial u-, then (ceteris paribus) the final -i is pronounced as though it were a -y. The result is that a unit containing six syllables (i-ti-ut-ta-ra-sya) is pronounced as though it contained only five (i-tu-ta-ra-sya). By identifying the two class of sounds above (Class G and Class D), this rule (the one describing the replacement of -i with -y) and three others like it (-u changes to -v, etc.) can be stated with just one rule. It might seem that replacing three rules with one rule plus two sets of objects is a negligible advantage: it replaces four objects with three. But theoretically, as will become apparent, the advantage is indeed great, because the discovery of more general rules which cover and explain more specific rules increases the explanatory power of the overall theory. In terms that Staal would approve of, this makes the theory more "scientific". In modern notation, we could write this rule as follows:

(1) $\Gamma \rightarrow \Delta | -\Gamma V-$  

\text{(Ast. 1.3.10)}
assuming the following classes of sounds:

\[ \Gamma = \{ i, u, r, l \} \]

\[ \Delta = \{ y, v, r, l \} \]

In other words, when one member of the class \( \Gamma \) ends a word, and a vowel begins the following word, then the \( \Gamma \) sound is replaced by the corresponding \( \Delta \) sound. (In my notation, the single bond ( ) indicates that what follows describes the environment in which the replacement occurs. If this statement of context was omitted from the rule, then Sanskrit would contain no -i- or -y- sounds, because they all would be universally replaced.) Of course this rule does not apply universally, but only "everything else being equal", or (using the Latin) "ceteris paribus". The exceptions would be spelled out in additional rules that follow this one, and describe more restrictive environments than this rule.

Now, such a rule makes sense in the context of a sūtra text, given that the sūtra genre seeks the greatest possible concision, but more importantly it increases the explanatory power of the grammatical theory. For a similar example, consider Aṣṭ. 1.1.50. This rule states (in the translation of Cardona) that "if there is a choice of replacements to occur in place of a substituend, that one occurs which is most similar to the substituend" (stāne’ ntaratamaḥ), and applies when referring to two lists with unequal numbers of elements. For example, a one-to-one mapping cannot be made between the following two classes of elements, since \( \Xi \) contains six elements and \( \Lambda \) only five:

\[ \Xi = \{ k, kh, g, gh, ṣ, h \} \]

\[ \Lambda = \{ c, ch, j, jh, ā, ī \} \]

However, once "most similar" is defined for this context, then a replacement rule can be defined over these elements as in the previous example.
The sounds \( k, kh, g, gh, \) and \( n \) are all gutturals, and the sounds \( c, ch, j, jh, \) and \( \bar{n} \) are all palatals --- gutturals and palatals are natural classes, defined by the way in which the mouth makes the sounds and by the distinctions that are significant in the Sanskrit language. "Most similar" can be defined in terms of two other natural distinctions among vowels, voicing and aspiration, as follows: \( k \) and \( c \) are both voiceless and unaspirated; \( kh \) and \( ch \) are both voiceless and aspirated; \( g \) and \( j \) are both voiced and unaspirated; and \( gh, h, \) and \( jh \) are all voiced and aspirated. Thus we can state a rule for the replacement of any sound in the class \( \Xi \) by a sound in class \( \Lambda \), as follows (I omit here details of the context, just indicating by an ellipsis that some context must be supplied):

\[
(2) \quad \Xi \to \Lambda \mid \ldots
\]

\( (\text{A\textit{ṣṭ.} 1.1.50}) \)

The important point here is that \( \text{A\textit{ṣṭ.} 1.1.50} \) does not provide arbitrary substitutions, but rather makes substitutions between classes that are systematically related to one another; and these classes are what I loosely described as "natural". Let me expand on the brief characterization I gave for what "natural" means in this context. Voicing, aspiration, and so on are in a sense "real" categories: they describe objective properties of the sound in question, independent of the language to which they belong. No language makes use of all the objective phonetic distinctions that humans are able to produce, however, so in a given language only certain distinctions are relevant for distinguishing sounds (phonemes) within that language. The relations between the sounds \( k, kh, g, gh, \) and \( n \) and \( c, ch, j, jh, \) and \( \bar{n} \) are natural because they are based on objective differences, and they are systematic within Sanskrit because of the overall phonological structure of the language.

0.2.5 Grammarians and Ritualists. Like the \( \text{A\textit{ṣṭādh\textit{yā}yī} of Pāṇiṇi, there is a class of texts that describe the formal properties of the Vedic ritual system, the Śrauta Sūtras}^{19}. There are problems dating any ancient Sanskrit work, but it seems clear that the oldest ritual sūtras precede the oldest grammatical sūtras, and that some features familiar from Pāṇiṇi’s work were developed in order to
describe Vedic ritual, including: the sūtra style itself; the use of formal, ordered operational rules; and metarules. While the similarities between these two types of work have long been known, the implications of these similarities are still open to debate. There are also differences between the use of rules in these two genres, however, and the differences between language and ritual are liable to be reflected in differences in the rules describing them. A theory that seeks to come to terms with ritual and its relations to language may take its starting point from these similarities and differences in the rules that govern them: if we are to follow Staal's suggestion of seeking to understand ritual by looking at the Vedic texts that describe Vedic ritual, then we are ultimately seeking a characterization of ritual systems based on the nature of the rules that best describe and explain those systems.

To give for now but one example (Renou 1941-2, 143-4): the word prakṛti occurs in both sorts of text. In ritual texts, it contrasts with vikṛti as archetype to ectype. For every type of ritual, the texts present one particular ritual as the archetype, and describe similar rituals (ectypes) by the ways in which they differ from that archetype --- this is in keeping with the concision found in sūtras, and it points (potentially) to underlying patterns in the ritual system itself. In grammatical texts, prakṛti contrasts with pratyāyā as base to affix. Sanskrit nouns are formed by adding a suffix indicating case, number, and gender to a nominal base. Sanskrit verbs add prefixes and suffixes indicating person, number, tense, mood, and voice. In other words, most words in Sanskrit are formed by adding affixes (prefixes and suffixes) to a base; the base gives the basic meaning of the word, and the affixes provide information about how that meaning is used in its sentence, i.e., how it relates to other words and their meanings. Clearly the two senses of prakṛti are related, as is to be expected since it is a single word used in different contexts. We find rituals or words that are similar, in that they derive finished forms (rituals in one case, words in the other) from a common core, but which are formed differently from this core: for example, the name of the god Agni will have the form Agniḥ when it is the subject of a sentence, but Agnim when it is the object. Similarly, in two versions of a given ritual, most of the elements are the same, and their order is fairly constant. A handful of elements might differ between rituals of the same type, though. This can happen when different gods are worshipped: in that case, the
name of the god will differ, though the prayers offered to those gods will otherwise be exactly the same (see Chakrabarti 1980, 98-99 and 135). Note that since prayers are both parts of rites and also forms of language, we find in this example that the name Agni is both a base (from a grammatical perspective) and a variable element (from a ritual perspective). Thus the base is Agni- in the grammatical situation, and the prayer (or rather the rite containing the prayer) in the ritual situation; the variable element is the ending (-ḥ or -m) in the grammatical situation, and the name of the god (e.g., Agni) in the ritual situation.

Similarities and differences will be a major theme of this inquiry, since Staal's theory seeks to understand ritual on the model of language.

0.3 Reception of Staal's Theory. It is customary to begin a dissertation with a review of recent scholarship on the question at hand. This often is done simply to demonstrate that the author has done his homework, but in the current case there are other reasons for beginning with a substantial chunk of, admittedly, rather dry material. This gets to the heart of why the current dissertation is this dissertation and not some other one instead. I have always felt that Staal's argument is more powerful, and more significant, than most scholars have acknowledged it to be. In fact, the theory is cited frequently in the literature on ritual, but it seems clear that most people read Staal's main publications on ritual either carelessly or unsympathetically: they latch onto one or two themes, such as the claim that rituals are meaningless or that ritual precedes language, and do not try to fit this claim into a larger framework that seeks to render the claim credible. Yet these are scholars who will carefully work through detailed philological and historical analyses of the Śatapatha Brāhmaṇa or the Hebrew scriptures, or works by Durkheim or Foucault. To my mind, then, a survey of previous reactions to Staal's theory is the best justification for the labor I will do in the bulk of this dissertation. Only when the reader sees how misunderstood the theory has been will the work of recreating that theory seem worth the effort.

The issues that Staal's theory highlights may seem somewhat obscure at first, and the best way of getting a grip on them is to return to them several times. I will also refer back to this part of the Introduction in subsequent chapters, when I return to issues addressed by one of the scholars discussed.
here. Please note that this section aims to give a representative sampling of the most significant reactions of Staal's work, and is not intended to be a fully comprehensive literature review.

The structure of this section is as follows: I begin with a paper by a rhetorician, to illustrate that Staal is read by scholars in a wide variety of disciplines, then cite one paper by an anthropologist that is intended to directly refute Staal's thesis. After this I move on to scholars operating within Religious Studies: Bell's survey of ritual theory, three contributions to a symposium on *Rules Without Meaning* in the journal *Religion* (Grapard, Mack, and Strenski), and finally articles by Indologists Hans Penner and Brian K. Smith. If there is one theme running through this survey, it is this: "*Rules Without Meaning* is . . . so radical a work that I fear many of the critiques leveled against it will simply be beside the point" (Strenski 1991, 219). This section, then, is intended to provide a review of criticism of Staal's theory, including both positive and negative evaluations. At the end of Chapter 1, after I have laid out Staal's theory in more detail, I will present a review of another type of literature, those works that accept Staal's basic principles and seek to build on them. Unfortunately little has been produced in this vein; there I will consider the dissertations of Payne and Gane.

### 0.3.1 Michael Carter

I take the main audience for Staal's work on ritual to be scholars in religious studies, anthropology, and Indology, and the scholars with the best understanding of the theory do tend to come from these fields. However, I wish to begin by considering the use made of the theory by scholars in fields relatively far from these, to indicate two things: first, how widely Staal's major publications are read, and second, to point to some widely held misunderstandings of what he actually says. I begin therefore with Carter 1991. Carter is interested in Greek epideictic oratory, in other words set speeches intended not for use in a courtroom but rather for literary consumption. In Athens (and also in the literary society within the *Iliad*) the only practical function of oratory was political or forensic, yet many of the extant texts we have are from other contexts. Carter recognizes the analogy between the non-pragmatic nature of such speeches and of religious rituals, for in the latter we encounter many seemingly practical actions bereft of a context that normally would justify their performance: for example, in many cultures statues representing a god are offered food, or clothed, or have their feet bathed, yet statues need neither food nor clothes. He therefore looks to ritual theory for
models that can be applied to the study of epideictic rhetoric. He introduces his discussion of Staal 1979a with these words (211):

In modern use, ritual often carries the connotations of empty, mindless, even neurotic behavior. It is eccentric, set apart from the ordinary, the purposeful, and the meaningful. Fritz Staal [sic], a religious anthropologist [sic], supports this notion of the inherent meaninglessness of ritual.

A footnote indicates that he sees this "modern use" as deriving from the Protestant criticism of Catholicism as "popery" and "mere superstition" during the Reformation. This criticism is certainly a historical fact, but it seems wrong to attribute this sort of prejudicial theology to Staal. He then goes on to cite "a revolution in the rapidly growing field of ritual studies in which Staal's idea of the meaninglessness of ritual has been rejected" (212). This scholarship, he says, attributes "value" and "power" to ritual, and the implication is that Staal's position is that ritual has no value to people, in other words that "meaningless" means "worthless". This goes well beyond anything Staal ever claims, yet many people seem to read him this way. I think Staal's own rhetoric, along with simple carelessness on the part of readers, is the main source of this misunderstanding, so in Chapters 1 and 2 I present a typology of the chief senses of "meaning", showing in turn how each appears in Staal's theory. Thus "meaning" as "reference" is distinguished from "meaning" as "value", and such confusion is avoided.

When one looks at Carter's sources for the "revolution" in ritual theory that rejects Staal's theory, two trends may be observed. First, half of the citations are to work published before 1979, so this revolution is clearly not an explicit rejection of Staal's theory. Second, most of the journals Carter cites are popular publications that aim for a wide audience or theological journals (Zygon, Man and World, Worship, Journal of Psychology and Theology). When he does use Religious Studies journals, his choice of articles is irregular. Of course, as a rhetorician he is coming to the subject of ritual as a non-specialist, so his preference for popular journals is understandable. One would hope that specialists in Religious Studies would present a better reading of Staal than Carter does.31

Without belaboring my reading of Carter's reading of Staal, let me just add this. From his reading of the Ritual Studies literature, he finds "three major functions of ritual. First, ritual generates a
kind of knowledge that is different from the knowledge generated by ordinary discourse. Second, ritual constitutes and promotes unity. And third, ritual offers its participants guidance in conducting their lives" (213). These all indicate trends that do exist in the literature. The first "function of ritual" is consistent with the view that ritual meaning is non-propositional, or that the force of ritual is primarily affective, or that ritual has its own internal logic ("defies logic", as he says, page 215). But if we say that ritual defies logic, are we not casting it as irrational and primitive, in keeping with the Protestant criticism of Catholic practice that Carter went to pains to reject? Thus, it appears that Carter's theory of ritual is just as prejudicial as Staal's. The brief and cavalier treatment he gives to Staal's theory is insufficient, and he should know better. The second "function of ritual" derives from Victor Turner; I would describe this as the claim that ritual is not so much concerned with meaning as it is with the social function that ritual serves in a society.24

0.3.2 Schröter. Next I wish to take an example from the journal Anthropos, a good German anthropological publication (Schröter 2000). Schröter, who did her fieldwork in Eastern Indonesia, seeks to demonstrate that the symbols and rituals of the Langa annual cycle have changed their meanings over time to balance tradition with modernity. She explicitly sets her theoretical stance in opposition to both Staal and Humphrey and Laidlaw 1994, "who provoked the scientific community with the argument that rituals are rules without meaning. Contrary to the cited authors, I argue that an analysis of rituals cannot focus on small ritual parts but has to take the whole social context into consideration" (463). As is common with ethnographies, the bulk of her paper (two-thirds, roughly) describes the data she collected in the field, and the local context for its interpretation: location and geography of her fieldwork, the traditional calendar and its annual cycle, and the role of foodstuffs in the diet and culture of the people, since food plays a dominant role in the rites of the annual cycle. When she reaches the theoretical portion of her argument, she makes two arguments intended to critique the "core theory" of Staal and Humphrey and Laidlaw.

First, she points to the fact that frequently ethnographers are puzzled by the inability or unwillingness of informants to answer the questions posed to them, questions often in the form of loose translations of the theoretical questions that the ethnographer brings with her into the field. Her
conclusion is that there is agreement among her informants as to what the "most important thing about the annual cycle" is, but considerable disagreement regarding "every small part and every symbol" (478). She views this as an indication of a "hierarchy of importance between the ritual levels", in other words "from my informants' point of view they had to teach me the most important thing about the cycle that was also the essence of every particular ritual". The result was answers that sounded to Schröter distinctly "Durkheimian", by which she means "the strengthening of society and the balancing of conflict potentials." Though she speaks of a hierarchy of levels of ritual, it sounds more like there is a complete rupture between the highest level of meaning, the 'Durkheimian' level, and all lower levels; the former are agreed upon and fixed through time, whereas the latter are subject to change over time. Presumably the degree of agreement on the meaning of individual rites and symbols is in inverse proportion to the rate of change of these meanings, though Schröter does not explicitly say this. For example, the major elements of the diet in Sara Langa are maize and rice, yet the dominant symbol of the annual cycle is the yam (or rather uwî, a term that covers both cassava and yam), which used to be commonly consumed but is no longer. Based on this fact, Schröter asks: "What does [uwî] stand for, when its meaning as a staple food has been lost? It is wrong to assume that it has no significance any more, rather, I suppose, its meaning has changed with the change of nutrition and the change of society." She seems to assume, in other words, that the symbolic meaning in a ritual context of foodstuffs in Sara Langa is just the social significance of those same foodstuffs at the present time. This is an unusual approach; for its justification we need to consider her second argument.

Schröter's second argument is based on the view that Indian religions, for social and cultural reasons unique to the subcontinent, possess certain common features not necessarily shared by religions elsewhere in the world. "For Indian religion meaninglessness and emptiness are central terms that characterize spiritual notions of the cosmos and the movement of all beings in it. Animals and humans, demons and gods are seen as entrapped in karma, the continuous wheel of suffering, illness, and death... Rites and mantras which are several thousand years old [therefore] need not have a meaning relevant to a modern Indian society" (479). It seems that Schröter's assumption that the ritual context of food in Indonesia is just the social role of those foodstuffs is based on her view of Indonesian society. I say this
because her implicit argument seems to be that Indian society works in one way, and Indonesian society in another. In Indonesia, the meanings of ritual elements are constantly updated to keep pace with social changes, but this is not the case in India.

Her treatment of India is essentializing\textsuperscript{25}, and her view of Indonesian society is likewise. At this point we get some manner of support for her differential treatment of South Asian and Indonesian societies: "Contrary to Indian societies East Indonesian societies do not cultivate life-negating models and the escape from the life cycle is never intended. Consequently people undertake great efforts to keep the cycle vital" (479-480). Of course the desire to escape the cycle of rebirths is not exactly life-negating in India: if the deeper reality is outside of time and decay, then presumably true "life" can only be lived when the veil of illusion is lifted. Schröter's phrasing indicates a distaste for what she seems to consider "Indian modes of thought" (not her phrase). In any case, even if her generalizations were true, her argument would not follow. Cosmological doctrines exist within the belief system of a people, whereas the principles that allow symbols to have meaning are objectively valid, and do not depend on any particular belief system. Consider what would happen if this were not true. Doctrines are expressed in words, yet the doctrines will be meaningless (without content) unless there is a system that allows words that express them to have meaning antecedently to their being used to express that doctrine. Let me put the case differently. It is possible that words get their meaning from the sum of the contexts in which they are used (holistically, as it is called). But the ability of words to express meanings does not depend on the contexts in which those words are used. Their content might depend on the contexts of their use, but not their ability to express meaning. Words have meaning from the fact that they are words, not from the particular meaning they have.

So Schröter's second argument is simply fallacious. But the first argument contains an intuition that could be correct: she holds that individual rites and "symbols\textsuperscript{26} do not have fixed meanings, and that the determinate meanings of ritual systems are functional (what she calls 'Durkheimian'). Unfortunately, she presents no argument for this claim, so it is up to her readers to accept or reject her intuition on the basis of their own experience and knowledge.
0.3.3 Bell. *Ritual: Perspectives and Dimensions.* Having considered the reception of Staal's theory in the literature of rhetoric and anthropology, I will now turn to the Religious Studies literature, which I begin with Catherine Bell's 1997 survey of the field of Ritual Studies (her earlier book *Ritual Theory, Ritual Practice*, I will discuss in section 0.4.3 rather than here). The first part of this book is devoted to a history of the study of ritual, and its categorization of theories is explicitly rough, since few scholars' work fits neatly in any one category, no matter how the categories are developed. The overall organization is generally historical: Bell devotes one chapter to trends originating in the nineteenth and early twentieth centuries, covering the Myth and Ritual School, the phenomenology of religion, and psychoanalysis. The second chapter covers trends deriving from any of the breeds of anthropological structural-functionalism, and the third chapter covers all later developments, such as processualism, symbolic anthropology, theories inspired by linguistics, performance studies, and theories of practice. Staal ends up, rightly, in the linguistics section of chapter three. Unfortunately, Bell's history seems rather poor when we look at its details. She traces the 'syntactic' approach to the work of J.L. Austin (68), even though his work on speech acts clearly falls within the orbit of linguistic pragmatics. Further, the chronological structure she adopts separates linguistic approaches from structuralist approaches, even though the dominant structuralist thinker in twentieth century scholarship on religion, Claude Levi-Strauss, based his theory explicitly on linguistic phonology (and especially Jakobson's work on distinctive features). So in her history Staal’s proposal that ritual is "for itself" ends up supporting Austin's claim that "the most essential feature of ritual language is that they [sic] are acts that do things, not mere bearers of information" (69). For Austin, the force of ritual language is not exhausted by the literal sense of words, so pragmatic considerations must also be considered to understand the full significance of ritual language. For Staal, ritual language qua ritual is meaningless, and any meaning it has qua language may be irrelevant to its use as ritual language. Austin is attempting to give a complete linguistic analysis, whereas Staal is interested in language precisely when it does not function as language. The contrast could not be stronger.

Later Bell more reasonably counts Staal among the scholars who have been influenced by Chomskian linguistics (70-1). She recognizes the important role that rule-following plays in Staal's
view of ritual, but misunderstands its implications. She asserts that "Staal also demonstrates that what makes an ordinary action into a ritual action is not primarily a change in its meaning but a rule-governed change in its form" (71). Since meaning for Staal is primarily a linguistic notion, it seems unlikely that any actions have meanings qua actions; in any case, rituals per se have no meaning whatsoever, so "change in its meaning" is misleading here. The example that follows makes it clear that Bell is thinking of rituals composed only of mantras (words, i.e., ritual language rather than the broader category of ritual in general). It is true that Staal emphasizes that ritual treats language as a structured object rather than as a carrier of meaning when it is used in ritual, and it is also true that he sees ritual language as forming a main component of ritual systems in general. But he does not argue that structural transformations make language into ritual; rather, when language becomes ritual, it is treated as a structured object of a particular sort. The difference is important, though it may seem subtle. Certainly Staal is particularly interested in cases where mantras are created from meaningful texts through transformations that make them meaningless as texts, but not all mantras are this way. Many remain meaningful as texts even when they are treated by ritual practitioners as ritual objects and not texts. This is true in some Vedic rites, and is even more obvious in the Abrahamic traditions, where religious texts are rarely dissolved into their phonological units, but maintain their ostensible propositional structure.

Bell is more accurate when she treats Staal's view of ritual study as a science: his claim is that ritual is like a language in many respects, but is not a language nevertheless. As my later discussion will indicate, Bell seems to understand Staal's theory better than many scholars, but she devotes very little attention to it anywhere in her writings. Of course, that does not save her from the mistakes just mentioned.

0.3.4 Symposium: Ritual As Such. Shortly after the publication of Staal 1989a, the journal Religion devoted part of one issue to discussing the implications of Staal's theory for the study of ritual, soliciting articles from Allan Grapard, Burton Mack, and Ivan Stenski; it then gave Staal an opportunity to respond. While the individual articles are brief and rather scattered, this symposium presents generally favorable reactions to Staal's work, in contrast to many of the other views that I
consider. A look at a few summary comments from these articles illustrates the level of praise found in them. The introduction to the symposium (Anon 1991) describes the book as "nothing less than radical --- some will say 'perverse' --- new thinking about the nature of ritual," and Mack 1991, 213 says that "Frits Staal's startling study of ritual ... renders a radical critique of traditional assumptions that still underly [sic] the study of (the history of) religion." He concludes: "To take Staal seriously would mean to rearrange and reassess most of what we have learned from the humanities" (218). Strenski 1991, 219 says: "Rules Without Meaning is a radical, powerfully argued, theoretical book. But, it is so radical a work that I fear many of the critiques leveled against it will simply be beside the point. Staal is not working within the existing paradigms of the theory of ritual ... he has reordered the whole menu."

When one goes beyond these initial statements of praise, all three articles sacrifice depth of treatment for range of coverage. Probably the authors felt that more focused but careful criticisms of Staal's theory would follow their brief reviews; unfortunately this has not yet happened. Here I will address just one argument or criticism in each article; some complaints in the Religion symposium are also to be found in Penner's article, and I consider these as I move from the former to the latter.

Strenski's two main criticisms relate to Staal's Ethology Thesis; the second is the more interesting. He begins by asking what happens if we take Staal's argument seriously as an evolutionary argument. Evolution must have chosen (meaningless) ritual form, but why? Evolution only chooses adaptations that are useful (or that supervene on useful traits), and what is the purpose of rituals that are labor-intensive but meaningless? None, it would seem. Evolution chooses meaning, Strenski argues. So mantras, if we accept Staal's thesis, must be "a throwback to an age long since bypassed by the course of evolution". Rule-governed rituals would then be only 'vestigial' (223). Better, Strenski proposes, to take the opposite course, and view mantras as "marked nonsense". In other words, we should suppose that the capacity to perceive meaning already was available to early man when rituals were first created: then rituals served as marked terms (in the linguistic sense) in contradistinction from meaningful activities like speaking. This approach adopts but reverses Staal's attitude. It assumes that etiology is relevant in discussions of ritual, but that ritual is posterior to normal language, and evolved in a way so as to differentiate itself from mundane language. A study of ritual therefore cannot help us
understand the origin of the human capacity for meaning. This is an interesting theory, and perhaps a more persuasive one than Staal's. Unfortunately, it is not clear how we could pursue this problem further, or what would count as evidence for one position or the other. The problem presented by Staal's Ethology Thesis remains a desideratum, and despite significant research in the last two decades on the evolution of language and other social forms among early hominids, scarcely any attention has been paid to the development of ritual or the implications of ritual for the development of human cognitive capacities.

Grapard, as scholar of Japanese religion, is particularly interested in Staal's approach to Asian traditions. He picks up on Staal's claim that "so-called religious systems enter into combination, not because of doctrinal comparative merits, but because of ritual" (Grapard 1991, 211). The traditional approach in comparative religion would look to syncretism in doctrine, and see ritual borrowing as a byproduct of agreement in doctrine, but this ignores the fact that, in Asia perhaps more than in the West, ritual traditions often operate largely independently of official statements of doctrine --- this would seem to constitute evidence for Staal's Meaninglessness Thesis. In other words, rituals seem to live lives of their own: they can migrate from one religious tradition to another, preserving much of their form but being reinterpreted to suit their new context, if they are interpreted in a systematic way at all. To make his case, Grapard points to syncretism between Shinto and Buddhist practices.

Mack 1991 says "what fun [!]" in reference to Staal's rejection of Eliade's program, which he views as covertly theological; he describes Eliade's program as "a theory of religion that celebrates Christianity surreptitiously by translating theological language into (semi-) neutral terminology" (215). The criticism that Religious Studies, and particularly the phenomenology of religion, derives historically from liberal Protestant theology and continues to bear the marks of this heritage is becoming widespread in certain circles.

One criticism is voiced, in different forms, by Mack, Grapard, and Penner. Mack and Penner both charge Staal with being a Romantic. Mack claims that Staal's theory is individualistic, and therefore does not acknowledge the social aspect of religion (216). He points out that Staal's theory does not have much to say about "eligibility, learning, status, bonding, preservation of tradition, linkage
to the ancestors and social identity", and asks "why not include them?" But he ends up admitting that Staal's theory does leave room for discussion of these topics; they are simply not germane to the problem that interests him. So this criticism is not really a criticism at all. Grapard makes a similar point when he observes that Staal's theory cannot account for power relations, which are essential constituents of social relations generally (209). But again, this simply demonstrates that Staal's theory is not a global theory of ritual; it does not explain all the properties of rituals.

0.3.5 Hans Penner. Penner 1985 is concerned with the problem of whether, and if so how, rituals have meanings, so he naturally uses Staal 1979a, published six years earlier in the same journal, as a starting place for his own inquiry. Penner's arguments are rather more technical than the others I will be considering here, since he has attempted in much of his work to bring a sophisticated knowledge of contemporary philosophy to bear on the arena of religion. As will be seen from my treatment of this one paper, he often fails in this goal.

At the core of Penner's argument lie two claims: first, that linguistics presents the best model for much theorizing in the study of religion, and second, that many scholars have gone wrong in assuming that the type of meaning found in ritual is reference. In the process, he paints Staal as a modern-age Romantic\(^\text{30}\) (5-7). As for the second claim: Staal does state that it is not incumbent on him to describe in detail what he thinks constitutes meaning, and he presents the outline of an argument in support of this claim. I agree with Penner in believing that this is a mistake on Staal's part, and that a clarification of what is meant by 'meaning' would help Staal's entire theory. This is just what I will present in my first two chapters. But Penner believes that for Staal meaning is just reference: he gives no arguments for this interpretation, but makes the global claim that "the grand assumption throughout the history of scholarship on ritual, myth and religious symbols is a simple one; if you want to determine the meaning of a ritual, myth, or religious symbol, then look for its reference" (4). If this has been universally true, he seems to think, then this is Staal's position as well. Penner is correct that even in language "a referential theory of meaning is inadequate" (2), but Staal is perhaps not the best object of his broad critique of previous research into religious meaning.
Penner next draws attention to Sperber's 1975 critique of Levi-Strauss. Although he says "historians of religion interested in symbolism should not overlook this text" (15, n. 11), he does not consider Sperber's positive contribution, instead using it only to argue that Levi-Strauss made the same mistake he is accusing Staal of, namely assuming that ritual meaning must be reference. In fact, Sperber presents a case for the meaninglessness of ritual (see Chapters 2 and 5). His argues against the usual view that symbols are relations between signifiers and signifieds, and instead proposes that symbolism is a cognitive mechanism. Data that the human perceptual system takes in from the world, he proposes, is first sent to a "rational" mechanism that tries to make sense of it, based on previous experience, general logical principles, etc. If the rational mechanism cannot make sense of the data, it sends it to a different unit of the mind, the symbolic mechanism, which utilizes different (and less logically rigorous) procedures to try to find an interpretation of sensory experience consistent with previous experiences as stored in memory. In an important sense, then, symbolic meanings are "absent": we only get interpretations from the symbolic mechanism when we cannot rationally associate sense data with a true meaning. So symbolic meanings are tentative and derive from a failure of the rational part of cognition. Sperber presents a truly revolutionary reinterpretation of what symbolism entails, though his exact proposals are quite tentative, and unfortunately few scholars have attempted to propose improvements on his work.11 Penner, while he does approve of Sperber's work, only takes from it the conclusion that "for Levi-Strauss if it were the case that in the last resort myths did not refer to something then they would be meaningless. Staal seems to agree with this position" (5) with regard to the meaning of rituals. Penner ignores the fact that symbols are also, in an important sense, meaningless for Sperber, and consequently does not tackle Sperber's challenging arguments in support of this position.

Let me now consider Penner's first claim, namely that linguistics presents the best model for much theorizing in the study of religion. Referring to a variety of good linguists (Jakobson, Benveniste, Ullman), he presents a theory that the relation between the signifier and the signified is a "necessary" one. He does not explain what sense of necessity is operative here, but the argument is clear nevertheless. Given the commonly understood definition of the sign (a sign is a relation between a
signifier and a signified), the relation between signifier and signified is fixed, or necessary. Penner concludes that "the double nature of the sign as a signifier/signified does not refer to an object. Once this is forgotten or overlooked we begin to search for 'meanings' where they cannot be found" (10). Unfortunately, this conclusion does not follow from the necessity of the sign. According to traditional semiotics, the meaning of a sign is identified with the signified, in other words the meaning is a part of the sign, but the sign is not itself meaningful. Rather, the meaning relation helps to constitute the sign. Given Penner's own confusion over this matter, it is surprising when he bluntly concludes that "given the above evidence from linguists, Staal's position is simply wrong" (11).

Two further points from Penner's paper deserve comment. The first is his claim that Staal's article contains its own refutation (12). This is a common criticism of Staal's theory, and thus worth comment even though Penner's argument is not clear in this case. Penner gives an extended quotation from Staal's article as illustration that rituals can be quite complex, and then says "the context it is clear includes a specific time and place, numbers, forms, myth and cosmology. This should suffice as evidence that the ritual is more than a purely syntactic structure lacking significance" (13). Penner seems to think that Staal's position is fallacious on its surface, judging by the fact that Penner jumps from his thesis to his conclusion without any arguments being given to support his position. A refutation of Penner's criticism will therefore simply require the demonstration of how Staal's argument does work, and that will be carried out in Chapter 1. But the basic problem with Penner's criticism should be clear from my discussion of the same criticism in section 0.3.4: Staal's theory is a general theory, but it is not intended to be comprehensive. There are aspects of ritual that it cannot and should not be asked to address. Penner would have to demonstrate that Staal's theory is incomplete (and thus inadequate) if it does not show how to address such contextual factors; but Penner does not present such an argument.

Penner concludes by making an interesting analogy --- this actually the most useful part of his paper, I think. Lots of systems are structured by rules, not just ritual systems. Consider kinship systems, for example. Kinship systems are certainly not meaningless: they describe real relations between people. So if Staal's argument applies to ritual, why does it not apply to kinship? There are
many ways to answer this question, and I will not give a detailed response. I will note that this is not a refutation of Staal's position: it simply forces us to recognize that diverse social forms might share common features, and that these features may co-occur. To the extent that social organization and ritual share the same syntax, and if meaning derives from this syntax in the former case, then we have an argument that it does in the latter as well. But this leaves a lot of details to be filled in. In any case, the general form of Penner's complaint is just right, even if it is not the cut-and-dry refutation he believes it to be. Eventually it will be necessary to look at different types of rule-governed social forms and ask how they differ. This will provide us with evidence for how social cognition works at general and specific levels, and hopefully will lead to a greater understanding of ritual. Penner rightly concludes his paper with this question, because it looks forward to future research that has the potential for helping us to understand how it is that people live in and form societies.

0.3.6 Brian K. Smith. I next turn to Brian K. Smith, a Vedic scholar who has attacked the meaninglessness theory on a number of occasions. His criticisms are quite consistent, but generally rather brief. It is therefore interesting that half of Staal 1993a is devoted to responding to Smith's short review of Rules Without Meaning (Smith 1991): I interpret this degree of attention as a sign that Staal thought Smith's misinterpretations of his work to be widely shared, and used this as an opportunity to clarify his theory. The ways in which Staal failed to clarify the issue in his response to Smith will serve as the beginning of my own treatment of the theory.

In his review, Smith makes four criticisms of Staal's theory. First, (1) he argues that Staal's position is internally contradictory: at one point Staal claims that rituals are meaningless, and a page later proposes that originally meaningless activity was over time infused with meaning. Elsewhere, he adds that Staal also claims just the opposite, that "ritual is the historical result of the obsolescence of once meaningful activities". Second, (2) he argues that the Vedic ritualists themselves did not view ritual as meaningless; instead, they disagreed as to what the meanings of particular rituals were, but they agreed that rituals did have meaning of some sort. Third, (3) he argues with Staal's claim that no symbolic meanings go through the heads of those engaged in ritual. Fourth, (4) he says that "Staal confuses 'no intrinsic meaning', 'changing meaning', and 'multiple meaning' with 'no meaning'. Of
course rituals have no intrinsic meaning; nothing has intrinsic meaning. Human beings infuse ritual (and everything else) with meaning..." The theme of these complaints is straightforward: Staal says (apparently) contradictory things at different places, and he does not clarify what he believes constitutes meaning in the context of ritual (he has no theory of meaning, we might say, or rather Staal never defines 'meaning'). If we were to give Staal's theory a charitable interpretation, these general complaints would provide a good place to begin in unearthing Staal's ideal intentions. But rather than giving a charitable reading, Smith concludes that there is a "fundamental poverty [in] the thesis" that ritual is meaningless; Smith rejects the theory outright, for its apparent absurdity.

Now for Staal's response. He frames his response to Smith's fourth criticism in the context of human evolution, arguing that meaning came into the world with the evolution of natural language. Afterwards, meanings came to be applied causally to all manner of things: "mountains, bushes, the universe and ourselves" (Staal 1993a, 12). But meaning is "primarily, a product of linguistic expressions" (Staal 1989a, 21, quoted in Staal 1993a). 31 Thus it seems Staal does not believe that words have intrinsic meaning in the way that, for example, electrons have spin (in other words, by some law of nature). Rather he believes that it is an intrinsic property of meaning that only words can have meaning. And presumably the fact that words can have meaning is related to the fact that the pairing of a word and a meaning is fairly firmly fixed. If everyone has a different idea of what 'loves' meant, then we can fairly say there is no agreement on the meaning of the word, and hence that the word is meaningless (though, as Lewis Carroll's poem makes clear, it sounds like it has meaning). So words can, and do, change meaning over time, and they can also have different senses simultaneously, but there is a principled difference between a word changing its meaning, or having multiple references, and it having no meaning but being treated as though it does. Let me put the case slightly differently. Smith holds that nothing has intrinsic meaning, including words, and therefore we should take any attribution of meaning equally seriously. Staal argues that meaning is an essentially linguistic notion, and it is to mis-speak to attribute meaning to non-linguistic entities. Put thusly, neither claim is very helpful. Smith leaves us with no principle for evaluating claims regarding the meaning of rituals, though scholars do need the ability to, for example, challenge claims of this type when made by their
peers, if not by their informants. And Staal stops short of telling us anything useful about ritual: if rituals cannot have meaning, then what is it they have when ethnographers erroneous report that they have meaning? If the Eucharist has something to do with the Last Supper, then what is the nature of the relation? If Staal cannot answer these questions, then his theory of ritual is poor indeed. I think he can provide an answer, though perhaps he does not express it as clearly as he could.

Staal goes on to point out that there is no exact equivalent of "meaning" in Sanskrit, and he applauds the Indian philosophers of language for not making the mistake of many Westerners, of extrapolating meaning wantonly to domains far removed from the linguistic.

He responds to Smith's second criticism34 by clarifying which "ancient Indians" he has in mind. "Meaning" cannot be applied to rituals in Sanskrit, so none of the ancient Indians will directly address the claim that ritual is meaningless. Ancient texts like the brāhmaṇas might attribute meanings to rituals, but these are not "meanings" in the sense used in Staal's theory. But one tradition, stemming from Kautsa, held that the words used in a ritual context (mantras) were in fact meaningless; Staal addresses this in his book (1989a, 234-5, 373-375), and as a reviewer Smith should have mentioned it since it directly addresses his own criticism of the book. In addition, Staal adds, the trend in Indian thought from the time of the upaniṣads and the rise of Buddhism onward was to devalue public ritual while emphasizing the value of jñāna, or knowledge. Thus Smith's second criticism does not seem terribly relevant. If "meaningless" for Staal is synonymous with "held in low regard", then Smith's general criticism does hold: later Vedic texts do tend to devalue the ritual tradition of action by reinterpreting it through an esoteric theory. But "meaningless" does not mean "held in low regard" for Staal, so the criticism misses its mark. And Smith's decision to privilege the brāhmaṇas over the ritual sūtras is not based on any solid foundation, except perhaps that Smith likes the meaning madness of the brāhmaṇas35.

Staal believes he has provided an answer to Smith's third criticism (1993, 15). I find Staal's claim that no symbolic meanings go through the heads of ritualists while they perform rituals to be somewhat beside the point, but regardless nothing in his theory rests on this one argument.
Staal does not explicitly address Smith's first criticism, but perhaps it is worth drawing attention to the fact that here, as elsewhere, Smith does not appear to be a careful reader of what Staal actually says. The claim that I call Staal's Ethnology Thesis holds that, phylogenetically, ritual preceded language in the evolution of anatomically modern humans. This thesis is used in an historical account of how it is that ritual is meaningless: if, in deep prehistoric time, ritualized activity preceded meaningful activity, then ritual evolved in an environment free of meaning; therefore ritual per se does not rely on meaning relations; therefore it is possible that ritual is still not meaningful. When Staal considers the fact that early Vedic texts (the brāhmaṇas) frequently attribute meanings to rituals, but somewhat later texts (Kalpa Sūtras) do not, he is making an entirely different sort of point. As it happens, Staal terms the Kalpa Sūtras 'scientific' and the brāhmaṇas 'non-scientific' (1989, 365) in response to another article by Smith; see my Chapter 5 for more on this issue. The point of this distinction is to argue that the Kalpa Sūtras serve a different purpose than the brāhmaṇas in that they successfully describe the actual mechanics of Vedic ritual, and the fact that they do not consider the meanings of rites is significant. It means that the Vedic ritual system has no semantics, though it does have an elaborate syntax. In other words, the brāhmaṇas are evidence that a lot of people attributed a lot of different meanings to the rituals, but the Kalpa Sūtras are evidence that the best ritualists were not confused by this proliferation of meaning attributions. Thus Staal here gives a discussion of the historical evolution of textual treatment of Vedic ritual, rather than an account of the evolution of the rites themselves. Smith is confusing three different arguments in his criticism: one that rituals are meaningless; another that gives an account of how meaningless rituals came into being in the first place; and finally, an account of the historical interpretation of a particular tradition among differing types of ritual specialists.

Smith makes a different sort of complaint in his book *Reflections on Resemblance, Ritual, and Religion* (1989, 30-34). Here he puts Staal into the same category as early Orientalists such as Müller, Macdonell, and Eggeling, scholars who practiced "the Indological tradition of denigrating the brāhmaṇas by contending that the explanatory connections drawn in the texts are inconsistent,
contradictory, arbitrary, and, again, 'fanciful'. Smith assumes that "meaningless" means "worthless". But, as I will attempt to show, Staal terms rituals meaningless in a rather precise and limited way, so this brute equation does not necessarily hold. At times Staal does say rather uncharitable things about Vedic ritual, but he also evinces a strong respect for the tradition. Whether and how Staal falls into the tradition begun by early Orientalists is a complex problem, as it is for any Western scholar working on Asian materials, Smith included, and I do not see the need to consider it at length. If "Orientalist" is taken as a term of abuse, then I think it should not be applied to Staal without very careful consideration, and not through of a caricature of his views on the meaningless of ritual.

This last criticism of Staal does not have a political motivation, however, as many such ad hominem attacks do. Rather, it is intended to further Smith's own approach to Vedic thought, and to be properly understood it should be put into this context. Smith believes that previous scholarship gave an uncharitable (in the hermeneutic sense) interpretation to Vedic thought in assuming that the equivalences (bandhus) thought to be so characteristic of Vedic texts as a genre were wanton and unmotivated by larger considerations. In contrast, Smith argues that there is a logic at work, and the purpose of his book is to show how this logical system works. Put very briefly, Smith sees the bandhus as "linking components of various planes of reality to one another" (1989, 218), in other words helping to conceptualize the overall cosmic and ethical worldview of the Vedic peoples, by showing the interrelationships between different levels of the phenomenal and social world. Thus bandhus are not wanton but studied, and follow a logic of their own. Smith is therefore very sensitive to the old, Orientalist paradigm, and in his vigilance he tends to tar everyone with the same brush: his argument that Vedic thought was logical is directly opposed to the Orientalist view that degraded the bandhus, and therefore Vedic thought generally. Thus he argues that Staal's attribution of meaninglessness to ritual is "a meaningful interpretation of a previously improperly understood phenomenon --- and thus is itself a paradox that calls out for a solution" (1989, 223).

From Smith's perspective, Staal occupies a status no different than the author of a brāhmaṇa, in that both are trying to enunciate the meaning (or lack thereof) of a rite. So, to adopt language currently in vogue, both are matters of opinion, nothing more. This approach is in direct conflict with Staal's,
who holds that it is a fact of the matter how rituals operate. Someone who successfully understands how rituals work (or perhaps one who abides by certain sorts of methodological principles --- Staal is not clear on what constitutes the relevant criterion) is engaged in science, and thus stands apart from someone who simply volunteers opinion. It is therefore not surprising that Smith misunderstands Staal. While both scholars share a deep knowledge of the Vedic source materials, their interpretive frameworks are quite different. As I will argue later, one advantage of Staal's theory of ritual is that it emphasizes the cognitive capacities that underlie human ritual action as a subject for inquiry, and this derives from his scientistic framework. Smith, I believe, is in contrast on shakier ground hermeneutically. But as it is not my job here to review his views, I will not present arguments for this claim.

I have considered Smith's criticisms and Staal's responses at some length, because I see Smith as the prototype of Staal's most powerful critic: a religion scholar familiar with the Vedic source materials, who sees Staal's theory as silly and misguided. It is with such criticisms in mind that I have devised my defense of Staal's position.

0.4 Definitions. Many people expect a study that relies heavily on key concepts (such as ritual) to begin with a definition of terms. I do not believe that this is always necessary, or even salutary, but since many of my readers will no doubt expect some clarification of what I mean by "ritual", some discussion of the issue may prove useful at the outset. I provide a brief overview of how definitions may be made, and then consider what type of definition would be most appropriate to the present study.

0.4.1 Types of Definitions. To begin with, since I am considering Staal's approach to ritual, and Staal relies entirely on Vedic evidence to support his claims, it would be easy to say that what follows is a theory of Vedic ritual. Vedic ritual may be considered a closed corpus with a finite number of rites, and in this case a conjunctive definition would suffice:

(1) ritual = {Agnyadheya, Agnihotra, . . . , Agniṣṭoma, Ukhyā, . . . Aśvamedha}
By conjunctive definition I mean a complete statement of all the particular types of performance that count as religious rituals. In this example I use ellipses to indicate what a complete enumeration of Vedic rites might look like, without listing all the members of the set. If I wanted to give a complete enumeration, I would have to devise principles for individuating rites, and that would take me far afield from the question at hand. For example, should we include just śrauta rites, as Staal has done, or also include grhya rites? Also, different Sanskrit ritual manuals describe different numbers of rites, and sometimes the differences between rites are slight.

It would also be possible to include other rites in this list, once it has been demonstrated that Staal's theory can account for those rites. So if a study of the Christian Mass were to demonstrate that the theory applied to it, then (1) could be extended to include the Mass as well:

\[ (1') \text{ritual} = \{ \text{Agnyadheya, Agnihotra, \ldots, Agniśṭoma, Ukthyā, \ldots, Aśvamedha, \ldots, Mass} \}\]

and so on for other rites. It might also be demonstrated that there are rites to which the theory does not apply, perhaps Christian Baptism or Islamic pilgrimage (Hajj). Then we would have a different list:

\[ (2) \text{non-ritual} = \{ \text{Baptism, Hajj, \ldots} \}\]

If it were the case that Staal's theory applied to some putative rituals but not all, then we would have grounds for typologizing putative rituals into different categories. We might with to rename categories (1) and (2) and call them both 'rituals', in order to preserve the commonly used term 'ritual' for the wider class of religious performances, then adopt other, similar terms for the more restricted classes:

\[ (3) \text{ritual} = \{ \text{rite, ceremony} \}\]
In this case, for convenience, I have denoted class (1) as "rite" and class (2) as "ceremony". I should point out that these terms were chosen at random from the vocabulary of ritual studies, and do not indicate any assumption about essential features of the categories in question. In fact, essences need play no part at all in a conjunctive definition.

To rely on essences would therefore constitute another and different approach to definition, what I will call an analytic definition. For example, when Geertz describes ritual as "consecrated behavior" (1973, 112), he is giving a definition in terms of necessary and sufficient traits that determine the class in question. Taking this short definition (admittedly, not Geertz' ultimate answer to the question of what ritual is, but a convenient example none the less), we take ritual as being a subset of the class of all behaviors, distinctive on account of the process of consecration:

(4) behavior = {ritual, . . . }

Of course, a definition of consecration would likewise be required under such a theory. Two components are, therefore, necessary to such a definition: first, the larger category (behavior) to which all rituals belong, and second, a character possessed by all rituals but by no other elements of the larger category (thus all and only those behaviors that may be described as consecrated are rituals). Other things may be consecrated (objects, for example), but if they are not rituals then we know they are also not behaviors.

Traditionally, analytic definitions were seen to parallel the real character of things. Thus, Aristotle describes man as a "thinking animal" for two reasons: to develop a biological typology, but also to identify the characteristics that really make something a man and not a tree or a cat. But the metaphysical interpretation of analytic definitions (the assumption that definitions identify true characteristics) may be set aside if the goal is solely that of typologizing. In this case, there is an acknowledgment that other, different typologies could be developed that are equally useful, because neither gets to the real essence of the thing itself. We might call analytic definitions that do not make metaphysical claims heuristic definitions.
A third method of definition is by similarity to a prototype, or by a set of characteristics that are neither absolutely necessary nor sufficient, though taken as a whole the possession of some bulk of those characteristics is necessary for class inclusion; the latter is commonly known as family resemblance. Such definitions as a class I will call *polythetic definitions*.

**0.4.2 Definitions of Ritual.** Most commonly when people expect a study to begin with definitions, they expect analytic definitions. They want to hear a characterization at the outset of the inquiry as to what kind of a thing ritual (for example) is, and the what-kind-of question is most easily answered by a general description. But there are dangers in using analytic definitions. First, it is possible that a subject under consideration does split into natural kinds. A study that presumes the wrong nature of its subject will likely fail, but the failure might not be noticed for a long time. Persistent scholars might continue trying to force square pegs into round holes, as it were, perhaps because of complexities or limitations in the data at hand. Second, it is possible that the subject under scrutiny is a disparate set of kinds of things, and that no analytic definition will therefore capture any common features. In such a case a conjunctive definition is best, even if it does include ellipses that will hopefully be filled in at some future time. (At a later stages of study, a conjunctive definition may have analytic components, rather than components that are lists of individuals, but this hybrid style of definition is only liable to be encountered when the object of study is understood quite well.) Third, even if an analytic definition is only heuristic, there is a tendency to hypostasize such definitions and think of them as describing real essences. Avoiding definitions that look analytic is often the best course, at least in the early stages of an inquiry, to keep from mistaking the force of one's heuristic definition.

I believe that the last of these dangers plays a role in Catherine Bell's (1992) criticism of theories of ritual. Since her book has been very influential\textsuperscript{37}, I will consider it at some length. But since a consideration of Bell's theory will appear to be a shift away from my discussion of definitions, let me say this before moving on. *Readers who expect definitions at the outset may assume for now that what follows is a theory of Vedic ritual,* and further, that it may also be successfully applied to other rituals as well, on a case-by-case basis and only after it has been shown that it is useful in understanding...
those rituals. My approach will be to focus on Staal’s theoretical commitments, and to look for tangible examples that will help solidify arguments that might seem rather aetherial otherwise. These examples will come from the Vedic tradition (generally these will be examples used by Staal himself) and from other traditions that might be more familiar to some readers.

Staal believes that his theory is in fact a general theory of ritual, but I will neither make nor deny such claims. Instead, I will focus more on a position that is at least implicit in Staal’s presentation of his theory, namely that the goal of ritual studies is to identify the capacities that allow humans to engage in ritual activity. Understanding these capacities will help us understand their products (rituals themselves), but the understanding of what rituals are is very much a secondary process here. Thus I believe that Staal’s project is much more in keeping with Bell’s than even she realizes, and I believe that Staal escapes from the mold of ritual theorizing that Bell devotes such pains to identifying. Since Staal’s theory, as I reconstruct it, is focused primarily on understanding cognitive capacities, and only secondarily on understanding rituals, I do not think an explicit definition of ‘ritual’ is helpful. It might be that the relevant capacities are utilized in some of the things we commonly call ritual but not in others. Then we might be inclined to posit natural kinds based on these conclusions, but we will be under no obligation to do so. And it might be that these capacities are utilized in all or some putative rituals, but also in other, non-ritual contexts. This will tell us how sui generis rituals are as a class, however rituals may be defined.

0.4.3 Ritual Theory: Bell and Staal. Two concerns seem to be primary for Bell; I will call them the Metaphysical Problem and the Ethical Problem. Bell proposes that previous theories of ritual have created an unnecessary and unwarranted dichotomy between thought and action. The first part of her book attempts to describe this dichotomy, show how it develops in a number of influential scholars’ work, and describe the dangers of such an approach. The second part of her book attempts to provide an alternative framework for discussion of religious rites; it rejects ritual as an analytic category, and replaces it with ritualization as a mode of action rather than an object of study in its own right. The underlying approach is therefore to avoid the problem of hypostasizing ritual by adopting new terminology that facilitates the conceptualization of ritual action as a process.
Sometimes it seems that Bell is in sympathy with a general philosophical approach that in the philosophy of science is known as the Social Construction of Reality; more generally, this is part of an anti-realist program that may be fairly described as relativist (though the term conjures up many connotations that are not relevant in this context). The Metaphysical Problem derives from this tendency in her thought. Thus she often will speak of ritual as "(just a) theoretical construct", which serves to construct both the object of study and also the subject himself.38 Her theoretical allegiances to the anti-realist camp (anti-realist because it is anti-scientistic) can be assumed from the fact that she founds her criticism of traditional approaches to ritual, because of their historical origins, in "that period in which 'reason' and the scientific pursuit of knowledge were defining a particular hegemony in Western intellectual life" (6). The usual criticism of scientific knowledge argues that the entities posited by scientific theories do not exist apart from the theories in which they occur, and that there is no correspondence between such theoretical constructions and what actually exists "out there" in the world. This anti-realist approach faces the slippery slope common to all breeds of relativism, according to which the anti-realist theory itself is just another constructed theory, and hence has no more claims to truth than the scientific theories it rejects. Bell seems aware of this problem, and hedges her claims. Her bibliography and language suggest that she has strong anti-realist commitments, but she also seems aware of the danger that such a commitment would present for her own, positive contribution to ritual theory.

The Metaphysical Problem may therefore be stated roughly as follows: Ritual theory should not make use of the notion of ritual, because ritual as an object of study is generated by the discourse of ritual theory and has no independent existence in the objective data themselves. The whole notion of objective or raw data (14, 17) is, of course, problematic in itself, in the context of an approach that rejects objectivity. After all, if theories construct their own subjects and objects, then they certainly construct the data that are used to support their claims: this is the only way that data can support theories that bear no relation to the actual organization of the world. Bell seems to favor as an approach "truly thick ethnographic description" when it is not "due to the influence of categories developed to empower theoretical discourse" (4), presumably because this is the only way to separate ritual studies
from any larger theoretical project. But a strict anti-realist would reject the very notion of thick ethnographic description, and would argue that it relies on categories that are generated by larger patterns in the culture of the subject or interpreter, and that these are just as subjectively conditioned as categories generated by an explicit, theoretical discourse.

It should be noticed that even in my explanation of the Metaphysical Problem, references were made to 'hegemony' and 'empowering'. Bell finds it difficult to make a metaphysical argument without giving it an ethical tinge, and I think this points to her basic concerns, which ally her uncomfortably with the anti-realist camp. Bell seems concerned that Western theoretical discourse tends to serve the ends of Western scholars, and allows no role for the agency of objects (objects of this discourse and of the power relations that such discourse supports). A better ally for Bell might be the Subaltern Group rather than the anti-realists; the Subaltern Group consider an epistemological problem rather than a metaphysical one, and their concerns with the ethical implications of modes of discourse are quite explicit. We see these issues most clearly in Bell's chapter on "Constructing Discourse". Here Bell admits that "perhaps the initial generation of polarized distinctions and their subsequent homologous replication is inevitable in the process of analysis or interpretation" (48), in which case her theory will fair no better than those she criticizes; I take this as an acknowledgment of the dangers of relativism. She goes on to add that dichotomization may be "integral to fundamental processes of differentiation involved in perception, cognition, and human activity in general." She then suggests that only "closed and highly structured discourse" is what generates "secure bodies of knowledge". This is consistent with her preference for truly thick ethnographic description, because the latter is generated non-systematically and therefore constitutes what we might call an "unsecure" body of knowledge: its warrant derives solely from its agreement with its raw data, and not from general principles. Clearly such unsecure knowledge has no greater claim to objectivity than secure knowledge based on a highly structured discourse; in fact, Bell prefers unsecure knowledge precisely because it does not make claims to being objective. So I think that ultimately Bell is not driven by metaphysical concerns. This becomes clear when we consider why unsecure bodies of knowledge are to be preferred over secure ones.

46
"Yet the more subtle and far-reaching distortion is not the obvious bifurcation of a single, complex reality into dichotomous aspects that can exist in theory only. Rather, it is the far more powerful act of subordination disguised in such differentiations, the subordination of act to thought, or actors to thinkers" (48-49).

I take the first part of this passage to support my claim that Bell is not primarily interested in metaphysical problems resulting from scientific discourse, and the second as supporting the argument that this passage introduces, to the effect that Bell is most concerned that the thought-action distinction privileges thought, and therefore sets actors in a subordinate position to those who study them and theorize about them.40

More importantly for my purposes, the thought side of the dichotomy encompasses two major areas: indigenous belief and the theoretical knowledge of observers. A view that Bell traces back as far as Durkheim41 holds that the essential core of a rite is expressed in indigenous beliefs. According to this tradition, the purpose of scholarly research is to get at the core beliefs of natives: therefore good theory unearths beliefs, and in a sense replicates them.42 But she finds it highly problematic when an observer generates theoretical knowledge about a human object, then claims that this knowledge is equivalent to the core beliefs underlying the religion and society of the people being studied. This necessarily constitutes appropriation of another culture for one's own purposes, by reconstructing that culture in an image of one's own making.

The Ethical Problem may thus be stated as follows: Explicit and strict dichotomies tend to privilege one member of the resultant pair, and this privilege tends to support power relations that subject one element of or group within society by setting the terms of discourse in such a way as to systematically disempower that group; on ethical grounds such disempowerment is to be rejected, and this rejection entails the rejection of strict dichotomies.

This should suffice as a summary of the position of Bell 1992. She mentions Staal's theory twice. One casual mention is surprisingly accurate: "Frits Staal's arguments about the meaning of ritual residing in the structure of the act itself" (89). She is thus one of the few scholars who recognize that
the Meaninglessness Thesis is founded on the Ritual Syntax Thesis. Elsewhere, she is perhaps critical of Staal's theory:

"Frits Staal gives an interesting demonstration of the problems that arise when ritual is seen as 'pure activity'. By this characterization, made in the context of a clear and complete opposition between thought and action, Staal wishes to maintain the total resistance of pure activity to any theoretical appropriations whatsoever. Thus, Staal concludes that ritual cannot be understood, that it is meaningless." (59-60, n. 48)

What Bell actually thinks of Staal's theory is not entirely clear. When she sees Staal use phrases like "pure activity", she immediately puts him in the lineage of thinkers that her book aims to critique; she does not cite Staal 1989a, in which he gives his preferred intellectual lineage. Pure activity must be the opposite of pure thought, after all. But my reading of Staal takes pure activity as the manifestation of cognitive capacities, and under this reading the expected antithesis of thought and action is not so clear.

So in one respect Bell seems to lump Staal with other thinkers whom she has considered, but she adds that a notion of pure activity requires the rejection of "any theoretical appropriations whatsoever"; this certainly agrees with her view of what ritual theory should do, but it has little to do with Staal's theory as I read him. The context in which the note appears seems to support this interpretation. Bell is criticizing scholars like Geertz for appropriating native tradition when they go beyond the raw data and thick ethnographic description simpliciter to create a structured discourse founded on secure knowledge. Perhaps Staal is intended as a partial antidote to Geertz's theoretical excesses. Ironically, then, she appropriates Staal's theory, which scholars like Brian K. Smith see as rejecting indigenous agency out of hand, in order to emphasize one implication of the theory that she finds particularly salutary. Unfortunately, Bell never discusses Staal's theory except in passing in any of her publications, so all of this is somewhat hypothetical.

Earlier in this section, I did two things. First, I proposed a conjunctive definition of ritual for those who expect such things, but I also said that I do not feel such a definition to be helpful in the present inquiry. My reason was that the Staal's theory is ultimately a theory about certain cognitive processes, and the light it sheds on the "essence" of religion may therefore be rather minimal. It sheds a
great deal of light on rituals, I hope, but not necessarily on their essence or inner character. So not only is a definition not necessary at the outset, it might also not be forthcoming by the end of the inquiry. (Then again, there is no principled reason why such an inquiry might not end up finding good grounds for some particular definition of ritual. The point is that a lot of work still needs to be done before we know whether this approach will shed any light on the essence of ritual.) Second, I pointed to a reason why Bell should be expected to find Staal’s theory salutary. Staal’s theory is one of the only theories that seems to avoid her criticisms of the current state of ritual studies, namely the complaint that they generate a definition of ritual predicated on a strict thought-action dichotomy, and that this definition is suspect because it implicates the resulting theory in the appropriation of natives’ agency by the observer. She seems to see Staal as buying into the thought-action dichotomy, but avoiding theoretical appropriation through his understanding of meaninglessness.

1 In Chapter 1, I consider the third thesis and the argument that underlies it. Let me summarize that overview here: The claim that I call Staal’s Ethology Thesis holds that, phylogenetically, ritual preceded language in the evolution of anatomically modern humans. This thesis is used in an historical account of how it is that ritual is meaningless: if, in deep prehistoric time, ritualized activity preceded meaningful activity, then ritual evolved in an environment free of meaning; therefore ritual per se does not rely on meaning relations; therefore it is possible that ritual is still not meaningful.

2 Note that McCauley and Lawson 2002 includes some minimal discussion of ritual compositionality.

3 This argument recurs throughout Staal’s writings on ritual. See especially his short volume on The Science of Ritual (1982b) and the chapter in Staal 1989 based on this book (chapter 26).

4 Vedic is an old dialect of Indo-Aryan. It is not precisely the direct antecedent of classical Sanskrit, but very nearly is so (much in the same way as the language of Beowulf is not the direct ancestor of Chaucer’s Middle English). So both types of treatise described the language or ritual of a particular group in ancient India.

5 Pāṇini is the chief exponent of the Sanskrit grammatical tradition. I will discuss him and his work further below.

6 In fact, Jones was not the first to note the relation between the classical languages of India and Europe: Of particular note in this regard is Halhed.

7 I will subsequently refer to Pāṇini and his successors in the Indian Sanskrit tradition as the "Pāṇinian tradition", or simply as 'Pāṇini', meaning Pāṇini et al.


9 My discussion of early linguistics course offerings at Penn is based on relevant issues of the University of Pennsylvania Bulletin.

10 Chomsky’s B.A. and M.A. theses constituted successive drafts of the work Morphophonemics of Modern Hebrew.

11 Dr. Ludo Rocher informs me that it is unlikely that Dyen learned his Pāṇini from Brown. He suggests that perhaps Franklin Edgerton did.

13 Strictly speaking, the sentence does not occupy pride of place in Pāṇini’s linguistics that it does in Western views of language. This is in keeping with the presentation of Sanskrit: sentences composed entirely of nominal constructions are common in the language, and the limited punctuation that devanāgarī (the script in which Sanskrit is most often written) provides distinguishes clauses more clearly than sentences.

14 One of the dominant themes within twentieth century linguistics as been a concern with distributional issues: put loosely, this means that linguists have tried to develop theories that can account for the order in which elements (sounds and words) occur within a sentence. Graeco-Roman linguistics had little concern for distributional issues, in part because Latin and Greek allow virtually free word order. Word order is similarly free in Sanskrit, but nevertheless Pāṇini shows an almost modern concern with the distribution of elements in many respects. Distributional issues have long been important in the Chomskian tradition, and they quite obviously are of concern in computational linguistics: if we wish to program a computer to generate sentences, we need to give it all the tools necessary to go from a semantic interpretation all the way to a complete sentence, and this means it needs rules for not just generating words, but also arraying them in a linear string.

15 Examples here are taken from Cardona 1988²/1997, 257-8.

16 The nominative case is used for the subject of a sentence in Sanskrit ("Fred' in the sentence "Fred knows the answer"), or for a predicate nominative ("blue' in "The car is blue").

17 The word bahuvrīhi means "much rice", but it is used to describe a certain type of nominal compound, one that functions essentially like an adjective by stating the relationship between two things: for example, Indra-śatu means "the foe of Indra", or "having Indra as a foe", and hence can be used to describe one who is opposed to Indra.

18 These words actually state rule 1.1.67. In this case, though, I am interested just in the words, and not in the rule they express.

19 These texts describe the so-called "solemn" rites. A similar class of texts, the Grhya Sūtras, describe the daily, household rites that also form part of the complete Vedic ritual system.

20 As in so many other areas of Sanskrit scholarship, the work of Louis Renou (1941-2, 1963) is still helpful many years after his death.

21 Chakrabarti 1980, 136 gives a list of the ectypes corresponding to each archetype.

22 By 'core' I mean base in the linguistic situation (for example, the base of the name Agni is Agn-, i.e., the finished form without the affix -i) and prototype in the ritual case. The most obvious difference between these two cases is that the core is a ritual in the latter case but not a word (indeed, it is only part of a word) in the former case.

23 His choice of citations from the Classics literature is somewhat better, although his apparent translation of the noun didaskaloi by the verb taught is perhaps cause for concern (229). And while he acknowledges that few scholars believe the Menexen is actually by Plato, this does not seem to discourage him from referring to it as "Plato's": this indicates some disinterest in philological and historical issues.

24 Note also that Carter's use of the word 'function' is misleading since this term has generally been reserved for a more specific meaning, namely what I am calling "social function".

25 What I mean is this: karma was not a concept in the toolbox of the Vedic ritualists (we can see the development of this doctrine in the early Upanishads); Buddhists do not, strictly speaking, recognize gods, though the Jains do; meaninglessness is not so much an Indian concept, though emptiness does play a role in Buddhist thought. Schrörer seems to have East Asian Buddhism as her prototypical image of Indian religion, but since Staal and Humphrey and Laidlaw focused their inquiries on Vedic and Jain ritual practices, Schrörer ends up presenting a rather stereotypical picture of Indian religion that is ill-suited to the purpose she puts it to.

26 I use shock quotes here because a meaningless symbol is not a symbol at all. For the question whether "changing meaning" means "no meaning", and hence whether Schröter means symbol when she says "symbol", see my discussion of Brian K. Smith below.
27 Of course, words also contribute to a structure when combined in unmarked speech, but the structures in this case are of a different type. Thus I add the caveat "of a particular sort".

28 Markedness is a notion from linguistics that has been well incorporated into theory on social forms. The idea underlying markedness is that often in languages two opposing features occur of which one is more general or basic than the other. In such cases, we can describe that feature as being 'unmarked', and the other as 'marked' in reference to its unmarked twin. For example, the plural of English nouns is viewed as marked in contrast to the singular, or words referring to females are marked in reference to their male partners (actor vs. actress, waiter vs. waitress). The masculine version is more general in that it is used to refer to mixed groups of both genders or to individuals of unknown gender. The notion of markedness is used in different senses in a number of linguistic theories; in Strenski's argument, it is clear that he means that mantras (or religious language generally) are marked with regard to regular speech, much as poetry with regard to prose. In other words, mantras stand out by their formal properties as unusual and special, and this markedness accounts for the special status and treatment accorded to them.

29 Perhaps Donald Weibe (see his 1999 collection of previously published articles) is best known for his publications in this area. Of note for their review of the history of the discipline are Hart 1999 and Shepard 1991.

30 Penner sets Staal in the tradition of nineteenth century Romanticism due to the latter's characterization of ritual as "for its own sake". As I will propose in Chapter 1, this phrase should only be taken in the context of Staal's Ritual Syntax Thesis; it bears only surface similarities to phrases such as "art for art's sake". An exception is the interesting paper of Toren 1983.


32 It is the 'primarily' that gets Staal into trouble here, but more on that later.

33 "Nor is it clear who, exactly, besides Staal regards rituals as 'meaningless'. The ancient Indians certainly did not." (Smith 1991, 143)

34 Staal 1993a, 12, likes the phrase "meaning-madness", used originally in Strenski 1991, 221.

35 What I mean by this comment is the following: After we understand a scholar's theories and have defined "Orientalist" in its current, derogatory sense, then we can label the scholar with the term if we find it appropriate. But we cannot label as scholar an Orientalist because of his field and personal background, and then be able to anticipate details of his theories on the basis of this alone. The latter approach, which I think could readily be shown to be fallacious with a simple case study in intellectual history, I would term a "political" criticism.

36 Let me give just two grounds for calling her book influential. First, it has been cited over 150 times in journal articles, both in Religious Studies and in many other fields, according to the ISI Citation Index. This is on the same order of magnitude in which Staal 1979a has been cited. Second, it is used as a main textbook in a number of schools, including: Wesleyan (in a course in Anthropology of Religion), Harvard (How to Understand Religion: Theory from Max Müller to Catherine Bell), Princeton (Problems in Roman History), Haverford (Approaches and Methods in the Study of Religion), Hamilton (Chinese Intellectual History), and the University of Texas (Ritual, Power, and Politics); URLs describing these courses are as follows:

http://www.wesleyan.edu/course/reli395f.htm
http://icg.harvard.edu/~hds3321/syllabus/
http://www.haverford.edu/regi/REL398a.html
http://academics.hamilton.edu/history/twilson/His337.html
http://nic.utexas.edu/asnic/subject/courses/fall98cotes.html

37 In fact, she views this process as dialectical: the subject constructs the discourse, and is in turn constructed by it.

38 The terminology I adopt here is adapted from the terms that Bell uses. I find it rather awkward, but such a course seems best when trying to treat her arguments fairly.
I would argue that the dangers of any discourse for the objects of study should be demonstrated on an individual basis, as there clearly are benefits that derive to certain people from many discourses. That is the whole point of hegemonic discourse: its adoption benefits one class in society. Other discourses are more problematic; for example, the economic strategy of the International Monetary Fund does ultimately support such ends as the provision of clean water and basic human services to the poor in underdeveloped countries, but at the price of generating large debts to banks in the developed world. Whether the economic discourse that underlies such policies is a net benefit or harm to underdeveloped countries is an active subject for debate. But for Bell, highly structured discourse necessarily supports inequalities of power; this might be generally true as a practical matter, though probably not as a matter of principle.

Bell 1992, 15; see also Durkheim 1912 [1995], 34. Note that Bell does not claim that this view originates with Durkheim; she just uses his work as a locus classicus.

There is, of course, a problem here in that theoretically-informed scholarship makes many propositional statements beyond just the recitation of native beliefs. But according to Bell, such beliefs form the goal and therefore the essence of ritual theory as practiced by many theoreticians of ritual. Her book is, after all, called Ritual Theory, Ritual Practice, and not Ritual Belief, Ritual Practice.
Chapter 1

In the Introduction, I attempted to justify my project. In this chapter, I will provide an overview of Staal's theory. Although a critical reading of his works on ritual is necessary in order to identify the key elements of his theory and present them in the most intelligible light, I will attempt to keep the following discussion as simple as the subject allows. I will try to hold back any arguments I have with Staal until Chapters 2 and 3, where I will address the meaninglessness and syntax theses in more detail. The current chapter, then, is both an introduction to the theory and a self-contained summary of it.

After serious consideration, I have divided this chapter into three parts. The first presents a close reading of Staal 1979a. This is probably the best-known and most widely read of Staal's writings on ritual, and in my experience it is frequently misunderstood. A careful analysis of the argument in this paper should therefore be of value in its own right, and I believe it provides the best way to introduce my readers to the argument. The second part of the chapter presents my own synthetic statement of the theory, drawing on Staal's writings (and providing some key references to them) but reorganizing the argument in what seems to me the best logical order. Thus, this chapter presents Staal's theory twice, once following more or less closely Staal's own presentation, then again following my own. I have attempted to make both sections relatively independent, but I have seen no reason to repeat arguments from the first part when they are called for in the second, and when I could not improve on my initial formulations. Thus the chapter is meant to be read from beginning to end, and my hope is that Staal's theory will be best understood by repeatedly returning to the same arguments from different perspectives (first in section 1.1, then in 1.2, and finally in Chapters 2 and 3).

The final part of this chapter completes my survey of critical responses to Staal's theory. In the Introduction I considered arguments critical of the theory; in this chapter I consider research of scholars who, on the whole, accept Staal's approach and attempt to build upon it by considering different religious traditions. Two of the scholars treated in this chapter were students of Staal's, and one is a colleague of his. I am interested both in the readings these three authors give of Staal's writings, and
also in the value of their own contributions to the syntactic theory of ritual. Thus, I do not address other advances made in these publications, for example the contributions they make to the study of particular ritual traditions (Shingon, Western Asian, and Taoist, respectively).

1.1 Staal 1979a: "The Meaninglessness of Ritual". This paper may usefully be divided into four parts for purposes of this study. The first part extends through the end of section III (8), and provides an introduction to Staal's original contribution to ritual theory, which occurs mostly in sections IV and V. The second part (section IV, 8-15) treats the problem of meaning in some detail. The third presents a brief outline of the formal analysis of Vedic ritual treated in more depth elsewhere, notably in Agni (Staal 1983) and also in "Ritual Syntax" (Staal 1979c); it comprises section V (15-19). Sections VI (misprinted as IV in the original Numen article) and VII form the conclusion, which discusses the similarities between the formal structures of language and ritual. Before examining the paper section-by-section, a few general comments may prove helpful.

It is worthwhile noticing that this article does not aim to present anything approaching a deductive proof for Staal's theory. Rather, the argument is largely informal, and intended to persuade rather than demonstrate the value of viewing ritual as meaningless. Clearly Staal misunderstood his audience, and in retrospect it is possible to see why: understanding Staal's theory requires an insight as to the value of a theory of the type he proposes, and anyone who is not disposed to see the argument in this light will quickly be put off by the tone of the article. Appreciating the care with which this article was written, therefore, requires that one be suitably prepared and take the claims Staal makes in the proper light.

Rather than presenting a deductive argument, Staal's approach is strictly essentialistic. This is a point I believe few people have realized, though it is implied by the statement that "several anthropologists have detected features of meaninglessness in ritual, without recognizing that these features express its essence" (10). To understand the essence of something requires no more and no less than looking at that thing from the right perspective, and "The Meaninglessness of Ritual" seems intended to lead one to that perspective.
Several other choices seem not to have been as successful as Staal originally intended. One is the decision to base the theory on ritual in one tradition, and within that tradition largely on the basis of one, complex ritual, the Vedic Agnicayana. Staal chose the Agnicayana because he had recently completed a detailed study of this rite, which would result in a large, multi-authored, two-volume edition, as well as an audiotape and a video. The Agnicayana is also notable for its place in the Vedic ritual canon as being (on the one hand) one of the longest and most complex of the Vedic rites, and (on the other) well attested and known to have been somewhat regularly performed. It is therefore a "real" ritual rather than a theoretical construct, yet complex enough to allow detailed analysis. It also contains several independent rites as proper subparts, a fact which is of great importance for Staal's theory. I will have more to say about this decision later.

A second unwise decision was the context in which Staal presented his argument. He considers a wide range of approaches to ritual (7-8), and then rejects them cavalierly and without detailed argument. Perhaps it is not surprising that other scholars have treated his own theory in an equally cavalier fashion. Staal's language is also poorly chosen on many occasions --- this constitutes a third poor choice in the mode of presentation of his theory. He eschews the consistent use of terminology. One example should be sufficient to show that this is the result of a conscious choice rather than careless writing: "to say that ritual is for its own sake is to say that it is meaningless, without function, aim or goal, or also that it constitutes its own aim or goal" (9). I will return shortly to this sentence, but for now I simply wish to point out that Staal is treating two apparently inconsistent statements as synonymous: for a ritual to be without aim or goal is just the same as for it to have itself as its aim or goal. Yet no ritual is nothing; every ritual is a ritual, a socially constructed action. So to say that rituals are "without aim" must be a shorthand for saying that they are "without any aim (except themselves)". Similarly, "meaningless" must be shorthand for some more complex claim, something along the lines of "having no meaning of the type one would expect", or (in more essentialistic terminology) "having no meaning inasmuch as meaning attaches to ritual in virtue of the type of thing ritual is". 

55
1.1.1. Sections I - III: Introduction. Staal begins by clarifying the main source for his theory: the Agnicayana ritual performed in 1975 in southwest India (2). He then states the key elements of his theory (3): rituals do not refer to anything other than themselves; rather, participants focus on the correctness of their actions.

Staal goes to some lengths to support the claim that symbolic meanings do not "go through the heads" of agents while they are performing rituals. Since he has not provided a theory of what ritual meanings are, it is not clear whether this consideration is relevant, but it is a point he seems intent on making. He then considers several possible meanings that rites (or, in the case of the third example, the absence of a rite) within the Agnicayana could have, only to reject their significance:

1. The patron of the ritual keeps his hands closed "like a child in the womb of its mother, ready to be reborn".
2. The fire altar is shaped like a bird because fire was brought from heaven by a bird.
3. The south is inauspicious, and therefore priests do not go south if they can avoid it.
4. Certain bricks in the altar are consecrated so that it will rain.

It should first be obvious that (contra Penner --- see section 0.3.5) none of these examples assumes that ritual meaning is reference. Example 1 is based on a similarity, and phrased as a similarity. Example 2 is etiological. Example 3 is hard to classify. If we treat it as causal, the result (priests generally don't go south) is a consequence (though not in the logical sense) of the reason (the south is inauspicious); the latter could perhaps be viewed as resulting from a dual classificatory schema (see, e.g., the articles collected in Needham 1973). Notice that in this case, the relation is nearly analytic: the meaning of "inauspicious" is fairly close to the meaning of "things to avoid doing". In the final example, ritual is viewed strictly as efficacious action, just like any practical, mundane action would be. In other words, this is quite a miscellaneous assortment of types of meaning, and in none of the four cases does ritual meaning have any close connection with an analogous sort of linguistic
meaning. At this point, it is not clear what Staal means by "meaning", or why he chooses the word "meaning" to describe these sorts of cases.

He rejects the theoretical significance of these examples (4) by pointing out that "such simple answers . . . are given rarely, and only in reply to similarly simple questions [about the meaning of rituals]." An interesting analogy then follows: ritual is like dance, and the only way to get at the meaning of ritual is by observing it, not paraphrasing it. It seems clear from this example that whatever meaning rituals have is not propositional meaning, because propositional meaning could be easily paraphrased in terms of (any suitably rich) language. At this point Staal seems to be assuming a distinction similar to, for example, the distinction between lexical and intentional meaning made by Humphrey and Laidlaw 1994, 90-91. Humphrey and Laidlaw say that "in general actions do not have a lexical meaning" (90), but rituals can have intentional meaning: "it is in grasping the illocutionary force or point of the utterance that the skater understands its meaning as an action, and it is this 'meaning as an action' that we call its intentional meaning" (91-92). Humphrey and Laidlaw acknowledge their debt to Staal, and their distinction, while not necessarily the same as his, is effectively a gloss on this aspect of Staal's theory. Thus, when Staal compares ritual to dance he means to emphasize the aspect of ritual according to which ritual is an action. After making the comparison to dance, Staal comments that "ritual, then, is primarily activity. It is an activity governed by explicit rules. The important thing is what you do, not what you think, believe or say."

While my discussion has been rather concise up to this point, it should by now be clear that a careful reader of Staal's first contribution to ritual theory will already have realized, three pages into the paper, that Staal holds that ritual does not have meaning, at least in the primary and chief sense of "meaning" found when one speaks of linguistic meaning. Any meaning that ritual has is only a secondary and derivative sort, as the four examples of ritual meaning given on page 3 indicate.

When Staal says that ritual is "for its own sake" (9) or when he denies that the "symbolic activities [of ritual] refer to something else" (3) beyond themselves, he has in mind this idea that ritual does not mean in the same sense that language means. If language means through sense and reference, then ritual means, to the extent it means at all, in some different way. Staal is free, as on page 3, to use
the word "refer" in regard to ritual, but this is no more than a figurative use of the verb, to the same extent that his use of the word "meaning" is itself figurative. Penner's assumption that "ritual refers" implies "ritual means through its reference" is thus a misunderstanding. How rituals can mean for Staal is still not clear, but it should be clear at this point that they do not mean in the same way that language means. Instead of ritual meaning, Staal prefers to talk about ritual rules: it is rules that govern ritual activity, and "the important thing is what you do" because what you do is constrained by the rules of ritual.

Section II of this paper addresses only aspects of the Vedic ritual tradition, and its later interpretation up to and through the Mīmāṃsā philosophy. Many of the things Staal says in this section will be important later, but at this point I will only mention one. Staal points out (5) that the Vedic ritual manuals describe some extremely long rituals, lasting up to a thousand years, that could never have been performed in practice; the category into which these rites fall is denoted "sattrā" in the ritual manuals. These "purely theoretical" rituals will be important in explaining the role of rules in the construction of a ritual system, because they indicate that the system is generative: new rituals can be created given a list of elementary constituents (simple rites found in multiple rituals) and rules governing the combination of these constituents. See the discussion of section VI below (1.1.4) and in section 3.4.2.

Section III of Staal 1979a is entirely negative in its conclusions. After claiming that the Indian tradition, while it "offers suggestive speculations", does not present a general theory of ritual, Staal briefly considers three modern theories of ritual, only to dismiss them. These are: the idea that ritual reenacts myth; the theory that rituals are used in small-scale societies for educational purposes, to teach social values; and a class of theories that explain rituals by reference to the distinction between the sacred and the profane. As the beginning of the next section indicates, Staal thinks that his theory will succeed where previous attempts have failed. His negative comments in section III are thus not intended to disprove earlier theories, but rather to point to failings that he takes to be widely recognized.
1.1.2. Section IV: **Meaning.** Section IV directly addresses the problem of ritual meaning. Whereas the first section had introduced the problem of how rituals can mean and pointed towards a possible solution, it is here that Staal presents his best arguments for his position.

1.1.3. **Definition of Meaning.** An important argument occurs on page 9. In section I, Staal stated his thesis that ritual is pure activity, and hence strictly meaningless. He must recognize that his readers will expect a clarification of what he takes "meaning" to mean, and in section IV he presents an argument intended to address this very concern. I believe that Staal has already attempted to clarify his understanding of ritual meaning to some extent, in section I:

5. it is different from the sort of meaning that is attributed to natural language;
6. the only true sense of meaning is that found in natural language; and
7. those meanings usually attributed to ritual are not really meanings at all, in other words the term "meaning" is used only in a secondary and derivative way in talk about ritual.⁴

At this point, Staal is faced with two choices. Either he can follow the path of other scholars of ritual and attempt to clarify those properties of ritual that have sometimes been called "meanings", for example social-functional roles or homologies; or he can clarify how these sorts of meaning differ from the true meaning found in natural languages. The latter path is the less well trodden, and promises some interesting payoffs --- payoffs which Staal does not clearly address, though I attempt to do so in Chapter 5 below. If his concern is with meaning as found in natural language, and the absence of this meaning in ritual, then there is no need to clarify the senses in which meaning is found in ritual, since these are not *true* meaning at all. This, I propose, is the reasoning behind the oft-quoted statement that:

Ritual is pure activity, without meaning or goal. Let me digress on a point of terminology. Things are either for their own sake, or for the sake of something else. If I were defending the view that ritual is for something else, it would be necessary to distinguish between such other things as meaning, function, aim or goal. But since my view is that ritual is for its own sake, I shall not bother about these differences. (9)
The apparent logic of this passage is not, I propose, the same as the actual logic of Staal's argument at this point in his paper. For Staal's apparent argument to work, he would already have demonstrated that there is no possible way that ritual could be for something else. The obvious ways to do this, either to typologize all the ways that a thing can be for something else and demonstrate how each of these is not true for ritual, or else to find general properties of such things and make deductive arguments about them, are precisely what Staal is rejecting here. I would state the logic underlying this passage as follows:

However we might understand "meaning" (true meaning, that is, meaning in the primary sense), meaning is for something else. Pure activity cannot be for anything else, so whatever is pure activity has no meaning. Ritual is pure activity. Therefore ritual cannot have meaning.

This argument is deductive, and the conclusions follow once the premises are accepted. If my analysis is correct, then Staal would need to persuade his readers that ritual is not for anything other than itself, and he would have to clarify this notion. He would also have to persuade them that for itself and for something else exhaust the possibilities: there is no third option. Staal never makes much of an attempt to do the latter, but he spends a great deal of time on the former. And the key to understanding his theory, I will propose, is understanding how he understands the claim that ritual is for its own sake. As will become clear in sections V and VI, this fact is closely tied to the observation that rituals have syntactic structure. But I am not yet ready to move on to the next section.

Immediately following the above quoted passage, Staal says:

To say that ritual is for its own sake is to say that it is meaningless, without function, aim or goal, or also that it constitutes its own aim or goal. It does not follow that it has no value: but whatever value it has is intrinsic value. (9)

Normally, one would expect that if something constitutes its own aim or goal then it has an aim or goal, namely itself; however, anything without aim or goal has no aim or goal. Yet clearly in this passage Staal treats "it constitutes its own aim or goal" as being synonymous with "it has no aim or goal", so he does not mean what the casual reader might think he means here. This is a good example
of Staal's careful, but far from transparent, use of language. Staal must mean that if something is its own aim or goal, then it has no aim or goal beyond itself. This can be phrased more concisely by saying "it has no aim or goal (besides itself)"; and this is precisely what Staal means to say about ritual. His argument will be (to anticipate my presentation, and Staal's) that rituals have the same formal structure, or syntax, as natural languages, and consequently we would expect that syntax functions for rituals just as it does for natural languages, namely that it allow rituals to refer to things other than themselves, just as sentences do. But this is not what happens. Sentences refer to facts outside of themselves, but rituals only refer to themselves.

Presumably Staal would hold that words refer to themselves and also to their meanings. This would account for the necessity of the use/mention distinction: it is because words have both functions that punctuation must be used to indicate when a word refers to itself, or when it refers to its referent. For example, in English (or at least philosophical English), mentions are traditionally marked with quotation marks; in Sanskrit, mentions are marked with the adverb iti; but in the Sanskrit grammarians, uses are marked with iti.

Thus, this passage serves the role of a preliminary definition of "meaningless", a definition that Staal never explicitly gives:

8. "Meaningless" means "having no aim or goal (besides itself)".

In addition, the passage clarifies the role that "function", "aim", and "goal" serve in Staal's terminology. These are clearly intended as synonyms for "meaning": nothing more, nothing less. Thus, example #4 above (certain bricks in the altar are consecrated so that it will rain) states a goal as Staal uses the term, and also therefore states a meaning. It is a claim made to explain why certain bricks are consecrated, but it is not (probably) the real reason why the bricks are consecrated. Rather, the claim is made because certain ancient scholars felt a need to justify the act. But Staal says that the act is its own meaning, or its own purpose. People consecrate bricks in order to have consecrated bricks. The purpose of the act is its successful completion, nothing more. We might not be persuaded by Staal's analysis, but Staal's position should now be somewhat clear. Now, we might wish to say that
rituals do have functions, for example they create a bond between participants, reinforce solidarity, boost morale, or constitute a link with the ancestors. These are all examples Staal gives (11), but he calls them "side-effects" rather than "functions". Staal is clear what he thinks we should make of such cases: "such side-effects cannot be used to explain the origin of ritual, though they may help to explain its preservation."

If we wish to distinguish various senses of "meaning", then, we will have to depart from Staal's own usage, since he explicitly rejects any such distinctions; the only exception is "value". Staal allows that rituals have value, presumably in the sense that money has value, or is a valuable: this is not in the sense that x dollars can be exchanged for y euros, but rather in the sense that people attribute value to money, and for this reason money serves as a medium of exchange. It is because money is a valuable that people are willing to exchange commodities for it. I take the comment that "whatever value [ritual] has is intrinsic value" (9) to mean no more than that ritual is valuable because it occurs, not for anything beyond itself. If it were valuable for anything beyond itself, then it would have some sort of meaning, and Staal will not allow this.

Staal's use of the phrase "original function" (14) supports my interpretation. Here we are told that the original function of ritual was "pure activity". This is just a case of ritual for ritual's sake. The original, or true, function of ritual is just the action of ritual itself.

1.1.4. Rules and Action. A discussion of the difference between ritual action and ordinary action (9) helps to clarify the role of rules in Staal's theory. If my goal is to take my suitcase from home to the bus stop, then there are no "rules" that govern my actions. I can transport my suitcase by putting it on my brother's bicycle, or on a skateboard, or in some other way (these are Staal's examples). A look at the Vedic ritual manuals will clarify why he calls these "rules": if it were a rule that I must transport my suitcase on a skateboard, then I would not be able to successfully transport it any other way. Such rules are constitutive of certain actions: they determine what it is to, e.g., transport something. Clearly, there are no rules that govern the transport of a suitcase to the bus stop: the action is identified as the action of suitcase-transport by the transfer of the suitcase to the bus stop, not by the means by which it arrives at the bus stop. Contrast this with the Agniprapayana, the transport of the fire
from the Old Altar to the New Altar in the Agnicayana. The fire may be transported between the altars in indefinitely many number of ways, but only if it is done according to fixed rules does the transport count as an instance of Agnipraṇayana. The point Staal is making here is subtle. Let "suitcase-transport" be the name of an action done under a particular description, just as "Agnipraṇayana" is the name of a (different) action done under another particular description. There are other ways of transporting a suitcase to the bus station, but they are not suitcase-transport, and similarly there are other ways of carrying fire between altars. The Agnipraṇayana is a ritual, but other forms of fire-carrying might not be.

In the previous paragraph, I have assumed that ritual actions are identified (or individuated, as the philosophers would say) both by the purpose that is achieved, and also by the description or name under which the action is performed. Ordinary actions are individuated by the former, not the latter. And it is the function of ritual rules to determine what manipulations are necessary for an ordinary action (such as carrying fire) to count as an instance of a particular ritual, such as the Agnipraṇayana. Such talk of ritual individuation goes beyond anything Staal considers, but I hope it highlights the role of rules in his theory.

Staal could describe the situation differently: he could say that an action only becomes a named action (to create another technical term) when rules are developed to govern the attribution of the name to the action. Then, suitcase-transport only could happen once rules have been determined that distinguish suitcase-transport from other means of transporting suitcases. Staal's method has the advantage that it provides a principled distinction between ordinary and ritual actions. It has a corresponding disadvantage as well, namely that a theory of ritual action is not a part of a general theory of action. But I will not consider the relative advantages of each position further.

Some of the background to Staal's theory becomes clear in this section, though this is a topic Staal does not explicitly address in this brief article; a more detailed discussion of his intellectual allegiances opens Rules Without Meaning (Staal 1989a, 1-60). Like many scholars (Sigmund Freud and Walter Burkert come readily to mind), Staal assumes that many properties of culture can be explained
in terms of certain sorts of universal, human affective states, for example anxiety. In Staal's case, I believe the most direct influence was J.C. Heesterman and his theory of the development of Vedic ritual. Thus, Staal's theory explains why "ritual activity has a pleasant, soothing effect" (10): namely, "our anxiety is greatest when we don't know why we are anxious" (12), and ritual provides an outlet for this anxiety. The key is that in ritual "we are always assured of success". As long as the ritual is performed, it is performed correctly. After all, any departure from the rules that constitute a ritual means that the ritual was not really performed at all. 8

Staal calls attention to an ironic result of this view of ritual. The function of ritual is to alleviate anxiety, but ultimately ritual action results in further anxiety instead. The problem is that there is no way of knowing whether a ritual was performed properly, in other words whether it was performed at all. Staal says:

If I detect a mistake in cooking or calculating, I perceive the result and understand the reason. But if I have made a ritual mistake, I don't notice any difference and don't see any reason. I am not even sure whether I made a mistake or not, and there is no way to determine it. (12)

The reason for this ironic result is precisely the fact that ritual is meaningless, in other words that it is individuated by rules. The correct actions in the case of ordinary action will result in the correct results, and thus the success of an ordinary action may be judged by its fruits. But there is no way to know whether ritual actions have been performed correctly, because the results are the same regardless. The fruits of ritual action are invisible (5-7) and not open to inspection.

The affective basis of ritual as a problem for study is completely separable from Staal's main argument, but it is good to understand it since it appears in a variety of places in Staal's presentation of his theory.

1.1.5. Ritual Change. A topic that has confused many readers is Staal's treatment of ritual change. In 0.3.6, this appeared as the first charge that Brian K. Smith leveled against Staal: the argument was that Staal was inconsistent, sometimes arguing that once-meaningful rituals gradually become meaningless, and other times arguing that meanings gradually accrue to rituals. I believe Staal's theory is carefully thought out, and while it does have its flaws, it does not suffer from this sort
of blatant inconsistency. Rather, apparent inconsistencies are signs that Staal's underlying argument is not always well presented by its overt expression. Thus, if a ritual is strictly meaningless, this does not mean that it is impossible for people to attribute meanings to it. The ritual does not, strictly speaking, possess such meanings, but while speaking loosely we may say that meanings accrue to rituals, as long as we understand that such meanings are not intrinsic to the ritual but rather are putative meanings only.

The case of meaning loss can be treated in the same way, by saying that putative meanings gradually fall away from a ritual over time. But at times Staal uses meaning-language to refer to real properties of events and actions, and then the situation becomes slightly more problematic. Take, for example, the Agnipraṇayaṇa. Staal associates the transport of the ritual fire from the Old Altar to the New Altar with the historic, eastward movement of the Vedic peoples into the subcontinent region. Thus the ritual appears to be a commemoration, and this would in turn appear to be an intrinsic meaning. After all, if the Vedic migration was the causal origin of the ritual, it might be hard to explain what coincidence would allow the ritual to replicate the migration without there being some referential connection between the two.

Perhaps it would help to briefly explain this example before continuing. The Vedic peoples presumably came to the subcontinent from wherever the *Urheimat* of the Indo-European peoples was located, for example Sweden or Anatolia. They brought with them eternal fires (as we know from cognate traditions such as that of the Vestal Virgins at Rome), horses and chariots with which they conquered less technologically-advanced, indigenous peoples, etc. --- this is just the traditional story of the arrival of proto-Sanskrit speakers in the subcontinent, and Staal accepts it uncritically. Therefore, it is literally the case the Vedic peoples carried fire from west to east, arriving in what is now Pakistan perhaps three thousand years ago. In the Agnipraṇayaṇa, fire is similarly carried from one hearth (and by analogy, one home) to another, and on the surface the ritual might appear to be a commemoration of the earlier, historical fact. To support this interpretation, Staal points to the text recited during the ritual (the Apratiratha, from the Taippirīya Saṃhitā) as evidence for some connection between the two events.
The Apratratha sings the praises of the warrior-god Indra, and this might appear a commemoration of the battles that the Vedic peoples waged against the earlier inhabitants of northwest India and Pakistan.

Staal analyzes this case quite concisely: "But the priests are not celebrating the ancient raids of their ancestors, of which they need not even be aware. The function of the hymn has not changed. It has become ritual, i.e., disappeared" (14). This is misleading just as his initial comments were on page 3: the thoughts going through the minds of practicing ritualists do not determine the meaning of a ritual in any case. Staal seems to implicitly accept that the historical event was a necessary and sufficient precondition for the ritual, but he does not take the further step that many scholars of religion would and argue that the ritual is therefore a commemoration. Rather, his position seems to be that when the historical event was re-performed as a ritual, it was just that, a ritual, and consequently intrinsically meaningless. For Staal, causal history is not constitutive of meaning relations, at least in the case of ritual. This is a minority position, but it seems just as viable a choice as its alternative, and I think Staal could fairly argue that the burden of proof is on his critic to prove that causal histories do determine ritual meanings.

Staal's discussion of eternal fire is explained in the same way (13): humans once had to carry fires that they found in nature, because they did not know how to make fire from scratch. But once they learned how to make fire, they continued to carry it as well. This did not mean that a referential relation was established between ritual fires and the earlier fires that were preserved for pragmatic purposes. There was a relation of similarity between the former and the latter, but similarity does not always imply a relation of meaning. Similarly, rituals may resemble pragmatic actions; this does not mean that the rituals refer to those pragmatic actions (12).

Staal's ethology thesis can perhaps best be understood in this context⁹ --- and in fact, this is the topic with which section IV concludes. Staal tells a deep-historical story according to which early man, like other animals, had the ability to form mental representations of the external world and possessed some sort of pre-linguistic means of communication. Ritual gradually evolved as a means of resolving the inevitable, psychological stresses of life. Just as now, even at this early period people began to attribute meaning to rituals, though this did not mean that the rituals became meaningful simpliciter.
And once people had meanings and also syntax, the development of natural languages was a natural result. But to understand the role that syntax plays in this story, it is necessary to look more closely at ritual structure. This is the subject of section V.

1.1.6. Summary of Meaning Section. To quickly review this discussion: Staal begins section IV with a fairly comprehensive treatment of the problem of how "meaning" is to be understood in the context of rituals. I take an oft-quoted passage from page 9 to present two claims: first, what it means to say that rituals are for themselves; and second, that rituals do not refer in the linguistic sense. From my discussion, it should be clear that Staal's position is not (as many people have assumed) "reference is the only type of meaning, and since rituals do not refer, they do not mean", but rather "rituals are like sentences in that they possess syntax, but unlike sentences in that meaning does not derive from this syntax". When Staal considers properties of rituals that other scholars might identify with the word "meaning" (for example, causal antecedents or social functions), he in general uses the term "side-effects" instead. This no doubt derives from his essentialism about rituals. The only exception is the word "value", which neither refers to linguistic meaning nor is a side-effect.

Also in this section are found three sorts of considerations. First, Staal considers the differences between ritual action and ordinary action. In clarifying Staal's position, I have been forced to introduce some distinctions that he does not himself make, but I hope these serve merely to clarify Staal's position; these distinctions are, to some extent, indebted to Humphrey and Laidlaw. Briefly, an action becomes (or corresponds to) a ritual just when there is a set of rules that identify how that action is to be performed. Rituals are individuated by rules, whereas ordinary actions are individuated by their fruits.

Second, Staal devotes some attention to his view that rituals came into being because they appeared to serve an affective need in human psychology. There is no particular reason why this discussion occurs at this point in Staal's argument, as far as I can determine. Third, Staal's discussion of ritual change that has confused many readers. I attempt to show how such talk is consistent with the view of Staal's theory that I have championed.
1.1.7. Section V: Syntax. Staal published two major papers on ritual in 1979: in addition to "The Meaninglessness of Ritual", he devoted 23 pages to "Ritual Syntax" in a festschrift in honor of Daniel Ingalls (Staal 1979c). Clearly the two works are complementary, and each addresses the main subject of the other only cursorily. But the basic theory expressed in both works is essentially the same. It therefore is slightly odd that Staal concludes his discussion of meaning (the core of his argument in "The Meaninglessness of Ritual") with the words "enough of generalities" (15), since these "generalities" are in fact the main purpose of the paper: what follows in sections V-VII are a summary of the ritual syntax paper and concluding comments.

It should be obvious from the way Staal presents his argument that ritual syntax is vital to any discussion of the lack of meaning in religious rituals, but unfortunately Staal does not emphasize this key connection as much as he ought to. And it is precisely for this reason that section V occurs in Staal's paper on ritual meaninglessness, namely to emphasize that any analysis of ritual meaning is incomplete without a corresponding analysis of ritual syntax. Section 1.2 will address the relation between meaning and syntax, so I only mention it here in passing. Chapter 3 below will consider the problem of ritual syntax in much greater detail, but since we might expect a precis of his argument in the paper on meaninglessness, section V of Staal 1979a deserves our attention at this point.

1.1.8. Rules and Vedic Examples. Staal begins section V by arguing that a justification for the inquiry into ritual rules is found in the notion of a "science of ritual" (15): statics and dynamics are understood through the laws of physics, and grammar is understood through the laws of linguistics, so ought we not understand rituals by developing laws of ritual? Though this is only at the stage of "pre-scientific groping", he asserts, we can develop some tentative rules of ritual theory by examining a complex and well attested ritual system like the set of Vedic śrauta rituals. A brief survey of previous theories of ritual structure follows (less than a paragraph), in which Staal takes aim at Levi Strauss and Hubert and Mauss. Levi-Strauss is of little use because "he offers no actual rules", whereas Hubert and Mauss (who based their theory largely on the Vedic evidence) "showed little more than that rites have a beginning, a middle and an end" (15). The way to develop a science of ritual, we should infer, is to develop rules that meet standards of scientific adequacy (simple enough to encompass a wide variety of
phenomena, but complex enough to provide proper explanations, etc.), and this is what Staal sets out to do.

In this paper, Staal presents the core of his syntactic model very quickly. The problems for his reader are quite different than they were in his discussion of meaning. Had he typologized the various senses of "meaning" (broadly understood) that he treats, then his theory of meaning would at least have been easy to follow. It might not have been persuasive, but at least his assumptions and methods would not have been so misunderstood. In the case of ritual syntax, however, the problems Staal faces are more difficult. His argument is sometimes hard to follow simply because of his compressed expression, but his use of language is, on the whole, not a problem, as it was with his discussion of meaning. Rather, the ideas he expresses are more difficult. Later in this chapter and in Chapter 3 I will consider Staal's analysis of ritual syntax in greater detail, but for the present I will limit myself to the simplest presentation that does justice to the problem and to Staal's theory. If this discussion seems challenging at times, I hope that the more detailed treatment will resolve any lingering confusion. But ritual syntax is a more difficult topic than ritual meaning, and as a result sections 1.1.8-1.1.13 may prove harder to read than 1.1.2-1.1.6.

The set of śrauta rituals (known in English as "solemn rituals") that Staal considers includes the Dārśapūrṇamāsa, the Paśubandha, the Agniṣṭoma, and the Agnicayana. Further discussion of these Vedic examples will come in Chapter 4. These rituals are listed in order of increasing complexity, and each subsequent ritual includes at least one occurrence of the former as a proper subpart. This last fact is, of course, iterative, so that the Agniṣṭoma contains two instances of the Paśubandha; it also contains a number of instances of the Dārśapūrṇamāsa, some of them being elements of the Paśubandhas that are themselves contained in the Agniṣṭoma. This embedding of one ritual in another is essential to Staal's theory. Another fact that Staal observes (16), namely that a ritualist is only authorized to perform each complex rite after he has already performed the simpler rites, is of less importance to his theory (but of great importance to the theory of Lawson and McCauley 1990, who cite Staal on this point).
Thus, it seems clear that *embedding* is one of the basic rule types of ritual syntax: one ritual can be embedded in another. The other type is *modification*. A modification occurs when one element of a ritual is changed (modified) in some way. At first this seems fairly clear, though the relevance of these rule types may not at first be obvious. I therefore will consider these two rule types at some length, and unfortunately the subsection names below are only a partial outline of the actual flow of my discussion. The following section, on "Notation", allows for a precise statement of how ritual embedding works. The corresponding discussion of modification is held off until section 1.1.12.

If we accept that the Paśubandha contains two instances of the Darśapūrṇamāsā, then this means that we could describe (very casually) the Paśubandha in something like the following terms: in the Paśubandha, some things happen in the beginning; then a Darśapūrṇamāsā is performed; then more things happen; then another Darśapūrṇamāsā occurs; the ritual ends with other things happening. In other words, to say that one ritual is a "proper subpart" of another is just to say that the one is contained within the other in a very real sense, not in some abstract way. Part of the performance (a segment in time) of the larger ritual is in fact a performance of the smaller ritual. This is a very significant point, and one that Staal does not dwell on. Perhaps a further bit of terminology will help. Goldsmith and Huck 1991 typologize theories of language as tending towards one of two poles, the distributional and the mediational, and this distinction is helpful in understanding Staal’s theory. A distributional research program is concerned primarily with accounting for the sequence of elements in a sentence; in the case of ritual theory, it would seek to account for the sequence of subrituals and ritual elements within a complete ritual. A mediational program in linguistics, in contrast, focuses on accounting for the relationship between sounds and meanings. I will return to this distinction in section 1.2, but for the present I wish to focus on the distributional character of Staal’s theory. His ritual syntax is really just an attempt to develop a scientifically sufficient description of the sequence of actions within any given ritual. This bears repeating.

Staal’s ritual syntax is really just an attempt to develop a scientifically sufficient description of the sequence of actions within any given ritual. What the relation is between description and
explanation I will pass over for now. Hopefully, however, it should be clear how Staal's ritual syntax is
distributional: it focuses attention on the individual actions that compose a ritual, and on their order of
performance in time\textsuperscript{10}. Embedding also serves to demonstrate how it is that different rituals share
common components.

In this section, I have quickly introduced several key notions that will remain important in
what follows: in particular, the concept of a rule and the idea of a distributional model are both key to
any proper understanding of Staal's theory. In addition, this section has combined a discussion of
theoretical problems with practical examples from the Vedic tradition. This is a hallmark of Staal's
presentation of his theory, and one that I will therefore have to abide by.

1.1.9. Notation. Let us begin with a definition (17): a rite is a smaller unit than a ritual; in
fact, it is what I have called a proper subpart of a larger ritual. This distinction is relative, not absolute.
An action sequence that is called a rite in contrast to a larger ritual may also be called a ritual in
reference to one of its own proper subparts. Rituals will be denoted (initially, at least\textsuperscript{11}) with capital
Roman letters, and rites with lower-case Roman letters. Thus, for example, the Darśapūrṇamāsa will be
denoted by a D, the Paśubandha by a P, the Agniṣṭoma by an A, and the Agnicayana by a C. Rites that
are part of a Darśapūrṇamāsa will be denoted by d\(_1\), d\(_2\), d\(_3\), etc., parts of a Paśubandha by p\(_1\), p\(_2\), p\(_3\), etc.
(Subscripts will thus be used to distinguish subparts of a given ritual from one another.) In other words:

\[
\begin{align*}
D & \quad \text{symbolizes the } \text{Darśapūrṇamāsa} \\
P & \quad \text{symbolizes the } \text{Paśubandha} \\
A & \quad \text{symbolizes the } \text{Agniṣṭoma} \\
C & \quad \text{symbolizes the } \text{Agnicayana}
\end{align*}
\]

and likewise
$$d_1, d_2, \ldots, d_n \quad \text{symbolizes the} \quad \text{Darśapūrṇamāsa} \quad (10)$$

$$p_1, p_2, \ldots, p_n \quad \text{symbolizes the} \quad \text{Paśubandha}$$

$$a_1, a_2, \ldots, a_n \quad \text{symbolizes the} \quad \text{Agniṣṭoma}$$

$$c_1, c_2, \ldots, c_n \quad \text{symbolizes the} \quad \text{Agnicayana}$$

Let us provide an analysis of the Paśubandha such that it is composed of exactly six proper subparts. This just means that the sequence of actions that constitutes the Paśubandha can be chopped up into six segments or divisions. If we divide the action sequence in a certain way, the second and fourth subparts will be instances of a Darśapūrṇamāsa, as follows:

$$p_1, D, p_2, D, p_3, p_4 \quad \text{symbolizes the} \quad \text{Paśubandha} \quad (11)$$

In this case, $$p_1$$, $$p_2$$, $$p_3$$, and $$p_4$$ represent the remaining segments, which according to our analysis do not constitute one of the four, named rituals D, P, A, or C. Staal introduces at this point two methods of notation, which are largely equivalent: the use of (single) arrows and trees\(^{12}\). Using the arrow notation, we can rewrite (11) as follows\(^{13}\):

$$P \rightarrow p_1, D, p_2, D, p_3, p_4 \quad (11b)$$

Similarly, (11) can be rewritten in a tree diagram:

\[\text{Diagram}\]

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The two instances of the symbol D represent identical sequences of action, so they require no subscripts, but \( p_1 \) and \( p_2 \) represent different sequences, and this fact is indicated by their different subscripts. They are both denoted with the symbol "p" because they are both proper subparts of \( P \). The two instances of D are also proper subparts of \( P \), and they could be labeled \( p_3 \) and \( p_6 \), respectively, but they are labeled "D" to indicate that they are also instances of a Darśapūrṇamāsa, as well as being subparts of a Paśubandha. That these two instances of D are proper subparts of \( P \) is indicated by the fact that they are at the termini of tree branches in (11c), and to the right of the arrow in (11b).

Note that since each D is itself composed of parts \( d_1, d_2 \), etc., (11c) could also be rewritten as a more complex tree:

![Tree Diagram](image)

(11d)

Notice here that the root ends of all tree branches are marked with lower-case Roman letters. For current purposes, we are treating these as terminal symbols: in other words, based on the analysis that (11d) provides for the Paśubandha, no further extension of any branches is possible, as had been done in the move from (11c) to (11d).

One way of interpreting the arrow and tree symbols is to say that both indicate that a given ritual is composed of certain proper subparts, or rather (in Staal's terminology, which I will adopt henceforth) rites. Rites occur to the right of (single) arrows\(^{14}\), and at the root end of branch structures in trees. Rituals occur to the left of (single) arrows, and at the apex of branch structures in trees:

\[
\text{Ritual} \rightarrow \text{Rite}_1, \text{Rite}_2, \text{Rite}_3
\]

(12)
1.1.10. **Language Syntax.** Now, how should we interpret this notation? It is clear that Staal has modeled his presentation, down even to details of symbolism, on Chomskian transformational-generative grammar. In linguistics, statements like (12) and trees like (12b) are rules, and rules can be interpreted as one of two things: means of generating sentences, or systems for assigning syntactic structure to input sentences. Take the sentence "The printer is warm." It might be analyzed in the following terms:

The analogy to (11c) or (11d) should be clear. Terminal symbols are words in a linguistic tree structure, and non-terminal symbols indicate grammatical categories. If we think of (13) as describing
how a sentence is generated, then we would think of it as describing the application of a set of rules like (14):

```
  Noun
 /    \
Phrase
 /     \
Article Noun
```

and (15):

```
Noun

  printer
       |
       v
```

A complete list of such rules is the grammar of a language. This grammar can be programmed into a machine like a computer or a brain. Whenever the root of a tree branch has a non-terminal symbol, the machine is programmed to add onto the total tree structure another branch (apply another rule) that has at its apex node the same symbol that occurs at the root of a non-terminating branch.

For example, consider a machine that is designed to generate complete sentences. It starts with an initial symbol "Sentence", then adds to this (since it is a non-terminal symbol) the second line of (13), in accordance with rule (16) (I will now switch to single arrow notation for convenience in stating rules):

```
Sentence → Noun Phrase, Verb Phrase
```

"Noun Phrase" is not a terminal symbol, so rule (14) must be applied next. "Noun" is not a terminal symbol, so the machine then applies rule (15). "Printer" is a terminal, so this branch of the tree
is complete. Follow the same procedure to eliminate all non-terminal symbols, and one has generated a sentence. Think of this as "running down the tree" to get a sentence.

If we have a sentence that we need to parse (determine the grammatical structure of), we can apply the same rules in reverse order, and "run up" the tree. Then the sequence of non-terminal symbols gives us the grammar of the sentence: "the" is an article, the sentence has an intransitive verb (its object is a predicate nominative, not a direct object), etc.

1.1.11. Applying Syntactic Methods to Ritual. Does Staal want to use transformational-generative notation in exactly the same way in ritual studies as it is used in linguistics? It would appear not.

In fact, Staal's application of this notation is not entirely clear, either in his 1979a paper or elsewhere. It does seem that Staal is proposing a generative system for ritual, and in principle each parse could be extended all the way down to terminal nodes that represent simple, atomic actions. But there are other ways in which Staal's ritual syntax diverges significantly from its linguistic prototype.

To give but one example, the rituals denoted by D, P, A, and C are not categories in the same sense that article and verb phrase are. D is simply a series of actions $d_1, d_2, \ldots, d_n$. In other words, D is the same kind of thing as $d_m$ (i.e., any element of the series $d_1, d_2, \ldots, d_n$) even when $d_m$ is an atomic ritual action, an action that cannot in turn be split into two smaller, sequential ritual actions. In other words, terminal symbols in a ritual parse do not indicate a different kind of thing than non-terminal symbols; this is not the case in a linguistic parse. The rituals denoted by D, P, A, and C are sequences of ritual actions, not types of ritual action (e.g., sacrifice, libation, song, or prayer). But the linguistic analogues to D, P, A, and C are linguistic types (e.g., noun phrase, article, or verb). D is really nothing more than an alternate name for the series $d_1, d_2, \ldots, d_n$; this would seem to indicate that Staal's syntactic approach is nothing more than a heuristic.

1.1.12. Modification. In Chapter 3 I will consider how successful Staal's adaptation of linguistic syntactic methods to ritual is. For the present, I wish only to consider issues that are explicitly discussed by Staal 1979a. Here he considers the two rule types embedding and modification.

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I have already described the formalisms used for the former in the subsection "Rules and Vedic Examples", but I still need to describe the formalisms Staal presents for the latter.

Staal gives as an example of embedding the fact that "in C, a performance of A, fourteen performances of P and numerous performances of D, some already embedded in A and P, are embedded." (11d) gives the structure of the simpler case of P, which contains two instances of D. Embedding should therefore be clear, from this example and from my earlier discussion: when one ritual forms a proper subpart of the other, then it is said to be embedded in the latter.

As an example of modification, Staal points to the fact that a given mantra (a traditional text recited during Vedic rituals) may occur at different points in a ritual; the text is basically the same each time, except that the name of the god worshipped is changed depending on the context. Thus, at the full moon sacrifice, the name "Agni-Soma" occurs, whereas at the new moon "Indra-Agni" is used. Similarly, the animal sacrificed differs depending on the god worshipped: this introduces modifications both in the action (different animals are used) and in the mantras. What we actually have here, looking at the ritual from a purely formal perspective, is similar mantras and similar actions. But Staal considers one a modification of the other, in other words if you start with one mantra you can get the other by simply replacing individual words. I will return momentarily to the notion of modification, but first let me explain the relevant notation. When Staal discusses this, he begins by arguing that modifications are not simply a form of embedding, though in my opinion this is more confusing than helpful. But since at this point I am attempting to clarify his text, I will follow his lead in what follows.

Let us assume, hypothetically, that a rite E occurs as a proper subpart in two different rituals. The first occurrence, which we will call "E" because it is the prototypical instance of E (the unmodified form), is composed of rites $e_1$, $e_2$, and $e_3$. The second occurrence will be treated as a modification of the first, and we will call it $E^*$; it is composed of the rites $e_1^*$, $e_2$, and $e_3$. We could try to represent this with existing symbols as an embedding, where $e_1^*$ is embedded as the lone, proper subpart of $e_1$:

$$e_1 \rightarrow e_1^*$$

(17)
This might at first seem appealing since applying (17) to a description of E (E → e₁, e₂, e₃) would give us a description of E* (E* → e*₁, e₂, e₃). But there are two problems with this approach. First, this looks different from the previous examples. e₁* is not a more detailed description of the action represented by e₁; rather, it is an alternative rite. So if we were to interpret the problem along the lines that (17) indicates, we would have to reinterpret the meaning of the (single) arrow operator --- even the minimal understanding that we currently have of this operator would be insufficient here.

Second, while this might give us the correct result for the purely hypothetical ritual E, a similar maneuver would not work for the Vedic ritual of the Paśubandha. Though I presented (11d) initially as a parse of the Paśubandha ritual, Staal gives the slightly different analysis presented in (19): the difference is that in (19) the second Darśapūrṇamāsa differs from the first in its initial element (d₁*).

Rule (17) would have us replace every instance of d₁ with d₁*. In P, d₁ occurs in the first performance of D; but in the second performance of D, d₁* occurs instead. Thus we need a means of selectively replacing d₁ with d₁*, and rule (17) cannot do this for us (or rather its analog, rule (17b), which applies to the ritual D, cannot). The problem is that rules like (17) and (17b) cannot be applied selectively to only some instances of the relevant rite.

\[ d₁ → d₁* \]  \hspace{1cm} (17b)

Thus, D occurs twice in P, once in its unmodified form and once in its modified form D*. If we were to apply rule (17) to the result of (18), and embed that in (11b), then we would get an incorrect description of P because then no instance of d₁ would remain unmodified:

\[ \begin{align*}
D & \rightarrow d₁, d₂, d₃ \\
P & \rightarrow p₁, D, p₂, D, p₃, p₄
\end{align*} \]  \hspace{1cm} (18)  \hspace{1cm} (11b)

Clearly a new rule type and a corresponding formalism is needed. We need a type of rule that will allow us to selectively replace one element in a sequence with a different element, in a certain
context, what we may call a *context-dependent rule*. The symbol we will use to represent this type of "modification" is the double arrow (\(\Rightarrow\)). Thus, a proper analysis of \(P\) would look like the following:

\[
P
\quad p_1 \quad D \quad p_2 \quad D \quad p_3 \quad p_4
\]
\[
d_1 \quad d_2 \quad d_3 \quad d_1^* \quad d_2 \quad d_3
\]

(\(19\))

and we could derive this tree by making the following modification of (\(11d\)):

\[
P
\quad p_1 \quad D \quad p_2 \quad D \quad p_3 \quad p_4
\]
\[
d_1 \quad d_2 \quad d_3 \quad d_2 \quad d_3
\]

\[
P
\quad p_1 \quad D \quad p_2 \quad D \quad p_3 \quad p_4
\]
\[
d_1 \quad d_2 \quad d_3 \quad d^*_1 \quad d_2 \quad d_3
\]

(\(20\))

To sum up, the double arrow indicates modification just as the single arrow indicates embedding. The double arrow joins two tree structures, whereas a single arrow joins a single item with a series of items. A single arrow is an alternate notion for a simple tree structure.

1.1.13. **Modeling Complex Rituals.** Let me clarify one point before continuing. (\(19\)) presents the most precise statement of the syntax of the Paṣubandha, according to Staal’s analysis. This is not a full analysis of the ritual by any means, but it is accurate to a degree of approximation. Similarly, (\(11b\)) - (\(11d\)) had provided slightly less precise statements of the form of this ritual; in fact, it should be clear in retrospect that even ritual E provided a very coarse model of the Paṣubandha. I thus
will describe these as *approximate descriptions* of the ritual. Clearly a *fully exact description* of the Paśubandha will be extremely complex, and at the present time I am unable to provide one. Since my concern is with a general problem in the analysis of ritual form, it is of course not necessary that I provide a fully exact description of any of the rituals that I am considering. A *fully exact description* of the Paśubandha would probably be a valid subject in its own right for a dissertation in Vedic studies. Wheelock (1980) and (1985) provide an idea of what such a description would entail.

Now, since (19) is only an approximate description, the elements $d_{in}$ are not strictly terminal elements, but I have treated them as terminal from the perspective of my analysis. This is also an approximation to a fully exact study. Wheelock estimates that there are about 1,200 "separate mantra utterances" in a performance of P (1985, 170), and this would mean that the total number of terminal characters in a fully exact description of P would be much greater than 1,200 (since it would have to include all mantras, all physical actions, specifications of ritual implements, etc.).

The way Staal addressed this problem has perhaps created some confusion. After providing a tree diagram of ritual A similar to my diagram of P --- namely diagram (19) --- he says "this picture does not correspond to any existing ritual" (Staal 1979a, 18). In fact, it does correspond to the ritual of the Agniṣṭoma, to a certain degree of approximation. It omits many details, but to include all those details would require a tree with many thousands of nodes, and such a precise description would be quite impractical. More importantly, a fully accurate description of P or A would require the analyst to determine the relationships between all the similar yet distinct rites within either of these rituals. The relation between D and $D^*$ above should serve as a simple example of what this would imply. And to chart all the modifications in a ritual of such complexity would be a major task even for a competent Indologist.

A monograph-length study of the Paśubandha ritual, then, would require at least three components: the minimal elements composing the ritual (the smallest rites, corresponding to the terminal characters of a tree diagram) would have to be identified; the similarities and differences between subparts of the ritual (not just at the level of terminal characters, but at every level) would have
to be charted; and a complete description of the ritual (in the form of an annotated list of elements illustrating the distributional characteristics of the ritual) would have to be generated.

1.1.14: Sections VI - VII: Conclusion of Staal 1979a. It is only in his concluding remarks that Staal points out the similarity that I have noted from the beginning, namely the similarity in form between syntactic rules in language and ritual. He explicitly says that single arrows (embeddings) correspond to phrase structure rules and double arrows (modifications) to transformational rules (19) in transformational grammar. He further adds that these rules \textit{really are} the rules that structure the ritual, that he has not misled his reader in his presentation. Of course, we cannot take his word for granted, but there seems no reason to question his sincerity.

At this point, Staal shifts again to his ethology thesis, arguing that ritual syntax preceded natural language syntax. He presents two sorts of arguments. The first, that animals have ritual but not language, is beyond the scope of the present study, though I will say that I find the argument, as it stands, rather insufficient. The second argument is very interesting, however. It is based on the claim that "if language were rational and adapted to its purpose, sounds and meanings would be related by means of a 1-1 correspondence \textit{sic}, and translation could be effected with the help of dictionaries only" (19). I will argue in Chapter 5 that Staal's theory is best understood as a cognitive theory of ritual; but here, at least, Staal seems explicitly to reject this possibility. Syntax allows the comprehension of an infinite number of sentences that a listener has never before heard. Without syntax, Staal is right, people could communicate through what may be called "call systems", in which meanings were matched as though directly through a dictionary. But an infinite number of meanings would require an infinite number of lexical entries, and no finite being could have the infinite memory that such a dictionary would require. In fact, it is one of the great advantages of a generative grammar that it allows the expression of an infinite number of ideas with finite means.

One example can suffice to clarify this point. Take the sentence "Staal wrote the book". We can create an infinite number of sentences based on this sentence by replacing the name "Staal" with other personal names. Of course, this would be little advantage if we required an infinite listing of personal names. But we could create a morphological marker (perhaps, "AU") that could be attached to
any sequence of sounds, indicating that that sound sequence was a personal name. Then a listener who spoke our language could recognize the meaning of "Quine-AU wrote the book" and "Coward-AU wrote the book" without previously knowing that Quine and Coward were names of people. As a practical matter, of course, most cultures have a finite number of names, and rely on context to distinguish one John from another. But the point is that we do not need to look to contextual clues in such a case. If there was a one-to-one relation between sounds and meanings, we would quickly fill an infinite dictionary just for sentences of the form "X wrote the book", another one for sentences of the form "X read the book", and another for sentences of the form "X read the sign", etc.

Thus, by taking this approach Staal seems to miss one of the great advantages of his theory. I will also point out that Staal wants to use transformations (modifications) to indicate the formal relationship between distinct rituals, and it is because of these transformations that ritual syntax is as complicated as it is. Yet it is because of the generativity of language that many sentences with similar meanings are also formally similar: thus, "Staal wrote the book" and "Quine-AU wrote the book" are both instances of the general form "X read the book", and the phonological similarity in this case does successfully point to the semantic similarity of the two sentences. So it is odd that Staal takes this stance.

The fact that syntax lies between sound and meaning in natural language is, for Staal, a sign of "redundancy" (20). But, he then adds, redundancy is not for the sake of communication, because "that assumes that language is only for the sake of communication, which it is not" (20). He does not go on to say what else language is for the sake of; perhaps he thinks that it, like ritual, serves affective ends. We might think of "sweet nothings" in this context. And, if we are to speak loosely of "ends", we might also think of much talk about the weather and sports and similar mundane conversation, which serves the end of social solidarity more than communication of novel meanings.

Staal concludes section VI with a brief listing of three topics that are illuminated by his view of ritual and language. Two of them he has published on extensively. First is the problem of mantras as meaningless sequences of sounds. Second is mysticism, which moves beyond propositional language. Third is "a curious fact" (21) that he does not explain very well. He points to some of the
myths according to which the Vedic gods fought and created with words themselves, as well as with actions. (The creation story told in the Hebrew scriptures would serve his purposes just as well.) Staal concludes from such stories that "meters and chants are like ritual in that they fail to express meaning, but reflect syntactic structure in its purest form, hence pure activity" (21). Staal must be arguing that the Vedic poets made an understandable mistake: since language reflects the structure of action, they mistook the one for the other. In the Vedic tradition, efficient results can be obtained through the proper use of language, without physical manipulation of the world, so it is reasonable to say that in the Vedic worldview, language could be productive as well as communicative. But it is a jump to go from the claim that language structure reflects the structure of action to the claim that speech can have direct physical effects in the same way corporeal actions can. At least this is what I make of Staal's claim.

Section VII contains a long summary of the construction of the Vedic fire altar, to indicate the sheer quantity of rule-following necessary for a major rite like the Agnicayana. He concludes with an attempt to make his readers feel less saddened by his conclusion that ritual is meaningless: "for all we know life itself may be meaningless" (22). I doubt this has served as much of a consolation to his readers.

1.2 Staal's Theory. The road leading to this point has been rather long, but now I finally feel ready to present a careful and systematic statement of Staal's syntactic theory of ritual. In the Introduction, I provided a context for the theory, and in the first part of this chapter I looked carefully at Staal's most oft-read paper on ritual. My reader should therefore have some idea why such a theory might be desired, and also should understand some of the ways in which Staal's presentation of the theory has mislead many of his readers. My belief is that the theory holds more promise than it has been given credit for, if only the various misunderstandings about the nature and substance of the theory can be cleared away. Some of these misunderstandings derive from Staal himself, I think, but many have been unintentional results of the way Staal has presented his position. In any case, I hope that I have cleared away most of the misunderstandings, and that the reader who has reached this point in my discussion is now eager for a systematic presentation of Staal's best arguments. What I present now,
therefore, is the substance of the theory that I find in Staal's work on ritual, but the presentation of this theory is largely my own.

Problems will as much as possible be held back for later chapters: the present section will focus on identifying the key elements of the theory and considering how they interrelate to present a general theory of the semantic and syntactic elements of ritual.

1.2.1. Three Theses. Staal’s theory of ritual is best presented in the form of three, interrelated theses. Each thesis can be understood on its own, but the full impact of the theory derives from the support one thesis derives from another. Together, and when properly understood, they constitute the theory as a whole. Individually, they are each supported by a series of arguments. These three theses may be stated as follows:

Syntax Thesis: Religious rituals have a syntactic structure.

Meaninglessness Thesis: Religious rituals are per se meaningless.

Ethology Thesis: Natural language syntax evolved from ritual syntax.

Key to understanding these is realizing that the syntax and meaninglessness theses may be stated separately, but are in fact as closely connected as obverse and reverse of a coin. Everything has some structure or other, but religious rituals are viable candidates for meaning just because their structure is syntactic. In natural languages, syntactic structure contributes to the meaning of sentences, and it is Staal's key claim that this is precisely NOT what happens in the case of religious rituals. Whatever meaning may be attributed to rituals, this meaning does not derive from any structure that is syntactic in nature --- in other words, that is usefully like the syntax of natural language. Thus, a key to understanding the syntax thesis is to determine in what ways ritual structure is both like and unlike natural language structure --- in other words, to what extent we may usefully speak of ritual syntax.

The second (meaninglessness) thesis is frequently treated as the core of Staal's theory as a whole; if this is an accurate way of viewing the theory, then a properly axiomized version of the theory will begin with that thesis and derive the other theses and subsidiary assumptions from it. I prefer not

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to follow this approach, but rather to follow my above model, presenting the three theses as parallel and supported both by each other and by a number of separate arguments based on principled or empirical evidence. However we choose to axiomize the theory, however, the thesis of interest to most people is liable to be the second; subsequent chapters of this dissertation will hopefully bring more attention to the first thesis — if any thesis stands out as more important than the others, I think it is the syntax thesis.

In any case, I wish now to consider each thesis one at a time. In the final part of this section (1.2.8), I will consider how the theses mutually support one another. At the risk of some repetition, I will present some material that already has been encountered previously (especially in section 1.1), but in different language and with an aim towards improving the way that material is to be understood.

1.2.2. Syntax Thesis Explained I: Natural Language Syntax. I have stated the first thesis as follows: *Religious rituals have a syntactic structure.* It will turn out that the notion of syntax cannot be applied in a clear and unambiguous way to ritual, and it will be necessary to clarify precisely what syntax should mean in this context. In its primary sense, syntax is just one of the types of structure that natural languages exhibit; in particular, it is what allows words to be combined into sentences. Therefore, the syntax thesis could be restated as follows: *religious rituals have the same structure as natural language down to the level of their primary constituents, namely rites or words.* Yet this still does not capture all the key distinctions that we need. Syntax needs to be redefined, to some extent, when applied to ritual, and likewise the notion of similarity does as well: ritual syntax is not like language syntax in every regard, but only in some. Yet my initial formulation has the advantage of simplicity, so it serves as a good, abbreviated description of what the thesis entails.

I will begin this subsection with a brief outline of how syntax works, using examples from English. The idea here is to illustrate how syntax works in natural languages, since Staal models ritual syntax on natural language syntax. Then I will look briefly at some notations that linguists use to describe syntax; and finally consider how this syntactic model may be applied to the structures found in religious rituals. At this stage, I do not want to worry about whether Staal’s thesis is true: Chapter 3
will consider the thesis in much more detail. All I want to do now is clarify the thesis itself, and begin to consider how one would go about determining whether it is true.

But first let me briefly digress on the word "structure". "Structure" refers to the relation between the parts of a thing. Everything has some sort of structure, and in fact everything is structured in many different ways, depending on how the observer identifies the relevant parts. Any physical thing that has physical parts therefore is structured. Thus we can give a structural description of an automobile by first listing its parts: it has a chassis, four wheels, an engine, a steering wheel, etc. Then we would describe how these parts are connected: the chassis "sits" on the wheels; the orientation of the steering wheel determines the angle that the wheels make with the central axis of the chassis, thanks to a complex series of interconnections that we could describe in detail; etc. Non-physical things also have parts: for example, in mathematics a fraction has both a numerator and a denominator. The relation of these parts is more complex, since a description of how we write a fraction does not tell us anything about what a fraction really is: to say that the numerator is above the denominator is a description not of the fraction, but of the physical mark that names the fraction. Even indivisible things can have parts: thus, a quark (unfortunately, atoms are not really indivisible, despite their name) can be divided in theory, but not physically, into its spin, color, etc.

Linguists provide multiple structural descriptions of language. Sentence structure is perhaps the most obvious way to divide a sentence, since we tend to think of sentences as being composed first and foremost of words. But a sentence can equally well be viewed as a series of sounds, and in other ways as well. What I have called sentence structure, and what is technically called syntax in linguistic literature, grade-school textbooks typically refer to simply as "grammar".

Consider a relatively simple English sentence: *The man hit the ball*. This sentence contains five words, and also five syllables since each word is monosyllabic. It contains 13 phonemes, or distinct sounds, and these phonemes in turn may each be divided into a structured set of distinctive features, much like the quark is divided into spin and color. Since words can be individuated within a given language by phonological considerations, all of these structural divisions may be made purely
through an analysis of the sound of the sentence when it is spoken: we have not yet introduced any syntactic notions.

When a spoken sentence is understood, the listener hears the sounds that compose each word one at a time, and gradually grasps the meaning of the sentence by putting together the individual words and their meanings as the sentence unfolds in time. No doubt a person fluent in English will be able to anticipate to some extent what is liable to come next in a sentence, because of his knowledge of regularities between many previously heard sentences and the context of utterances, but nevertheless the comprehension of meaning is generally a gradual process. One way of describing sentence structure, then, would be to build rules that would allow us to describe sentence structure by identifying, given the previous word in the sentence, what word might come next.

Thus, our model sentence, "the man hit the ball", contains only four unique words; and there is at least one possible, grammatical sentence that could begin with any of these four words. So we could create a syntactic theory of Linglish (a word I will coin to describe a particular, limited subset of English) that says that the first word of a sentence could come from the list (the, man, ball, hit). Another rule would state that after the word "the", the only words that can occur will come from the list (man, ball). "The ball hit the man" is likewise a possible sentence, as is "the man the ball hit the ball", so there will be a rule stating that after one of the words (man, ball), the list of possible elements is (hit, the, #). A listing of all the rules of this sort will give a description of the syntax of Linglish. The syntax of English could perhaps be described in similar terms, though there would be a huge number of rules in that case.

We have now considered one way of describing the sentence structure of a natural language. This method would (1) give a listing of all the words occurring in sentences of that language, and (2) tell us what words might occur in any given sentence, following any previous word of that sentence. If we limit our description of the syntax of a language to a description of the possible sequences of words in sentences of that language, then we will end up with a grammar along these lines. But it turns out that there are limitations to this approach.
So let us consider a second method of describing sentence structure, again taking our model sentence ("the man hit the ball") as an example. According to a standard, grade-school-level analysis of this sentence, we can say that it is composed of two parts, a subject and a predicate. The predicate in turn contains a main verb and an object of the verb. Two nouns occur in the sentence, and both are introduced, as is usual in English, with articles. One noun is the subject of the verb, and the other is the object. As it happens in this simple case, the subject noun and its article form the entire grammatical subject of the sentence, and the object noun and its article form the entire predicate of the sentence, other than the verb itself, which is simple (it has no "helping verbs"). The structure of the sentence may be illustrated by a tree-diagram, as follows:

```
Sentence
   NP       VP
     T      Verb NP
      N     T   N
  the   man   hit the ball
```

Such a diagram illustrates the fact that the sentence, at a gross level, contains two parts, grammatical subject and predicate; also, that these are each compounds formed of simpler parts. In the diagram, the subject is labeled NP and the predicate VP, for noun phrase and verb phrase respectively. The grammatical subject has only two parts, a noun and its associated article, "the". The predicate also contains two parts, but one of these contains subparts of its own whereas the other is simple. The simple component of the predicate is the verb itself, and the compound part is the object of the verb. The object has exactly the same structure as the subject of the sentence, a noun plus the article "the". This is a structural description, because it lists the parts (words) of the sentence and also describes how they are connected to form the whole sentence. There is a reason why the subject of the sentence is labeled "noun phrase" rather than "subject": noun phrase is a more general category, so this description
is simpler. This method of labeling also has the advantage of illustrating that the structure of "the man" and "the ball" is the same: both are noun phrases. "The man" is also the subject of the sentence, and "the ball" is the direct object. So from the perspective of the structure of the sentence as a whole, these two phrases serve different roles. But the internal structure of the two phrases is the same, and the common class NP (noun phrase) captures this similarity.

In fact, I spoke initially of "the man" as being the grammatical subject. Once we have the tree structure and the notion of noun phrase, we can define "grammatical subject" in terms of its class and its position in the tree: the grammatical subject of a sentence is the NP directly under "Sentence" in the diagram. Similarly, "direct object" is the NP when VP is analyzed as Verb and NP.

Now, we customarily might say that "man" is the subject of the verb "hit", and "ball" the object. This is certainly true, but in the present context it is slightly misleading. According to the present analysis, the subject of "hit" is "the man", and its object "the ball". Our customary manner of speaking is one that abstracts what we identify as the basic elements of the sentence — basic in that they give us the key elements of the meaning of the sentence. The sentence is about a man, and what he hits is a ball. But the point of the tree-diagram is to illustrate that "man" is most closely connected with "the", that the two words form a phrase. They combine with "hit" to form a sentence, but "hit" combines more closely with "the ball" than with "the man": both are parts of the predicate, and hence they connect with each other on the tree-diagram before they connect with "the man".

A key to this second approach to syntax is the notion of "constituent structure". Our first attempt to describe sentence structure looked only at the words in a sentence and considered the possible sequences that could be constructed from those words, but with this second method we have introduced a more abstract level of description, according to which different sentences might be said to have the same structure, for example Subject-MainVerb-DirectObject, or Subject-LinkingVerb-PredicateNominative. This method thus describes the structure of the sentence in abstract terms, and it does so by identifying the constituent parts of the sentence, and then the constituents of those constituents, and so on until it breaks the sentence down into individual words. This is just what the tree-diagram illustrates. And this is what is called constituent structure. As an alternative to the tree-
structure (22), we can also make use of a different formalism, thinking of sentences as being constructed out of \textit{rules}, as follows:

(i) Sentence \rightarrow NP + VP \\
(ii) NP \rightarrow T + N \\
(iii) VP \rightarrow \text{Verb} + \text{NP} \\
(iv) T \rightarrow \text{the, a, some} \\
(v) N \rightarrow \text{man, ball, player, tee} \\
(vi) \text{Verb} \rightarrow \text{hit, placed} \hspace{1cm} (23)

These two formalisms are consistent. When we wish to draw the tree-structure corresponding to a given sentence, row by row downwards, these rules instruct us how to draw each lower level. When we reach the point where all the elements at that level are terminal characters (those that are italicized in (22)), then the tree diagram has been completed. Terminal characters are formally defined as those symbols that occur to the right of at least one single arrow, but not to the left of any. Thus, "hit" is a terminal character, but "Verb" is not. Commas indicate that in any given case we should take any one element from the list.

This brief discussion of syntax has been designed to serve two ends. First, it has hopefully pumped the reader's intuitions about natural language syntax, since a good understanding of the problems of language structure will be necessary when the move is made to ritual structure. Second, it has already introduced some distinctions that will become important in the further discussion in Chapter 3. My subsequent discussion of linguistics will follow the basic pattern I have followed here: I will only consider topics directly relevant to understanding Staal's approach to ritual syntax. Thus, some of the sources I cite may seem rather outdated. A look at Staal's work on ritual, and at his publications in general linguistics, makes it clear why I have followed this particular path. In the 1950's and 1960's, Staal was an active contributor to linguistics research. Chomsky 1956, for example, includes the following in a footnote (115, n. 8): "the incorrectness of my original example was pointed out to me by
E. Shamir, and other difficulties were pointed out by F. Staal. Further, Staal 1966b was an important review for *The Journal of Symbolic Logic* of Chomsky's early writings, and Staal wrote a review (1965b) of Katz and Postal's *An Integrated Theory of Linguistic Descriptions* for the first issue of the pioneering journal *Foundations of Language*. Yet Staal stopped publishing in linguistics, and appears to have stopped following developments in linguistics some time in 1960's or 1970's. At any rate, his theory of ritual does not require any knowledge of innovations in linguistic theory after about 1965. Perhaps he simply concluded that the subtleties of more recent work in linguistic syntax were simply not relevant to ritual syntax.

Consequently my discussion will focus on issues that were current at the time that Staal was an active participant in the linguistics community, since this is the work that affects his theory of ritual. It should be pointed out, however, that Staal has written on ritual theory as recently as the 1990's, and had he wished he could have introduced considerably greater formal complexity into his theory than he in fact has. Probably this reflects a conscious decision on his part: many of the formalisms developed for linguistic syntax result from concerns regarding the syntax-semantics interface, so perhaps a ritual syntax tied to a theory of ritual meaninglessness can dispense with many of the bells and whistles of current syntactic theory. In this work, I am concerned primarily with the analysis that Staal actually has provided; in future work I expect to study the possibility that other formalisms may be of greater utility in modeling ritual structure.

It happens that the concerns evident in Staal's ritual syntax are largely similar to the concerns that drove Chomsky's early work. In particular, Staal echoes Chomsky's concern with the problem of syntactic complexity, but since Staal only implicitly addresses this issue in his publications, the fact seems to have been overlooked by many readers. My treatment will attempt to redress this imbalance, and in Chapter 3 I will consider the notion of complexity for natural and artificial languages in some minimal detail.

The complexity of formal models is just as important a problem for ritual theory as it is for linguistics, since the goal of any scientific theory is to achieve an ideal compromise between range of applicability, simplicity of description, and explanatory adequacy. A good, complex theory will explain
more than a simpler theory, and it may explain a wider range of phenomena than a simpler theory; but a bad, complex theory will introduce needless detail and therefore serve the goal of explanation less well. Staal's theory is significant because, if true, it demonstrates that two apparently quite different spheres (language and ritual) are in fact governed by the same general principles. But if the linguistic theory of syntax turns out to overdescribe ritual structure, then the theory will prove unwieldly and unnecessary. Most of Staal's critics have intuitively concluded that Staal's apparatus is unnecessarily complex, but a clear statement of how it is so has not yet been given.

As will be discussed in further detail in Chapter 3, the two approaches to language that I have described in very informal terms in this section correspond to two different types of grammar, of differing levels of complexity, namely finite-state and phrase-structure grammars. Staal's argument for a syntactic approach to ritual closely parallels Chomsky's arguments, for example in Syntactic Structures, that English cannot be modeled adequately by a finite-state automaton; rather, it should be described with a phrase-structure grammar supplemented by grammatical transformations. An alternate way of rephrasing the Syntax Thesis may be stated, then, as follows:

Syntax Thesis (II): *The formal structure of ritual cannot be described by any formalism that does not include phrase-structure rules with transformations.*  \( (24) \)

This seems preferable to other alternatives for clarifying the Syntax Thesis, for example the claim that particular rules (such as VP \( \rightarrow \) Verb + NP in \( (23) \)) apply in both natural language syntax and ritual syntax. Thus, if \( (24) \) can be found to hold true, then Staal's arguments for ritual syntax will be fully upheld. If \( (24) \) turns out not to work, then Staal's underlying intuition can perhaps be salvaged by proposing an alternative interpretation of the original claim that religious rituals have a syntactic structure \( (21) \).

Having now given a brief overview of what language syntax looks like from a Chomskian perspective, let us now consider some facts about ritual structure that, at least on the surface, may seem amenable to specifically syntactic analysis. Staal uses examples from the Vedic tradition, so for the
sake of variety I will use some from medieval Christianity. I will now develop a structural description of the Divine Office, and consider how the formalisms developed above for natural language sentences may be used, following Staal's methods, for modeling ritual structure.

1.2.3. Syntax Thesis Explained II: Ritual Syntax. Perhaps the main form of corporate worship in medieval Christianity was the Divine Office. In this section I will develop a provisional, structural description of each hour of the office and then apply it to the linguistically-inspired formalisms that Staal adapts to his treatment of Vedic ritual.

In its developed form, the Office consisted of eight hours (or times of worship) spread relatively evenly through the day. The late night worship (Matins, held generally between midnight and dawn) differed the most from the other Offices, and was the longest. The other seven Offices (Lauds, Prime, Terce, Sext, None, Vespers, and Compline) were largely variations on a common pattern, though this pattern is also to be found to some extent in the general structure of Matins. The chart below provides a rough outline of the offices for the sake of comparison; Prime, Terce, Sext, and None follow so nearly the same pattern that they are labeled "Mid Offices" in the chart and treated as identical in structure.

<table>
<thead>
<tr>
<th></th>
<th>Matins</th>
<th>Lauds</th>
<th>Mid Offices</th>
<th>Vespers</th>
<th>Compline</th>
</tr>
</thead>
<tbody>
<tr>
<td>Opening</td>
<td>Opening Versicle</td>
<td>Opening Versicle</td>
<td>Opening Versicle</td>
<td>Opening Versicle</td>
<td></td>
</tr>
<tr>
<td>Versicle</td>
<td>Psalms</td>
<td>Psalms</td>
<td>Psalms</td>
<td>Psalms</td>
<td>Hymn</td>
</tr>
<tr>
<td>Psalms</td>
<td>Hymn</td>
<td>Capitulum</td>
<td>Respond</td>
<td>Versicle</td>
<td>Canticle</td>
</tr>
<tr>
<td>Hymn</td>
<td>Versicle</td>
<td>Canticle</td>
<td>Hymn</td>
<td>Versicle</td>
<td>Prayer</td>
</tr>
<tr>
<td>Distances</td>
<td>Canticle</td>
<td>Preces</td>
<td>Prayer</td>
<td>Blessing</td>
<td>Blessing</td>
</tr>
<tr>
<td>1st Nocturn</td>
<td>2nd Nocturn</td>
<td>3rd Nocturn</td>
<td>Lauds</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Within the Office of Matins, each Nocturn had essentially the same structure:
<table>
<thead>
<tr>
<th>Nocturn</th>
<th>Opening Versicle</th>
</tr>
</thead>
<tbody>
<tr>
<td>Opening Versicle</td>
<td>Psalms</td>
</tr>
<tr>
<td>Psalms</td>
<td>Prayer</td>
</tr>
<tr>
<td>Prayer</td>
<td>Blessing</td>
</tr>
<tr>
<td>Blessing</td>
<td>Readings &amp; Responds</td>
</tr>
</tbody>
</table>

The Divine Office provides a good example of the theoretical problem at hand thanks to three facts: first, the various Offices share many common elements; second, those elements are ordered linearly in time; and third, each Office differs from the others (except for those that I have labeled "Mid Offices") either in their exact constituent parts or in the order of those parts. Thus, the system of Offices appears amenable to distributional analysis in the same way that the sentences of a natural language are. To facilitate this comparison, let me introduce some notation.

First, I will define a number of special characters, which should help in restating the above tables in a more concise fashion. Let:

A represent Opening Versicle
B represent Psalms
C represent Hymn
D represent Capitulum
E represent Respond
F represent Versicle
G represent Canticle
H represent Preces
I represent Prayer
J represent Blessing
K represent First Nocturn
L represent Second Nocturn
M represent Third Nocturn
N represent Lauds
P represent Readings and Responds (25)

These symbols will allow us to state the structure of each Office as a linear sequence of actions\textsuperscript{21}, as follows:

<table>
<thead>
<tr>
<th>Office</th>
<th>Written As</th>
</tr>
</thead>
<tbody>
<tr>
<td>Matins</td>
<td>A-B-C-K-L-M-N</td>
</tr>
<tr>
<td>Lauds</td>
<td>A-B-D-E-C-F-G-I-J</td>
</tr>
<tr>
<td>Mid Offices</td>
<td>A-B-D-F-H-I-J</td>
</tr>
<tr>
<td>Vespers</td>
<td>A-B-D-E-F-G-I-J</td>
</tr>
<tr>
<td>Compline</td>
<td>A-B-C-D-F-H-I-J (26)</td>
</tr>
</tbody>
</table>

Whether one prefers to look at the charts or the linear sequences in (26), some patterns should be obvious. First, all Offices begin with the sequence A-B. Second, they all end with I-J, except for Matins. But in fact Matins contains three types of Nocturn, each of which does end with the sequence I-J, so we might be inclined to consider N an intrusive addition: without N, Matins would in fact end with the sequence I-J. With the realization that the basic structures of all the Offices are largely similar, we could simplify the sequences given in (26) as follows. Let:

<table>
<thead>
<tr>
<th>Symbol</th>
<th>Sequence Represented</th>
</tr>
</thead>
<tbody>
<tr>
<td>X</td>
<td>represent the sequence A-B</td>
</tr>
<tr>
<td>Y</td>
<td>represent the sequence C-D-E-F-G-H</td>
</tr>
<tr>
<td>Z</td>
<td>represent the sequence I-J (27)</td>
</tr>
</tbody>
</table>

Then the sequence of actions in the Offices could be rewritten as follows:

<table>
<thead>
<tr>
<th>Office</th>
<th>Written As</th>
</tr>
</thead>
<tbody>
<tr>
<td>Matins</td>
<td>X-C-K-L-M-N</td>
</tr>
<tr>
<td>Lauds</td>
<td>X-D-E-C-F-G-Z</td>
</tr>
<tr>
<td>Mid Offices</td>
<td>X-D-F-H-Z</td>
</tr>
</tbody>
</table>

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Vespers is written X-D-E-F-G-Z
Compline is written X-C-D-F-H-Z

(28)

No Office contains the complete sequence Y, but it is clear that the basic model on which all the Offices are (in some sense) constructed is X-Y-Z. Matins differs from the other Offices by letting C stand in for the entire sequence Y, and then recapitulating the entire sequence of the prototypical Office (i.e., the pattern X-Y-Z) thrice in a much abbreviated form in the Nocturns (i.e., the shortened sequence X-Z instead of the longer prototype X-Y-Z, followed by a concluding P); Matins then closes with a unique and therefore characteristic ending, N. Put differently, Matins is at once a shortening and an expansion of the basic sequence found (with slight variations) in the other Offices: it replaces the normal concluding sequence Z with three nocturns, each of which is itself a shortened form of the prototypical sequence. It consistently omits (in the Nocturns) the sequence Y, or at least telescopes it by giving only the initial element C (before the Nocturns begin). Notice also that Z serves to mark the end of every Office except Matins. Since Z occurs three times in Matins, it is immediately followed by P, to indicate the ending of the new sequence found in K, L, and M. Similarly, Matins indicates its own conclusion with the sequence N: since it has been established long before the end of Matins that Z does not mark the end of Matins, Matins itself ends with a unique sequence --- just as each Nocturn itself ends with a sequence not found in the other Offices.

Now, the analysis I have just given is certainly not immune to criticism. In fact, I think it almost certainly is not the best analysis that could be given of the Divine Office. We will actually get a better understanding of the very process of analysis by looking at potential problems in this analysis. But before doing that, I would like briefly to compare my analysis of Matins with my analysis of the sentence "the man hit the ball".

The sentence is structured as a linear sequence of words:

\[
\text{the-man-hit-the-ball}
\]

(29)

just as the ritual is structured as a linear sequence of actions:
Is it possible to draw a tree-diagram for the ritual, corresponding to (22)? Staal would argue that it is possible, because X is composed of the sequence A-B, and the Nocturns K, L, and M each contain both of the sequences X and Z. Thus we would get the following tree-diagram for the ritual of Matins (R is the name for the entire ritual, just as X and C are names of parts of the ritual):

![Tree diagram](image)

This would correspond to the tree structure (22) for the sentence "the man hit the ball". Underlying this structure is a system of rules, as follows:

\[
\begin{align*}
R & \to X-C-K-L-M-N \\
X & \to A-B \\
K & \to X-Z \\
L & \to X-Z \\
M & \to X-Z
\end{align*}
\]

As we know from (27), further rows could be added to this tree-diagram by additional rules, since K, L, and M contain X, and hence the parts A and B; and Z contains I and J.

Conclusion: This should be sufficient to demonstrate how ritual structure can be formalized along the same lines as natural language structure: in particular, it should demonstrate what it means to say that ritual structure is a **syntactic** structure. My presentation has also aimed at laying the
groundwork for the work I will do in Chapter 3. Just because formalisms taken from Chomskian linguistics can be used in describing ritual structure does not mean that they should be.

1.2.4. Meaninglessness Thesis Explained I: The Meaning of "Meaning". I have phrased this thesis as follows: Religious rituals are per se meaningless. The real work in this definition gets done by the concepts meaningless and per se. My treatment of meaning (and thus meaninglessness) will parallel my treatment of syntax: meaning is largely a linguistic notion, and the greatest difficulty for any theory of ritual meaning lies in clarifying how ritual meaning is related to linguistic meaning. "Per se" is a bit of a shorthand, and relates to the problem of how it is that rituals could get meaning. Clearly, then, meaninglessness needs to be treated before per se, and this is precisely how I will proceed in what follows.

The word "meaning" crops up frequently in discussions of ritual. Here is a very short list of titles of scholarly papers on ritual in a variety of cultures, to illustrate how commonly meanings are attributed, in one way or another, to rituals:

"The Religious Meaning of the Samoan Kava Ceremony" 22

"Night Village and the Coming of Men of the Word: The Supernatural as a Source of Meaning among the Coastal Saami [Norway]" 23

"How Ritual Means: Ritual Circumcision in Rabbinic Culture and Today" 24

"Religion among the Lugbara [Uganda]: The Triadic Source of its Meaning" 25

"The Ritual Meaning of Corn Pollen among the Navajo Indians" 26

My point is simply that it has been widely assumed, by both scholars and laypeople, that rituals are meaningful. Any theory that denies this claim, then, faces stiff opposition. However, it happens that there is no widely accepted theory of what ritual meaning entails; in fact, there might not be any serious contender. Given this, we can begin to see why, though Staal's Meaninglessness Thesis has been vigorously attacked, there has not emerged any sustained argument against it. Most of his readers find the thesis absurd on its face, and unworthy of more than passing attention. If it is obvious
that rituals mean, in one way or another, then a thesis that rituals are meaningless must simply be misguided or naive.

Anyone attempting to support or refute the meaninglessness thesis will have to begin by developing a theory of ritual meaning. Staal himself, as we have seen earlier in this chapter, argues that this step is not necessary, since he can eliminate not just ritual meaning, but any sort of "aboutness" in a theory of ritual. But this argument is telescoped, and he does not provide enough of an outline for me to be able to fill in the blanks. So I will begin with a step that he argues is not necessary: I will attempt to build a theory (albeit a very partial one) of what ritual meaning might be, and then see what his theory of ritual makes of this notion.

The debate between Staal and his critics hinges on the fact that for many people, the claim that ritual is meaningless tends to devalue ritual. I do not think Staal's aim was to devalue ritual, and this is certainly not my aim. Rather, Staal has explicitly sought to develop a "scientific" theory of ritual, inspired by ancient Vedic studies of ritual, and his meaninglessness thesis needs to be interpreted in the context of this agenda. "Scientific" in this sense does not mean "reductive", and the claim that ritual is meaningless does not mean that all the meaning people find in rituals really isn't there. The theory is not designed to argue that people are duped when they perform religious rituals. Rather, I believe, it is intended to clarify what it means, and does not mean, to say that rituals are meaningful.

In short, there are two opinions regarding ritual meaninglessness. One holds that any claim that rituals do not have meaning is objectionable and, likely, reductionistic. This is the most common opinion held by Staal's critics. The other position is that ritual meaning needs to be carefully delimited.

Paul Horwich begins his recent book on meaning with the claim that the question What is meaning? is "one of the most urgent of philosophical questions", and then goes on to list some of the many problems philosophers have encountered in developing a theory of meaning (Horwich 1998, 1-2). I am not interested in Horwich's theory itself, but I do wish to emphasize that meaning is still a problematic notion in philosophy, as it is in linguistics. That scholars of religion are also puzzled by meaning should not be surprising. As it happens, many of the philosophical debates relating to meaning are simply not relevant to the question of whether, and in what way, rituals can be meaningful. Shortly,
I will present a list of possible ways that rituals can be meaningful, and it will immediately be obvious that this list conflates different types of issues. This list will be derived from Staal's published thoughts on ritual, and thus the various alternatives have already been introduced in the opening section of this chapter. But first, let me present a coarse overview of philosophical discussions of meaning, since the philosophical debates lie at the root of similar debates in religious studies, whether implicitly or explicitly. I will not present a philosophical theory of meaning in detail, since this would require a substantial book in its own right, but rather point out the basic issues and attempt to draw some connections between them. The idea is to provide a rough context for my discussion of ritual meaning, which will follow.

I will organize this overview by broaching two basic questions about meaning: (1) What does it mean to say that something is meaningful? This requires an analysis of the concept MEANING. (2) What sorts of things are meaningful? I will briefly consider these questions in turn, and identify the sorts of answers that philosophers have given in each case.

1. **What does it mean to say that something is meaningful?** Meaning as reference: Since we are accustomed to statements of the form "A means B", we tend to think of reference as a primary type of meaning. Clearly there is more to meaning than just reference, but many of the major battles of the meaning wars have centered on reference, and few theories have attempted to do without it entirely. Penner 1985, which I considered already in my Introduction, accuses Staal of conflating meaning and reference, and seems to imply that reference is a small part of ritual meaning, or no part at all. Yet in the early twentieth century, when formal logic was seen as the key to finally making philosophy precise and complete, philosophers spent a great deal of effort asking how various types of words got their meaning: proper names, kind terms, indexicals, descriptions (definite or other), predicates, operators, and quantifiers. There are plenty of examples of words that do not have referents in the actual world ("unicorn", "Tiny Tim", "ether"), or that are members of word classes that serve non-referential purposes ("and", "are", "some"), but it seems contrary to the way scholars of religion talk to say that ritual meaning exists, but is entirely non-referential. In light of the history of philosophical discussions of ritual, it seems odd that Penner jumps so quickly to a purely non-referential theory of meaning.

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Devitt and Sterelny 1987 present a critique of structuralism that perhaps is helpful in understanding Penner's position. They argue that, for the structuralist, "syntagmatic and paradigmatic relations" exhaust the meaning of a word", and hence structuralism "omits reference" entirely (213). If true, this interpretation would certainly explain Penner's excursus on Saussure (9-12), which I have considered earlier. Whether all structuralists must reject reference is a question that I will not address, but the basic idea behind this critique is clear: internal relations between elements of a system are simply not sufficient to allow signs to be meaningful. Devitt and Sterelny present two strong arguments in support of this position: it seems impossible to explain language acquisition if words and sentences are not tied to the world in some manner; and linguistic change, for which philology presents clear and unambiguous evidence, seems entirely unmotivated under this view. We could add a further argument from artificial intelligence: how can a robot know anything about the world, or operate in it, if there were no relation between its internal representations and objects outside its metal casing? We can conclude that any theory of meaning that makes no mention of reference is going to be a difficult theory to support.

Returning to the case of words that seem non-referential (for example, operators, predicates, and quantifiers): while some words seem not to refer, they are allowed to do this only because other words (nouns, for example) do possess reference. And it is clear even for words that refer to non-existent things what the conditions would be under which they would refer. As philosophers say, there is a possible world in which there are unicorns. In fact, many theories of meaning resort to truth conditions, or assertability conditions, or something similar, to determine when a sentence has meaning, and such cases are really little more than extensions of the simple one-to-one reference relation, though from a philosophical perspective they have much more to offer than simple, picture theories of reference.

Meaning as sense: In addition to reference, recourse is frequently made to sense, the "mode of presentation" of a word's reference. To take Frege's classic example, "Phosphorus" and "Hesperus" both refer to the planet Venus thanks to the senses that attach to those terms (when it is seen in the morning, it is Phosphorus, but when seen in the evening it is Hesperus). The idea is that sense
determines reference, and consequently if we have an account of what the sense of a word is, and an account of how this determines the reference of the word, then we have an account of that word's reference. On this view, the meaning of a word reduces ultimately to its sense. For our purposes, this line of argument demonstrates that there is more to meaning than reference (the limits of a simplistic, referential theory of meaning could be shown in a number of other ways as well). But it also illustrates, contra Penner, that we should not simply leave reference out of the equation without good reason.

Forms of utterance: corresponding to many indicative statements, there is a corresponding imperative (command), interrogative (question), and optative (wish). In such cases, we may say that the interrogative is taking a meaning and doing something with it other than simply asserting it to be the case. Of course, there is a formal similarity between an interrogative and its corresponding indicative, but when one looks to the actual use of language many examples occur where the connection between form and meaning is less clear.

Contextual meaning: Sometimes we wish to say that a sentence means something that we would never guess just by looking at the words, and without knowing the context in which they were uttered. "It's after 10 o'clock" could in one context mean "the library closes in less than an hour", and in another context "it is past curfew, so you kids should be home by now".

There are a great number of ways we could explain what meaning is. This brief discussion barely scratches the surface.

(2) What sorts of things are meaningful? This formulation is ambiguous between two questions: What kinds of things may meaning be attributed to? and What kind of thing is the prime carrier of meaning? The first question is easier to answer. Meaning is clearly a property of language, or of things like language such as thoughts or propositions. But many philosophers allow for meanings to belong to other things, for example natural signs like that expressed by the statement "smoke means fire". Here all that is meant is that a certain sort of law-like relation obtains. But the comedic assertion\(^2\) (Adams 1979) that the meaning of life is 42, while clearly philosophical, would not be viewed as a contribution to semantics. Thus, many things can be said to be meaningful, but we must beware of thinking that every time someone makes a statement of the form "A means B" or "the
meaning of A is B” that real meaning, semantic meaning, is intended. Sometimes "meaning" is used only by analogy with true meaning. Having said this, it is possible that a semantic theory can be developed for non-propositional objects, like music or dance. So the precise, outer boundaries of (true) meaning are far from self-evident.

As to the question what is the prime carrier of (linguistic) meaning, two types of answer are given. Some philosophers take lexical meaning to be primary, while others take the prime carriers of meaning to be sentences. In my answer to question (1), it will be noticed, I alternated between these two approaches. The key fact to keep in mind is that it would make no sense to say that words and sentences get their meanings independently of one another, because then we would have to explain how it is that word and sentence meanings coincide to the extent that they do (why it is that sentences containing the name "Hamlet" tend so often to actually be about Hamlet, and rarely about Billy Kwan). So either lexical or sentence meaning must be primary, and the other in some sense derivative from this primary sense. Whatever linguistic entity is the prime carrier of meaning, it is a propositional entity. That should be sufficient for our purposes.

1.2.5. Meaninglessness Thesis Explained II: The Meaning of "Per Se". According to my construal of Staal’s thesis, "rituals are meaningless" is not only imprecise, but also strictly inaccurate according to many reasonable construals of "meaning". A proper statement of the Meaninglessness Thesis therefore should be of the form "rituals are meaningless in the following senses . . . but meaningful in the following senses . . . ." My formulation, "rituals are per se meaningless", is intended to capture the relevant caveats in the longer formulation (the one with ellipses) while still being easy to phrase. Thus, "per se" in this context implies that a clarification regarding the definition of the word "meaning" is in order, and it attempts to pick out, if only very roughly, the nature of the distinction that is relevant in this definition.

In this section I will attempt to outline the key features of ritual meaning that "per se" is intended to capture. This will lead naturally into my discussion in the next section, where I will identify the chief senses of "meaning" that are acknowledged tacitly in Staal's writings, and which will be used to round out the current discussion of "meaning per se".
In the Introduction, I made a distinction between meaning simpliciter and meaning relative to an observer. The former can be taken to correspond to a two-place predicate, Mxy, which translates into English as "x means y". The latter then perhaps corresponds to a three-place predicate, Mxyz, "x means y for the person z" (though I will revisit this shortly). A commonly held view of meaning these days is that meaning is relative, in other words meaning cannot be understood as a two-place predicate only: the very idea of meaning implies some, perhaps tacit, reference to a person for whom something means something else. Continental and postmodern thought generally seem committed to versions of this view of meaning, and this view is recognizable in the criticisms of Staal made by Penner (who generally is sympathetic to Anglo-American philosophy) and Brian K. Smith (who, I suspect, is more sympathetic to "postmodern" critiques at least if we understand this word in a loose sense). If meanings are part of the natural order of things, however, then it makes sense to speak of meaning simpliciter, and this is the path that Staal seems to follow.

Clearly there are arguments for relative meaning, in certain contexts at least, that do not depend on allegiance to continental or postmodern philosophies, so I do not wish to imply that the debate between advocates of meaning simpliciter and relative meaning is purely ideological. Frequently in the case of religious and artistic symbols, for example, the meaning of the sign is underdetermined. Abstract art, for example, does not privilege one interpretation in the same way that representational art does, precisely because it lacks the property of being a picture of its object. The case of religious representations is somewhat different. In one sense, religious symbols can be viewed as multivalent simply when they have a history of divergent interpretations over time. But in another sense, theological doctrines frequently hold that the divine cannot be fully cognized by mortals: in such a case, the multivalence of religious symbols can be an indication that they overflow the limited, propositional representational capacities of humans. Such theological views are based on plausible metaphysical stories, and cannot be rejected out of hand.

In short: while I do not feel that the burden of proof lies primarily with those who hold to meaning simpliciter, I also do not feel that this constitutes the default account of meaning, short of a proper defense of meaning relativism. Ultimately, I will argue for an ellipsis-version of the
meaninglessness thesis ("rituals are meaningless in the following senses . . . but meaningful in the following senses . . ."), and all this requires is that we define a sense of "meaning" such that all accounts of meaning (simpliciter and relativistic alike) be placed within one or the other ellipsis. However, if the set of all meaning simpliciter accounts is null, then this is a fact that needs to be known, since it will affect Staal's decision to define "meaning per se" as a breed of meaning simpliciter.

As will become clear in the next section, if it is not already clear from what has been said previously, the chief sense of meaning in Staal's theory is "linguistic meaning". And while there might be debate over how we should interpret the meaning of the painting Guernica or the cross as a symbol of Christianity, linguistic meaning seems a relatively clear case of meaning simpliciter. If I say that, for me, "black" means white and "white" means black, I can reasonably be charged with simply being confused, or intentionally deceptive. Though I might speak as though this meaning inversion was really true for me, a critic could argue that inasmuch as I was using the English language, "white" means white and "black" black for me as for any other English speaker. "White" has a meaning in English, regardless of beliefs about the word or its application by any given speaker. How it gets this meaning is, of course, an open question (whether by its connection to white things, or to the property whiteness, or its contrast with other color terms in English), but the objectivity of its meaning seems hard to deny.

From this we may conclude that, if rituals get their meaning in the same way that language does, then the type of meaning that rituals have is the same as linguistic meaning. Since Staal's syntax thesis draws attention to the distributional properties of ritual, the linguistic object analogous to religious ritual is the sentence and not the word: after all, it is sentences that have syntax. If there is a relation between the distributional properties of a sentence and its meaning (as seems likely), then a similar relation must obtain for rituals. This leads to the claim, which will be explained in the next section, that the primary, and only true, sense of meaning for rituals is linguistic meaning. Then I will argue (pace Staal) that true ritual meaning must be determined compositionally, as seems to be the case with natural language. But while sentences do derive their meanings compositionally, rituals do not. Thus any meaning that rituals have is not true meaning, but only meaning by analogy. This points to a necessary connection between the syntax thesis and the meaninglessness thesis, and provides a
justification for calling true ritual meaning "meaning per se": rituals can only get real meaning in virtue of their elements and the way those elements are combined. Meaning that is attributed to them by an agent is not true, real meaning, but only putative meaning. But this gets us well beyond the present state of the argument.

It does, however, bring to mind a related question which, if not properly addressed, might lead to some confusion: where does relative meaning come from? I have sketched an argument that accounts for the source of linguistic meaning (it is a breed and the prototype of meaning simpliciter); a similar account should be available for relative meaning. The answer here seems straightforward. Statements of the form "x means y for z" are not really best understood as being of the form Mxyz given earlier. Rather, their underlying, logical structure is quite different: namely, "z believes that x means y". In other words, statements of relative meaning are covert attributions of propositional attitudes to agents. This means that statements of relative meaning are not really meaning attributions proper (statements that predicate a meaning to a symbol, i.e., statements of the form Mxy) but rather statements in which a meaning attribution is embedded in a propositional attitude. Put simply, there is an important difference between the following two forms of statement:

\[
x \text{ means } y \\
z \text{ believes that } x \text{ means } y
\] (32) (33)

There is good reason to argue that "meaning" should be understood as implying the former sort of relation, which is clearly meaning simpliciter, and to take relative meaning to be expressed by the latter. Just because someone attributes a meaning to something does not mean that the attributed meaning is in any way objective, that it really "belongs" to the thing, that the thing really has that meaning. Such attributions are mere belief, and may be true or false depending on whether the belief corresponds to an actual fact of the matter, namely whether the belief is true.

The foregoing is well short of a formal argument that we should not take all meanings as relative. But despite its feigned formality, it should point to an important intuition: if we can find any grounds, any grounds at all, for thinking that there really are meaning relations out in the world, then we

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have good grounds for treating relative meaning as putative meaning only. And even if linguistic
meaning is grounded in mere use, this is sufficient to give it a certain objectivity as a social fact. This is
all that Staal needs to support his approach to meaning, and it seems hard to deny him this.

1.2.6. Meaninglessness Thesis Explained III: The Meaning of "Ritual Meaning". I will
return to this discussion of "per se" below in my discussion of "ectypic meaning", but before that I
should provide some context for the next part of this discussion. Thus, it seems best at this point to
return to the early parts of this chapter, and recall what Staal actually said in his 1979a paper.

Here is a provisional typology of possible senses of the word "meaning", based on my
discussion in sections 1.1.1 to 1.1.6. The names I give to these various senses might not be immediately
transparent, but I will explain each of them in turn.

(1) Referential Meaning
(2) Feature-Similarity Meaning
(3) Aetiological Meaning / Meaning as Commemoration
(4) Analytic Meaning
(5) Pragmatic Meaning / Meaning as Efficient Action
(6) Megameaning
(7) Meaning as Value
(8) Functional Meaning / Meaning as Side Effects
(9) Stipulative Meaning / Meaning as Intentional
(10) Ectypic Meaning
(11) Compositional Meaning

Referential and Analytic Meaning (#1). The first type of meaning is meaning as reference,
which I have discussed at some length already: it is the most common sense of "meaning" when the
term is applied to linguistic entities.
The second through fifth types of ritual meaning are inspired by examples given in Staal 1979a; see section 1.1.1 above. Staal seems to recognize all the other types, explicitly or implicitly. The last two will be especially important for my later discussion, and I will consider them at some length: my discussion of the first nine will be quite abbreviated in contrast.

Feature-Similarity Meaning (#2). The example given by Staal is: "The patron of the ritual keeps his hands closed 'like a child in the womb of its mother, ready to be reborn'." The idea that meaning is based in similarity has a long history, and in fact is the source of the idea- or picture-theory of reference. According to this theory, a given thought is about a certain object (and not some other) because the idea and the object have similar properties. This is then made the basis on which a theory of reference for natural languages is founded. While the picture-theory is generally not very popular anymore, it is still used in explaining the meaning of one class of words, iconic or onomatopoetic words: words that sound like they mean (e.g., Latin *ululare* to describe the howling sound that wolves make). A similar approach may seem a plausible explanation for symbolic actions, where the ritual is "iconic", in other words when it appears similar to some non-ritual action. Thus, washing the feet of a statue is like washing the feet of a person. The latter is a pragmatic action with clear "meaning", as it were (foot washing is just foot washing). Humphrey and Laidlaw 1994 present a version of the Feature-Similarity Meaning approach to ritual, to give but one example.

Aetiological Meaning (#3). This is the example that Staal gives: "The fire altar is shaped like a bird because fire was brought from heaven by a bird." In the case of Feature-Similarity Meaning, similarity occurs between an idea and its object. In the case of aetiological meaning there is also similarity, but in this case between two actions. Aetiological explanations have a long history in the study of ritual. They also have a long history in the study of language, but recently have been less popular. In many premodern cultures, words were frequently explained by means of what we would now call "folk etymologies". A common form of folk etymology was one that traced the origin of a word to a particular act; this act fixed the meaning of the word, and subsequently it could not be changed. Such a process would fix meaning aetiologically in cases where the word derived its phonological shape from the name of the person, or place, or context of the event. It would fix meaning
stipulatively in other cases (see below). Again, there are cases where modern etymologists still make use of explanations of this type, though they are rare. For example, a "Pyrrhic" victory is one like the victories of Pyrrhus against the Romans in the third century B.C. In such cases, the meaning of the word is not liable to have been fixed at the time of the event --- more likely, the word only came to be used later. But the word's meaning is still determined by the event. Thus, "aetiological" refers to a way that meaning can be determined, not a class of meaning in its own right.

Analytic Meaning (#4). To say that "bachelor" means "unmarried male" is to make an analytic statement, which is to just to say that the meaning of the predicate (unmarried male) is contained in the meaning of the subject (bachelor). Thus sentences are analytic because of the meanings of the words they contain. When arguments are made that rituals have intrinsic meanings, we could perhaps describe such claims as holding that meaning is possessed analytically by the ritual. When Staal proposes that, in the Agnicayana, "the south is inauspicious, and therefore priests do not go south if they can avoid it", it sounds as though he is making an analytic claim. When we say a place is inauspicious, we mean just that no reasonable person would want to go there (presumably because bad things are liable to happen there).

Pragmatic Meaning (#5). There is a long tradition according to which rituals were seen as failed attempts at pragmatic, technological action, much as religion was seen as a failed attempt at science. This contrasts to approaches that view ritual as primarily symbolic. Thus, Staal gives the example that "certain bricks in the altar are consecrated so that it will rain." Here we are to believe that a strictly causal relationship was presumed by the ritualists. Most people would probably not give the name "meaning" to the purpose for which an action is done, but Staal seems willing to consider this as a possible type of meaning, so I will also.

Megameaning and Meaning as Value (#6 and #7). Often we say that something is meaningful when we find value in it. See my discussion above in 1.1.3. Staal speaks clearly of meaning in the sense of value on page 9 of Staal 1979a. Closely related, but broader and less amenable to definition, is megameaning. Megameaning\textsuperscript{36} is perhaps the sense most often encountered in religious studies. Megameaning refers to the inferential connections that can be drawn between the various elements of a
religious system of thought and practice: it is because of megameaning that an adherent feels that the theology of a particular religion is appropriate to its ethical precepts, foundational myths, and ritual practice. These inferential connections allow the adherent to feel that the religion is one unified system rather than a collection of poorly joined parts. I believe many of the critiques aimed at Staal's theory are inspired by a tacit commitment to megameaning, though the notion is rarely discussed explicitly.

Functional Meaning (#8). The notion that ritual serves a social function is an old one; it was the dominant theory for much of the twentieth-century, due to structural-functionalism in anthropology, as well as the reaction to it, processualism. Staal allows that ritual has a function to play in social life, but he refers to this as mere "side-effect". In fact, though functions are often called "meanings" in studies of rituals, the word is used in a very loose way here. Functional relations are often hidden from actors in a given society, and the subconscious role they play in people's thoughts and judgments can be one of the things that gives rituals their power. It therefore seems strange that Staal is both criticized for disputing the meaning claims that many informants explicitly make, yet at the same time criticized for ignoring the clear social functions that ritual serves.

Stipulative Meaning (#9). The conscious attribution of meaning to a ritual is seen by many scholars as evidence for the true meaning of that ritual. It could be viewed as similar to aetiological meaning: at a certain time, the story goes, one particular person said that this ritual would have this meaning, and ever after it has as a result had just that meaning. I have already proposed an alternative approach to this phenomenon: rather than viewing an attribution of meaning to a ritual by an agent as a relativized example of meaning-possession (with the three-place predicate Mxyz), I proposed that we take this as an example of a belief that. Then Stipulative Meaning is not really meaning at all. Stipulation is not a means by which meanings get attached to rituals, but rather an attempt to identify the meaning of a ritual. It is the projection of human intentionality onto a product of human action.

Ectypic Meaning (#10) is especially relevant to a consideration of Staal's argument, in particular the "per se" quality of ritual meaninglessness. A key notion, perhaps the key notion, underlying Staal's theory of ritual is what he expresses when he says "my view is that ritual is for its own sake" (1979a, 9, my emphasis). This is the source of his claim that ritual is meaningless, and (1
will argue) it is what connects the meaninglessness thesis with the syntax thesis, thus forming a consistent theory out of disparate components. The claim is abstruse, however, and while I am pursuing one interpretation of this property of ritual as for its own sake, my interpretation might or might not prove acceptable to Staal himself. Along with compositional meaning, ecotypic meaning is a key notion to understanding Staal's theory of ritual meaning.

In any case, my analysis of ecotypic meaning is motivated by two concerns, which it seeks to reconcile. On the one hand is a property that is particularly associated with ritual. It might or might not be unique to ritual, but it is in ritual that it becomes most clearly manifest. Often, when people perform rituals, they recognize or seek to create a connection with other people who have performed the same rite at other times and places, or even a connection with the gods who initiated the ritual or practiced it in the Ur-time. Let me tentatively called this the "reflective quality" of ritual, just until I can introduce other, more precise terminology. On the other hand, the formal structure of ritual brings attention to the ritual: because rituals are repetitive in complex ways, because different rituals contain the same components in different orders and with different expansions, rituals bring attention to their own existence. Call this, for the moment, the "reflexive quality" of ritual. In both cases --- the reflective and the reflexive --- ritual calls attention to itself. Both are, I think, viable candidates for the properties of ritual that make it for its own sake, or about itself (rather than being about other things). If, and this is an important if, the reflective quality of ritual derives from its formal structure (and thus from its reflexive quality), then we might have a good explanation at hand for the unique nature of ritual action. And this is precisely what I am going to argue.

To explain the reflective quality of ritual, let me refer to the work of Israel Scheffler 1981 and 1997 on ritual reference. Building on Nelson Goodman's philosophy of art, Scheffler proposes that rituals refer in five different ways: by denotation, exemplification, expression, mention-selection, and reenactment. Scheffler's approach may appear unnecessarily complicated by his philosophical orientation. Most notably, he is strictly nominalist in his metaphysics, and this means that for him the ritual occurs in its performance; what unites two performances, and makes them instances of the same ritual, is a shared score (1993 and 1997). A ritual score here is to be understood on the analogy of a
musical score, as a prescription of actions that allow a given performance to be an instance of this ritual and not some other. The relation of performances to their score is one of exemplification: the performance is an example, or instance, of the ritual prescribed by the score. Reenactment is the relation between two performances that exemplify the same score. A ritual can also reenact itself, since it is an example of its own score.

Perhaps a more tangible example will help to explain this terminology. Consider the second volume of Agni (Staal 1983). This is the long book that Staal published to document the 1975 performance of the Agnicayana that was heavily studied by a group of Western scholars. The second volume is a collection of articles on technical topics relating to the performance, written by a wide variety of authors; it also contains the texts of several ritual manuals relating to the Agnicayana. If one looks at these ritual texts (478-734), one has, as it were, very nearly the exact score of the ritual? If one looks at the articles earlier in the volume by C. G. Kashikar, Asko Parpola, C. V. Somayajipad, M. Itti Ravi Nambudiri, and Erkkara Raman Nambudiri (199-255), one has a more or less complete listing of all the performances of the Agnicayana in recent years. Thus, for example, we know that the Agnicayana was performed in the year 1838 in the village of Sukapuram (no. 42, page 254) and in 1916 in the village of Akatiyur (no. 8, page 252).

Adopting Scheffler's terminology, both performances exemplified the score partially recorded on pages 478-734: in other words, they were both instances of the Agnicayana. Further, the 1916 performance reenacted the 1838 performance (and many other performances as well); the 1916 performance reenacted the 1916 performance; and the 1838 performance reenacted the 1916 performance. This last statement will sound rather odd, since it says that an earlier performance is the reenactment of a future performance that has not yet occurred. "Reenactment" is used here in a technical sense rather than in the colloquial way of standard English. The statement that the 1916 performance reenacted the 1916 performance will also seem odd, though perhaps not quite so odd as the one that follows it. In any case, particular performances exemplify their scores; in principle, the score for any ritual can be written down just like a musical score can be. Staal attempts to do just this for the 1975 performance in volume I of Agni (274-697). Similarly, any performance of a given ritual reenacts
any other performance of the same ritual. Exemplification and reenactment are both species of ritual reference, in Scheffler's terminology. I am not interested for the present in the three other sorts of ritual reference that Scheffler discusses, and I will pass over them here.

Scheffler's terminology is useful, as are the intuitions on which it is based, because it cashes out what I have recently called the reflective and reflexive qualities of rituals. One of the things that rituals typically do is call to mind other instances of the same ritual. Thus, a parent might remember his own confirmation when his son or daughter is confirmed; or communion might call to mind the Last Supper, which though strictly was not yet a ritual, was nevertheless the prototype of later communions. These are examples of the facts that Scheffler intends to capture with his theory of ritual reference.

Let me clarify the value of Scheffler's theory for Staal's. Staal tells us that what makes rituals special is the fact that they are for their own sake, not for the sake of anything else. This is what he means by saying that they are "meaningless". Scheffler provides us, I propose, with a way of thinking about how they might be for their own sake, though only three of the five types of reference that he discusses seem relevant to Staal's theory: among the ways that any ritual refers/means is by pointing to other instances of itself, by pointing to the score that defines it as the ritual that it is, and by denotation (what I earlier called "reference" in my original list of possible ways in which ritual means).

I introduced the notions of reflective and reflexive qualities of ritual as tentative means of identifying ways that ritual points to itself. In the reflective case, rituals point to other rituals; I then clarified this notion further by means of Scheffler's notion of reenactment. The reflexive quality of ritual is more akin to Scheffler's exemplification, though the latter is more specific than the former. It clearly is important to Staal that rituals are constructed by rules, and he thinks that rituals are for their own sake at least in part because they bring attention to their rule-governed-ness. This is not quite the same thing as pointing to their score, which could be thought of as the set of rules that define a given ritual, but it is similar.

In short, this is how rituals are for their own sake: they point to other rituals and they point to their own structure. I will clarify how they point to one another in Chapter 3. The meaninglessness

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thesis ties in with the syntax thesis inasmuch as rituals are meaningless because they point to their own syntactic structure.

Compositional Meaning (#11). Perhaps it would be better to count linguistic meaning rather than compositional meaning in the list of meanings that may be considered for rituals. The idea would be that, however sentences of a language get their meaning, rituals must get their meaning in the same way. It could happen that sentences get their meaning thanks to aetiology, or social function, and then linguistic meaning could be reduced to one of the other types of meaning that I am considering. Certainly some sentences get their meaning (at least in part) analytically, so in the case of "no bachelors are married" linguistic meaning does reduce to analytic meaning. But more generally, I will assume, sentences get their meaning compositionally. Let me first explain what this means, and then consider how this relates to Staal's ritual theory.

There are two dominant theories of linguistic meaning. The first holds that sentences get their meaning from the meanings of the words contained within them, and from the grammatical relations between those words. This is called "compositional meaning". According to this theory, meaning inheres primarily in words (and other sentence-components). The second theory holds that linguistic meaning is primarily contained in sentences rather than words. This was the position taken by Frege, and by Wittgenstein in his Tractatus, but despite some philosophical arguments in its favor it is not very suitable to a theory of meaning in linguistics. Consider the following sentence (which we have already seen):

\[
\begin{array}{c}
\text{Sentence} \\
\text{NP} \\
\text{T} \quad \text{N} \\
\text{the} \quad \text{man} \\
\text{VP} \\
\text{Verb} \\
\text{hit} \\
\text{NP} \\
\text{T} \quad \text{N} \\
\text{the} \quad \text{ball}
\end{array}
\]
Every word contributes to the meaning of the sentence. The sentence is about a man and ball and the action of hitting that unites them. Both are singular, so we know that one of each object is involved in the action described by the sentence. Both nouns have definite articles, so we know that a particular man and a particular ball are involved, not just any man and ball. The man is the subject, and the ball is the object, so we know who is hitting whom (or rather what --- we know that balls are not persons because we know the meaning of "ball"). And so on. All of the meaning we get out of the sentence can be attributed to the meaning of one of the words in the sentence (even articles, which do not get their meaning through reference) or to the grammatical relations that are indicated either by morphemes (both nouns are singular in form and therefore in meaning) or to word order ("man" is the subject because it is the only noun left of the verb within the clause of the verb; hence the man is doing the hitting).

This, then, is an informal introduction to compositional meaning. Let us assume that sentence meaning is compositional. Why should we think that ritual meaning is also compositional? Well, if we agree with Staal that rituals are structured in the same way that sentences are (i.e., syntactically), and if we agree (as we have seen that many people do) that it makes sense to speak of rituals as having meanings, then it seems only natural to hypothesize that rituals get their meanings the same way that sentences do, namely thanks to their elements and the syntactic relations between those elements. The elements of rituals are other rituals (embedded rites) and also things that are elements of rituals, but not rituals themselves (certainly ritual actions and utterances, perhaps objects used in ritual, roles of ritual agents, sacred places, etc., would count as ritual elements in this sentence). The elements of sentences are words and morphemes (e.g., the pluralizing suffix -s in English).

It certainly is possible that both sentences and rituals are structured syntactically, and that both are bearers of meaning, but that rituals get their associated meanings in an entirely different way than sentences do. But it is more efficient if syntax contributes to meaning in both language and ritual, once we posit ritual syntax. In fact, it would seem rather odd if rituals had syntax but this syntax did not contribute to ritual meaning. It seems intuitive to many people that rituals have meanings. If early humans wanted to do things to express meanings, then organizing those actions syntactically would be a
reasonable way of making sure that the meanings they intended really did attach to their ritual actions. Then we could tell one story and account for both aspects of ritual --- we would only need one account of why people want actions to be meaningful in the first place. But if ritual meaning and syntax are unrelated, then we need a second story as well, this time to account for why rituals are structured syntactically.

Staal avoids this problem by holding that rituals are syntactic but meaningless. Thus he does not need a separate account of why rituals are meaningful, because they are not according to his view. But he still needs an account of how languages get both meaning and syntax, and for this he relies on a single causal story, which also accounts for how rituals are syntactic. That story is what I call his ethology thesis, and it is that to which I now turn.

1.2.7. Ethology Thesis Explained. I have phrased this thesis as follows: Natural language syntax evolved from ritual syntax. The basic idea is straightforward: ritual syntax evolved before natural language syntax, and the latter came into being out of the former. Rituals are per se meaningless, but language is per se meaningful, so according to Staal's theory meaning did not become associated with syntax until the evolution of language. Staal has argued for this thesis in a number of places, and his argument is basically the same everywhere. Unlike the first two theses, which are empirically-derived and synchronic, this thesis presents an inquiry into prehistory. Thus, while the first two theses can be critiqued through a combination of theoretical argumentation and the examination of contemporary or historically-attested evidence, the ethology thesis relies on the backwards extrapolation of contemporary evidence relating to human and animal communication and ritual and social action. It would seem that Staal's presentation would therefore be replete with citations of evidence from a variety of sources, and complex argumentation. Unfortunately, he presents little of either. While he is sometimes rather careless in his discussion of linguistic and ritual evidence, he is at least well qualified to work in such areas. But he makes it clear that he is not a biologist or an ethologist, and clear that he is not really qualified to develop novel theories in these areas. Yet he does precisely this. I am inclined to think that the ethology thesis is the weakest link in his argument, and to consider it in as little detail as possible.
Staal finds this thesis on several key assumptions. First, he takes it for granted that humans have language, but animals do not. Second, he assumes that animals do have rituals. Given a simple, unilinear evolutionary model, it would therefore seem that primitive or proto-humans, like animals, had ritual but not language. I am inclined to think that both the assumptions that ground this argument are highly debatable, but if they are granted then the conclusion that ritual preceded language might follow. It would be nice to see if there is any relevant evidence, however --- for example, reasons to think that proto-humans had capacities for language that other, current non-human animals do not. The term ritualization (Ritualisierung) was coined by German ethologists to describe the situation in which "the original function of a behavioral pattern is no longer visible or known" (Staal 1985a, 554 and 1989a, 285). Now, it should immediately be obvious that it is not clear that such behaviors should be taken to constitute a scientific kind or category, since they rely on the scientist's ability to account for the origin of the behavior more than any property of the behavior itself or the animal exhibiting it. Animal ritualization tells us more about the ethologists observing the animal than it does about the animal itself. Setting that complain aside, it should also be clear that animal ritualization is defined and conceptualized very differently than religious ritual among humans --- even Staal does not argue that religious rituals are any behaviors for which the original function cannot be determined. So the association of the two seems purely a result of an unfortunate choice of technical terminology. This argument should, by itself, be sufficient to throw Staal's ethology thesis into doubt.

Another aspect of this thesis seems equally puzzling, however. Staal uses the complex structure of natural language syntax to argue that rituals are for their own sake, citing Jerry Fodor's statement that "nobody in his right mind would attempt to build an artificial language based on phrase structure plus transformations" (cited in Staal 1989a, 138). It seems more likely to him that syntax evolved in order to draw attention to its own complexity than that it evolved in order to express novel meanings efficiently. Yet I would be inclined to make just the opposite argument. The more complex language syntax, the easier it is to express complex thoughts simply --- up, that is, to the point that such syntax becomes difficult for the brain to process. And in principle it should be possible to explain
language syntax by reference to the logical structure of thought on the one hand, and the neurological organization of proto-human brains on the other.

1.2.8. Mutual Relations Between Theses. No matter how one makes the case for the ethology thesis, it seems that it is poorly supported and not worthy of serious consideration. However, if the syntax thesis is correct, then some explanation would be necessary as to why that thesis is true. The ethology thesis probably cannot shoulder the load, but if rituals and natural language do have the same syntactic structure, then a complete theory of ritual structure would have to give a reason why this is the case.

Thus, the syntax and meaninglessness theses are, as I said in section 1.2.1, like obverse and reverse of a coin: they are almost different ways of looking at the same problem. Each mutually supports the other, and we can learn about one by considering the other. The ethology thesis, however, is much less closely connected, and could be entirely abandoned without losing any consistency. Some sort of thesis would have to take its place eventually, if we were to have a complete theory of ritual, but if we were willing to give up historical explanations and settle for a functional explanation of the ritual faculty, then the first two theses would be quite sufficient on their own.

1.3. Arguments in Favor of Staal's Theory. I wish to close this chapter by briefly considering three scholars who have fought the current and attempted to further develop Staal's theory of ritual. Richard Payne and Roy Edwin Gane were both students of Staal's, and I will be considering their research in doctoral dissertations for which Staal was a reader. Kristofer Schipper is a French scholar of Taoism who has collaborated with Staal on research in comparative ritual theory. For Payne's work, I rely on the 1991 republication of his doctoral dissertation (GTU, 1985) by the International Academy of Indian Culture and Aditya Prakashan (New Delhi), though subsequently he has written several articles on the same topic. For Gane, I rely on his 1992 Berkeley dissertation. Not only was Gane's book never republished by a commercial house, for some reason it was never even included in Dissertation Abstracts. It thus appears to have been almost entirely overlooked by the scholarly community, and this is a shame. While both scholars appear to have made real contributions to the study of particular ritual traditions, Gane's work is significantly more sophisticated and
interesting from a strictly methodological perspective. For Schipper's writings, I rely on a single recent article that summarizes his earlier publications. My goal in this section is only to consider these publications for their (generally positive) criticisms of Staal’s theory, not to consider them in any more comprehensive way. I conclude this section with a look at arguments in favor of a syntactic analysis that favors symmetrical patterns.

1.3.1. Payne. Payne's dissertation provides detailed descriptions of several goma (fire) rituals within the Tantric form of Japanese Buddhism known as Shingon. In addition to discussing the historical and cultural context of these rites, he devotes a substantial portion of the dissertation to providing what I call "distributional analyses" of the fire rituals: he identifies the elements of performance and describes the sequential execution of those elements. Staal was a reader on the dissertation, and Payne's approach is heavily influenced by Staal's approach to ritual syntax.

Payne uses the relatively simple Jūhachi Dō as an initial example of the structure of all the goma rituals, and then describes the more complex rituals on this model. The Shingon ritual manuals provide a wide range of structural descriptions of the Jūhachi Dō, but Payne identifies two analyses as particularly traditional, one dividing the ritual into five parts and the other into nine. Following Taisen Miyata, he calls the five divisions Purification, Construction, Encounter, Identification, and Dissociation; Encounter he subdivides into Invitation, Greeting, and Feasting; Dissociation he subdivides into Departure of the Deity, Dissolution of the Sacrificial Site, and Departure of the Practitioner. The division into nine parts, called by Miyata "Dharmas", is as follows: Dharma of Endowing the Practitioner, Dharma of Samantabhadra's Vows, Dharma of Setting the Earthly Boundary, Dharma of Endowing the Dōjō, Dharma of Requesting the Deities, Dharma of Sealing the Dōjō, Dharma of Pūjā, Dharma of Recitation, and Dharma of Latter Pūjā and Upāya. To reconcile these two analyses, he views the first two of his five-fold division as each being bipartitioned, hence accounting for the first four Dharmas; the Encounter division accounts for three Dharmas; and the final two divisions correspond to one Dharma each. Thus:
A. Purification
   1. Dharma of Endowing the Practitioner
   2. Dharma of Samantabhadra's Vows

B. Construction
   3. Dharma of Setting the Earthly Boundary
   4. Dharma of Endowing the Dōjō

C. Encounter
   5. Dharma of Requesting the Deities
   6. Dharma of Sealing the Dōjō
   7. Dharma of Pūjā

D. Identification
   8. Dharma of Recitation

E. Dissociation
   9. Dharma of Latter Pūjā and Upāya (34)

The section of the ritual that most clearly marks it as Tantric is the stage of Identification, where the practitioner becomes the god who is worshipped. Payne considers this the core of the ritual, and though this is the fourth of five stages, he calls this the "center" of the ritual. Taking advantage of the tripartite substructure of the Dissociation stage and defining a "characteristic [ritual] pattern" that he calls "terminal abbreviation" (92), Payne argues for an analysis of the ritual that is perfectly symmetrical around the central rite of Identification. Below I attach (following Staal and Payne) asterisks to symbols in order to mark rites that are paired with unasterisked rites:

<table>
<thead>
<tr>
<th>Rites</th>
<th>Symbols</th>
</tr>
</thead>
<tbody>
<tr>
<td>Purification</td>
<td>A</td>
</tr>
<tr>
<td>Construction</td>
<td>B</td>
</tr>
<tr>
<td>Encounter</td>
<td>C</td>
</tr>
<tr>
<td>Identification</td>
<td>D</td>
</tr>
</tbody>
</table>
Dissociation

Departure of the Deity \( C^* \)
Dissolution of the Sacrificial Site \( B^* \)
Departure of the Practitioner \( A^* \) (35)

Thus, Payne sees a framing structure (on framing structures, see section 3.2) in which elements of type \( A/A^* \) enclose elements of type \( B/B^* \), etc. In what follows, I will (adapting Payne's terminology) call the sequence \( C^*-B^*-A^* \) the "mirror-image rites", since they form the mirror-image to the sequence \( A-B-C \).

Payne defines terminal abbreviation in these terms: "a part of a sequence of actions may be deleted or the number of repetitions of an action may be reduced" (92). This would appear a somewhat rough-and-ready definition, perhaps to leave room for further clarification on the basis of further empirical research. We are therefore justified in asking whether there are constraints on what can be deleted; what the difference is between deletion and lack of repetition; and whether the "or" is inclusive or exclusive.

If, motivated by Staal's analogy between ritual and language, we were to look for similar formal features elsewhere within linguistics (broadly interpreted), we might be inclined to compare terminal abbreviation to the metrical phenomenon of catalexis, according to which one or two syllables are cut off of the final metron of a period. If this were a proper analogy, we would expect the final portion of the mirror-image rites to be omitted. As it happens, the analysis that Payne presents in (35) does not meet the first criterion he lays out in his definition ("a part of a sequence of actions may be deleted") if we interpret this as corresponding to metrical catalexis, so this constraint does not apply to the Jūhachi Dō.

Next, we may wonder whether the mirror-image rites form a shorter sequence than the initial sequence \( A-B-C \). This would mean that the net number of actions would be reduced by omission of some elements among the \( N^* \) sequence, but not necessarily by the final-most elements. When, in Appendix I, Payne finally presents a detailed analysis of this rite, he subdivides \( A^* \) into six parts, one of
which is itself divided into three. A, in contrast, has 24 divisions, some of which are themselves subdivided. B only has five subdivisions, and C has 13 subdivisions. The divisions of C' and B' do not seem, from his analysis, to form two distinct units; rather, the first part of section 9 (i.e., C' and B') seems to be somewhat mixed. However, it does in general seem that the elements of E that correspond to elements of A-C are fewer in number, with many elements of A-C simply not having any corresponding element in E. Thus it would seem that there is deletion in the mirror-image rites, though it is not clear from this evidence alone whether there are rules that govern what elements are deleted. It would appear that "deletion" is a better characterization rather than "reduction in repetitions", since the deleted rites are not identical to one another. But in another ritual the rites may be identical, and then it would be hard to determine which characterization is preferable.

Of all of the different analyses that Payne considers, none is absolutely privileged in his eyes, even (35), which he presents as his best proposal. Rather, they all are simply "heuristic device(s)" for understanding the structure of the ritual. This differs substantially from my cognitive take on Staal's theory, and I think it differs also from Staal's scientific approach, since in both of these cases there is one privileged structure that is naturalized through some version of ritual theory.

1.3.2. Gane. Gane accepts Staal's general Syntax Thesis without question, and views the empirical part of his dissertation as a test case intended to extend to other traditions the "provisional" model developed by Staal on the sole basis of Vedic evidence (3). He applies Staal's theory to evidence from Israelite, Babylonian, and Hittite ritual traditions. However, he significantly reinterprets the Meaninglessness Thesis, and he rejects Staal's overall theoretical position. Staal seeks to contribute to the "science of ritual" and is concerned with explanatory theories, but for Gane "as with any other behavioral science, the goal of the modern analyst is to understand a given phenomenon without, as far as possible, imposing his own additional layer of subjectivity upon that which he is studying" (10; my emphasis).

In addition to Staal's ritual theory, Gane also relies on a version of systems theory developed for business management. He does not explain why he has chosen this particular model, but he is clear that systems theory is important because it captures the properties of ritual that obtain because rituals
are actions. Though they differ from mundane actions, rituals are a class of action and will possess whatever properties are true generally of actions. The dual nature of rituals means that rituals must be approached in two ways, descriptively from an "intrinsic activity perspective" and interpretively on a "cognitive task level". When Gane uses the word "cognitive", all he has in mind is that rituals get meaning because people attribute meaning to them. Here his position is close to Staal's, in that meaning is not an essential property of rituals. Thus "cognitive" means something like "intentional" and has little to do with its use in areas like cognitive psychology. He does footnote Lawson and McCauley (5) to explain his use of the term, but his use differs substantially from theirs, though he seems unaware of the fact.

Gane gives an interesting twist to the problem of ritual meaning. While he ultimately agrees that rituals are per se meaningless, he attempts to argue that ritual syntactic analysis must be preceded by a level of semantic analysis. This results from the nature of his evidence. Staal's sources for Vedic ritual were the Sanskrit ritual manuals, written by scholars who had a theory of ritual structure. They therefore are not simply objective descriptions of the actions that occur in a ritual: they present this information in a generative format, or as Staal sees it, they provide the rules according to which the Vedic rituals were structured. If we can call the Vedic texts that Staal studies "scientific", we must call the Middle Eastern ones that Gane studies "prescientific". As a result, Gane has to deal with a problem that Staal does not, namely the problem of ritual individuation. After summarizing the evidence from a Mesopotamian text, he asks "are there three separate rituals here, or is there one, or is this group of activities part of a larger ritual?" Staal treats the rites that are given names by the Sanskrit ritualists as independent units, and I will argue in Chapter 3 that he might err in doing so. Just because the Sanskrit ritualists were scientific does not mean their analyses were always accurate. As students of ritual, we need to ask the evidence itself how we ought to draw the lines between one ritual and the next. In seeking a way of doing this, I look to methods that have a long history in modern linguistics. In any case, because Staal views his sources as scientific scholarship, he feels he can presume that they have already answered the problem of how Vedic rituals are individuated. Gane cannot make this assumption.
To understand Gane’s dilemma, consider natural language. Sentences can be broken down into words and morphemes largely on the basis of phonological considerations, and these are purely formal criteria, not semantic ones. Admittedly, semantic criteria are necessary to individuate phonemes, but once we have identified these minimal units of utterances, then we can proceed on formal criteria alone. Gane argues that semantic (and interpretive) criteria are needed to determine what action sequences constitute distinct units, and only at that point do we have the option of continuing with a purely formal analysis. Though I will argue against this position, it might turn out that Gane is correct. This would mean that rituals are themselves meaningless, and their formal structures cannot be described without imposing that structure on them from without. So if rituals are meaningless, they are also strictly speaking structureless, because the initial stage of principled division of a ritual sequence into its component parts would not be possible. No possible structural analysis would be any more correct than any other, just as no plausible interpretation is more true than any other. Of course, intentional agents could project their own meanings onto rituals, then individuate them, and finally proceed with a strictly formal syntactic analysis. This is an interpretive approach, since the meaning found in the ritual is not a natural part of the ritual, but comes from the observer. This would sabotage Staal’s naturalistic approach to ritual studies, and therefore it seems an odd position for Gane to take. As I say, my approach in the end might not find sufficient support from the evidence, but at least my approach is more faithful to Staal’s basic program. And I never have said that I think Staal’s theory is correct. I think it is very interesting, and worthy of the detailed study necessary to prove or disprove it. Perhaps structuration and meaningfulness go hand in hand. Perhaps the best way to save Staal’s approach is not to give up on structure, but rather to give up on meaninglessness.

1.3.3. Schipper. Taoist ritual, which is Schipper’s specialization, is similar to Vedic ritual in a number of ways. For example, ritual areas may be consecrated anywhere for the duration of a service, and thus are generally not tied to a particular location; also, they are frequently performed by a number of priests who may perform different tasks simultaneously. On the other hand, the people who commission Taoist rituals are not restricted to particular classes or confessional commitments, in keeping with the fluidity of Chinese religions generally --- this forms a stark contrast to the caste-
oriented nature of much Indian, and particularly Vedic, religion. Since Schipper seeks to provide a syntactic analysis along the lines that Staal has laid out, he presents Taoist ritual with an emphasis on formal structures that is often lacking in most discussions of Chinese religions. Unfortunately, his sparse, social scientific writing style sometimes obfuscates what should otherwise be a quite transparent argument. I will not concern myself with details of his analysis, but rather will look only at his general strategy.

Schipper lists the structures of Taoist liturgy under the following categories: basic forms, rites, rituals, and services. The first are elementary forms, and all of the latter are combinations of the former, of increasing length and complexity, performed linearly in time. He subdivides the basic forms of liturgy into the following categories: clothing and masking, movements, other bodily functions, transformational activities, (non-linguistic) sound making, linguistic actions, graphic actions, mental functions, and measurements. These are heuristic categories intended to facilitate his presentation, and are not defined (it would seem) within the tradition itself or through a scientific study of the rituals themselves. In contrast, the elementary structures of Staal's analysis are defined within the tradition (since it is "scientific" --- see above in 1.3.2), though only at the level of action sequences (rites and rituals) and not at the level of Schipper's basic forms. The majority of rites that compose most rituals come from a relatively short list, and they exhibit a "usual order of performance" (104), in other words, some rites tend to precede others in whatever rituals they are embedded. Rituals can be categorized as either "major rituals" or "exclusive and special rituals", though in either case their component parts are mostly the same. The only difference between instances of a given ritual lies in "the intention proclaimed in the memorialis presented during virtually all rituals" (107).

Unfortunately, Schipper becomes less clear when he moves from his categorization of ritual components to his syntactic analysis. It appears that the large-scale structure of Taoist services (services are sequences of rituals) follows a standard model, and this structure seems to correspond to a structure common within each ritual itself. This basic structure is conditioned by two factors: the fact that rites tend to occur in the same order wherever they are embedded, and the fact that all ritual sequences tend to begin with introductory actions and conclude with closing actions. The latter fact is
just a property of successful action generally, whether it be ritual or pragmatic. Schipper is therefore picking up on the same idea that Payne also finds in Staal's work: namely, the symmetry so commonly found in rituals. This is clear from Schipper's syntactic parses, all of which present mirror-image, or framing, structures. Schipper does present a short listing of the sequence of actions in one ritual that, by his own admission, diverges significantly from the model followed by most Taoist liturgies, namely the "Presentation of the Memorial" (110). Yet in his subsequent analysis he then looks for the symmetrical pattern that can be found in this ritual, and he concludes that he has found "a clear instance of 'embedding' " (111).

Having thus considered the patterns that Schipper finds in Taoist rituals, let me now consider what use he makes of the rule types that Staal identifies in his Vedic evidence. It is clear that, looking at his analysis from the perspective of Staal's model, embedding occurs in the fact that a finite set of rites is found in most rituals, and a finite set of rituals is found in most services. After all, rites, rituals, and services are all ritual action sequences, of increasing length and complexity, and they could be seen as corresponding roughly to ishlist, animal sacrifices, and soma sacrifices in that larger rituals contain smaller rites as proper subparts. The finite set of basic forms that is found in all sequences would correspond to phonemes in language; curiously, Staal does not explicitly present a comparable listing for Vedic rituals, though he certainly could do so. Transformations (as Staal defines them) would seem to be less common in Taoist ritual than in Vedic ritual, occurring mostly in the statement of intentions in rituals. Just as variations in Vedic rituals may occur in the deity addressed, so in Taoist ritual they occur in the statement of intent. It is, of course, possible that more variation within component rites and rituals will be found when the Taoist canon is more carefully studied. The Taoist liturgical tradition has received much less attention in the West than the Vedic tradition, and while Schipper is under contract with the University of Chicago Press to produce a *Handbook of the Taoist Canon* (Schipper 1995,101) and other publications are underway, the field is still largely unexplored.

Schipper's claim that "since rituals are often expansions of rites, and services expansions of rituals, we may expect the organization to be repeated" clearly does not follow. Each level may contain multiple components at lower levels, and yet each level could be organized in quite different ways. In
fact this is how language works — phonological rules look very different from syntactic rules, for example. Schipper has helped substantially in organizing the data on the Taoist canon, but clearly much more work needs to be done before a true syntactic analysis can be performed on his evidence.

Like Gane, Schipper seems less committed to the Meaninglessness Thesis than the Syntax Thesis. Actually, Schipper seems to agree on the whole with Staal, though he sounds less radical in how he expresses himself. Attributions of meanings by modern scholars "never amount to more than the assumptions on which they are founded", and "the same holds true to some extent for diverse traditional interpretations" (112). Thus it seems that for Schipper, as for Staal, meaning is not an intrinsic property of rituals, though any meaning may be attributed to rituals. "People insist on giving their experience meaning" (120). The Taoist tradition provides "global interpretations", in other words spheres of meaning such as those originating in the Chinese bureaucratic system that are available for ready attribution on demand to any apparently symbolic action. The one place where Schipper seems to diverge from Staal is in the distinction he draws between "implied meanings" and "underdetermined meanings" (115-116). Thus, Schipper seems to agree that no rituals are intrinsically meaningful, but he finds that the meanings people attribute to them are sometimes worthy of the attention of ritual scholars.

1.3.4. General Themes. My summaries of the work of three scholars was extremely brief because I wanted to emphasize a few major points, which I will now highlight.

Let me begin by looking at the relation between meaning and individuation. Both Payne and Gane clearly recognize a problem that Staal has never addressed, namely that the individuation of ritual (i.e., determining what constitute the elementary structures out of which a ritual sequence is constructed) is a non-trivial problem, and one that is hard to separate from considerations of meaning and general approaches to theory construction. Payne essentially rejects Staal's goal of naturalizing ritual structure: he implicitly assumes that there is no privileged structure, and instead holds that formal analysis is purely heuristic. Gane accepts Staal's goal of naturalizing structure, but holds that it is not possible to do so, at least not completely. His solution, which Staal explicitly opposes (though without reference to Gane specifically), is to say that rituals cannot be explained except within the confines of an interpretive enterprise. One would normally expect a naturalistic and explanatory
framework to coexist, but Gane assumes that in the case of ritual structural analysis this is not possible. Basically, I think it needs to be proven that rituals cannot be individuated without reference to meaning. Gane has assumed this and not shown it. Otherwise, if we are to attempt a ritual theory along the lines of Staal's, we would assume for the sake of argument that such principled individuation is possible, and question this assumption only if we have practical problems. Schipper assumes a criterion of individuation, which he does not provide us with. He does not seem aware of the problem at all.

Let me also say a few words about symmetry and ritual syntax. All three of the scholars considered in this section have picked up on an argument that Staal makes according to which many rituals tend to form symmetrical, mirror-like patterns (for reasons of space, I have not addressed this in the case of Gane). I will devote considerable attention to this problem in Chapter 3, but for now I would simply like to point out that this might be a particularly poor theme for them to press. I will be arguing that Staal's program requires a distributional analysis, in other words one that begins with a study of the actual sequences of actions that are found in particular rituals; only after this is achieved can the overarching structural patterns found in the ritual be recognized. If we begin with an overarching model that we expect to find in a ritual, we are liable to fall into the very trap that Gane implicitly accuses Staal of falling into --- namely, we will presume an individuation of the sequence of actions that might turn out to be incorrect. This will almost certainly render subsequent analysis wrong as well, and thus is a mistake that we should avoid at all costs. But it is a trap that all three scholars seem to fall into.

1 These examples form the basis of claim #7 below.
2 See claim #5 below.
3 See claim #6 below.
4 Notes above indicate where I have introduced these claims already.
5 It is possible, of course, that this ritual was chosen over some other because at some point in history a ritualist reasoned that consecrating bricks could bring rain. This would then be a true historical account of the origin of the practice, but it would not constitute an explanation for Staal. Staal does not present his own theory of explanation, but presumably he would say that the fact that a ritual occurs can be explained, but the content of the ritual cannot be explained. I will not pursue this at more length in the present context, however.
6 I borrow this term from Humphrey and Laidlaw 1994, who adapt it from the terminology of action theory.
7 I created this name for the present context. There is no such thing, for Staal, as suitcase-transport, and this means just that suitcase-transport is not a ritual, but merely an ordinary activity.
I presented this argument, which is really tangential to the issue at hand, in a paper delivered in November of 2002 at the American Academy of Religion meeting in Toronto. I will therefore not repeat the details of the argument here. The general outlines of the argument should be obvious from what I have said.

Let me mention again that I find the ethology thesis to be the weakest element of Staal's theory. When I demonstrate how it fits into his theory, I should not be interpreted as approving of this thesis. Admittedly, it is quite common for a number of actions to happen simultaneously. This is not a problem for a distributional theory of ritual, provided one is willing to graph the sequence of actions in more than one dimension. In the examples given here, however, the assumption is made that the relevant ritual elements (all of which are themselves complex action sequences) can be ordered in linear time. A ritual description that accounted for simultaneous actions would have a multidimensional matrix to the right of the arrow, in terms of the notion that I will shortly introduce, rather than a linear sequence.

Since we can view many action sequences as either rites or rituals, a given structure can be described with different sequences of capital and lowercase letters.

Actually, tree structures indicate the application of a sequence of rules. Single arrows are equivalent to tree structures of two levels in that both indicate the application of only one rule. Strictly speaking, the single arrow does not represent the predicate "symbolizes". The proper interpretation of the single arrow will be discussed shortly.

No pun intended, though this provides a useful mnemonic.

Assume that the scope of the language in question does not include linguistic concepts: thus, "sentence" is not a word in the language, and thus is not a terminal symbol (though it is a word in English).

Note that I use the term "distributional" in a different way than linguists do: see section 3.3.4.

The example and some of its analysis below are taken from Chomsky 1957, 26-27.

For example, when listening to a telecast of a football game, a listener might expect the partial sentence "the receiver caught the --- " to be followed by the word "pass", or maybe "ball", and he would be rather surprised if the sentence concluded with "intent of the quarterback to run the ball in himself". The symbol # marks the end of a sentence, and thus indicates that no word follows.

Since the structure of the Divine Office has varied so substantially, due to diachronic changes, variations between secular and monastic patterns, and variations in accordance with the liturgical year, it is very difficult to give a general description of it. Here I follow Harper 1991, esp. the chart he gives on pages 76-77 and the discussion that follows.

These actions are in fact mostly composed of songs, readings, and other sorts of speech. But in keeping with a common tradition in ritual studies, I consider these as "speech acts": in other words, utterances viewed as actions rather than as statements of propositions.


After allowing that "a well-formed theory of meaning must include reference" (2), he then asserts that "rituals could be meaningful in spite of the fact, if true, that they do not have a reference" (3).

This classification of words was, of course, based on how sentences got translated into predicate logic.

In the sense of the all-pervading medium through which light waves propagate.

Syntagmatic relations exist between words that can be combined serially to generate a sentence. Paradigmatic relations exist between words that can be substituted for one another in a given sentence.

The definition of structuralism that Devitt and Sterelny accept holds that all the meaning-relations of a symbol system point to other elements of the system, rather than to things outside the system, for example to objects in the world.
I pick this example because, while people frequently ask the question "What is the meaning of life?", Adams is the only person I am aware of who has attempted to answer it, even in jest, in just this form. This seems to be good evidence that "meaning" does not have semantic force in this context. I take meaning to require a two-place predicate, Mxy, and cases where this does not occur should be viewed as immediately suspect. (This observation will become important in section 1.2.5.) "What is the meaning of life?" is usually synonymous with "Why are we on this earth?" or something similar, and thus semantic in surface form only.

See, for example, his use of Donald Davidson in Penner 1995.

Note that I use the word "distributional" differently than linguists do: see section 3.3.4.

Propositional attitudes are psychological states that relate a person to a proposition; examples include believing that and desiring that.

Dr. Robert Kraft suggested the name "megameaning" to me.

Actually, this is somewhat of a simplification, though it is sufficiently accurate for the present purpose. The Vedic ritual (śrauta) texts, for the most part, gave only the score for one of the priests participating in the ritual. Thus, to get the full score of the ritual, it would be necessary to collate several ritual texts. This work of collation was done in the medieval period by scholars and collected in texts called, often, paddhatis. It has also been done in the modern period in the work Śrautakṣa (1958-1970).

Note that I use the term "distributional" in a different sense than linguists do; see section 3.3.4.

See Chapter 5 on the role of naturalism.
Chapter 2

I believe that Chapter 1 provided a relatively complete discussion of Staal's treatment of ritual meaning. The current chapter is intended, then, to serve two purposes: first, it provides a context for Staal's Meaninglessness Thesis by looking at similar arguments both for and against ritual meaninglessness; and second, it helps introduce topics that will be discussed in Chapters 3 and 5.

The current chapter will be divided into three parts. First, I will look at some "traditional" approaches to ritual meaning. Most ethnographies and histories of ritual assume that rituals are meaningful, so there is not so much a traditional (i.e., widely shared) theory of ritual meaning as a traditional presumption of meaning. But the presumption of meaning is not the refutation of a theory of meaninglessness. The second section of this chapter looks at three theories of ritual meaninglessness: those of Staal, Dan Sperber, and Pascal Boyer. The idea here is first to show that Staal is not alone in his criticism of the traditional model, and second to consider the differences between these three models. While Staal, Sperber, and Boyer agree in their general orientation to ritual, they also differ in important ways, and understanding these differences should provide another means of understanding the significance of Staal's theory.

Staal's version of the Meaninglessness Thesis is no doubt the most well-known and most often cited in the religious studies literature, but even his theory has not yet received even one serious and careful critique – as I have demonstrated at some length in my Introduction. In section 2.2, I consider other theories of ritual that can be viewed as Meaninglessness Theories, and in section 2.3 I look at one theory of ritual meaning in some detail as a way of illustrating the points I have made earlier in the chapter.

Since I will be considering a number of theories in this chapter, I should point out at the outset that I will not present detailed summaries of each, but rather will focus on particular points that are important for my overall project. Thus readers who are unfamiliar with the works considered may not
be in a position to evaluate my assessment of each position, but they should at least be able to follow the comparisons I make and the conclusions I draw from this review.

It may perhaps seem odd to treat the semantics of religious rituals (Chapter 2) before their syntax (Chapter 3): after all, I think that the most obvious candidate for meaning in a theory of ritual syntax is compositional meaning. But most theories of ritual privilege meaning over form, and non-syntactic approaches to ritual form are less likely to emphasize the compositional properties of meaning or the computational properties of their instantiation. Therefore, beginning with meaning seems more in keeping with the way most people in religious studies think about ritual, and it has the added advantage that it provides a context for my detailed discussion of ritual syntax in Chapter 3.

2.1. Traditional Approaches to Ritual. There is no one, explicit theory of how to study ritual among either the religious studies community or the anthropological community. Yet the manner in which I wish to discuss ritual meaning in this chapter, namely by focusing on a small number of recent contributions that seem especially helpful in clarifying aspects of Staal’s theory, requires that I present some sort of foil against which the scholars I will discuss are reacting. This is not purely a rhetorical device, creating a foil ex nihilo that will be vanquished in the conclusion. While there is no dominant theory in ritual studies, there is a certain unanimity in practice among scholars (especially ethnographers, historians, and theorists) that is finally and belatedly coming under criticism. We might wish to consider this practice pre-theoretical: it has driven research like theory does, though without being rigidly codified; and it has, perhaps, inhibited the development of proper theory by satisfying some, though not all, of the practical needs of those who study ritual. I will begin with a brief review of the status of ritual theory, then present a model of traditional approaches to ritual that I borrow from Boyer 1994. Finally, I will present some examples to clarify the chief points of this characterization of how ritual is studied.

2.1.1. Method in Ritual Studies. In this dissertation, I speak of ritual studies just as I do religious studies or anthropology, as a scholarly field of study. This is justified inasmuch as there is now a reasonably large group of people who recognize it as a field in its own right, though a field without many of the traditional features of a discipline, like departments or faculty lines. The fact of
the matter is that ritual has always been studied by people in a variety of disciplines, and as a result the methods used in studies of ritual have been as varied as the disciplinary identities of scholars who study ritual. Over the last quarter century, there has been a move to create ritual studies as a field in its own right, or else as an interdisciplinary area through which people from different fields can share common concerns. I think that the best way to identify the dominant trends in the study of ritual is to look at the concerns of those who have tried to turn ritual studies into a discipline, and this is the path I will follow in the current section. I will avoid discussing disciplinary history for its own sake.

Let me begin by considering the fields that currently participate in ritual studies. Anthropology and religious studies are the prime contributors, but the nascent, interdisciplinary field of performance studies should also be included in the list: Richard Schechner, who collaborated with Victor Turner in the final years of his career, and Ronald Grimes, the "father" of ritual studies as a discipline, are largely responsible for making performance studies a prime contributor. In the new preface to the second edition of his *Beginnings in Ritual Studies*, Grimes gives the following list of scholars and fields interested in ritual studies: "psychiatrists and literary critics, anthropologists and theologians, religion scholars and kinesiologists . . . architecture, sports, journalism, history, business, and advertising" (Grimes 1995, xiii). Frederick Denny, the editor of the series in which this volume appears, gives his own list: "symbolic anthropology, liturgiology, theology, religious studies, performance theory, and biogenetic structuralism" (Grimes 1995, x). It should not be surprising that a field with such diverse contributors is having trouble developing an overarching theoretical program.

Let me present two other factors that work against consensus in this field. First, Grimes has always striven to be what he calls a "pragmatist" (Grimes 1995, xx) regarding theory in ritual studies. Though his own research has been in a fairly restricted area at the border of theological liturgics and performance theory, he has helped (notably through the early bibliography Grimes 1985 to his editorial work at the *Journal of Ritual Studies*) to keep the field as a whole open to as wide a range of perspectives as possible. This is pragmatic inasmuch as it has helped the field to persist long enough to develop a following and some degree of respect. If its boundaries had been demarcated in advance of detailed research, it is likely that it would not have reached its current state of maturity. The irony of
this approach is that the field is now fighting to develop a center and a dominant methodological orientation: "Theoretical and taxonomic coherence in the study of ritual has been slower in coming than I had hoped when I wrote [the first edition of] Beginnings [in Ritual Studies] . . . I imagined that stimulating new theories were just around the corner. Not so" (Grimes 1995, xii).

Second, religious studies has always been the dominant model for ritual studies as a discipline, and this means that the factors working against methodological consensus in religious studies have been imported wholesale into ritual studies. Chief among these is the conflict between interpretive and explanatory methods. Another conflict exists between academics and practitioners (only roughly equivalent to the religious studies/theology divide). To give an example of this conflict, consider the new Ph.D. program focused on "religious diversity in North America" recently announced by Wilfrid Laurier University, Grimes' homebase, in collaboration with the University of Waterloo. "Designed to attract administrators, artists and media professionals, counselors, journalists, religious officials, social workers, and teachers", the program focuses on having students learn "public intelligibility". Ritual studies, unsurprisingly, looms large in this program, though it is not focused solely on ritual. Such a program is clearly built on the model of a divinity program, where pastoral care and liturgies are taught alongside history of religions and comparative religion. This example is not intended to reflect any value judgment on the merits of such a program; it seems clear that Wilfrid Laurier and Waterloo have done a good job of market research, and that there is indeed demand for such a program. What I wish to point out is that it would be rather surprising if any program could be developed that could serve the needs of theoreticians and practitioners equally.

These are, in brief, the chief factors working against unanimity in method. Methodological differences are particularly vital to debates over ritual meaning, and for this reason I have prefaced my treatment of meaning with these more general reflections.

2.1.2. The Assumption of Ritual Meaning. In this section I wish to consider what Boyer 1994, 193 and 219-222, calls the "cultural" or "theological" approach to religious ritual – these terms are almost interchangeable in his usage. While there are many models for how to approach ritual (section 2.1.1 was intended to emphasize that point), there is also a surprising unanimity in underlying
assumptions, at least to a point. Boyer 1994, 193, attempts to capture the key elements of this approximate consensus, and thus I begin with his words:

In most anthropological studies of religious ritual, it is assumed that the actions performed in such situations can be characterized by their cultural "meaning", by the range of significations the participants associate with their performance. This is of course more often than not expressed in remarkably vague terms, which give little indication as to what cognitive processes are supposed to underpin such "meanings".

Boyer mentions several key points in this passage, all of which will be important for my subsequent discussion. First and most obviously, while the term "meaning" is frequently used in reference to ritual, it is rarely defined. I would suggest that the term is taken to be unproblematic by many authors, so they see no need for a clear definition. Second, the cognitive force of the term is not made explicit; this will become important in Chapter 5. Finally, cultural meanings and the private associations between acts and meanings are both frequently referred to in discussions of ritual. This is the most important, and from a theoretical perspective the most problematic, element of the traditional approach. It is, therefore, one that I will focus attention on in this chapter. I should add that though my argument as a whole is modeled on Boyer's, this subsection should not be taken as a mere summary of his theory: I am using his argument for my own ends, and thus my interpretation might be rather idiosyncratic.

Frequently it is asserted that ritual actors associate their actions with meanings that they get from their own cultural environment. At one level, this is perfectly sensible. The same action or gesture or metaphor might be used in rituals on opposite ends of the globe, in which case we would say that the meaning of the action depends on the culture. And since actors know their culture, they know how to interpret cultural products like rituals. Unfortunately, the situation is not as straightforward as we might hope.

It is a commonplace that ethnographers encounter difficulties when they try to determine the cultural meaning of a particular rite. Informants might disagree among themselves about the correct meaning, or the fieldworker might get, in answer to his questions, mutually inconsistent responses from a single informant. If a given ritual is clearly important to a people, because they practice it year after
year and spend significant time and resources to bring it off, and if that ritual seems invested with significant cultural meaning, we would expect that all the informants would give similar descriptions of the meaning of the ritual. Similarly, if we ask ten people what a given word means, we would expect them to give similar answers, or else to give answers that cluster around a small set of basic meanings. Yet it appears that informants are much better at agreeing on lexical meaning than on ritual meaning. Clearly there is a problem, but anthropologists differ in how they characterize the nature of the problem.

Two solutions are adverted to most often. First, the variation in answers given by different informants is explained by the hypothesis that rituals are symbolic, and symbols are multivalent: they can mean different things, but once someone associates a particular symbol with a particular meaning, then that thing means that meaning, at least for that person. This is the idea of relative meaning that I alluded to in Chapter 1. Sometimes Saussure is mentioned, and we are reminded that meaning is not a natural property, but rather a cultural one. In this context, "cultural meaning" does not imply that a cultural artifact has a given meaning relative to the culture, but only that any meaning it has is merely cultural, i.e., not natural. But this seems to conflict with the problem I mentioned earlier, namely that different symbols mean different things in different cultures. If symbols are truly multivalent, then it is not clear how we can explain different meanings in different places by reference to their local, cultural contexts. In short, cultural meanings must be determinate just like natural meanings are. This still leaves the possibility open that cultural meanings are not fully determinate: perhaps culture only dictates constraints on possible symbolic meanings, so that knowledgeable informants can differ on the meanings they assign to particular symbols, but there is a limited range within which they can differ. It may be possible to reconcile cultural determinism of meaning with the idea that meaning is relative, but little effort has been devoted to working out such theoretical details. More often, scholars argue against the idea that cultural meanings are absolute, and go no further than this.

When they do go further, they attempt to show that the putative meanings of a given symbol are all consistent with one another. This is the approach to cultural products that resulted in, and followed after, the so-called "rationality debate" that produced much theoretical literature around the third quarter of the twentieth century. The debate has since died down, but not so much because it was
resolved as because an impasse was reached, and people tired of the problem. I will characterize the rationality debate in terms of a problem that I mentioned already, namely the problem that the claims made by one informant sometimes appear to be internally contradictory. In such a situation, the ethnographer is faced with a choice between reporting the words of his informant uncritically, portraying the informant as irrational, or finding a way of reconciling apparently contradictory statements. The last of these choices was the one that has generally been followed, at least in the last century or so. (Up into the nineteenth century, it was common to speak of "savages" as "prerational" or something to that effect.)

The program of reconciling apparently irrational claims involves two components. First, one extrapolates from an informant's statements a list of beliefs that are attributed to the informant. These beliefs are assumed to be relatively stable over time, though they are prone to change periodically. If a person's beliefs changed constantly, then it might not be possible to reconcile contradictory statements – but since a person with constantly varying beliefs would have trouble even surviving from day to day, it is assumed that most beliefs are relatively stable, and changed only when there is warrant for belief change. This is all consistent with commonsense psychology, which assumes that each person subscribes to a given set of beliefs and, ceteris paribus, acts in accord with those beliefs. Second, a deductive argument is constructed so that some of these beliefs are shown to follow logically from others, which are taken to be premises. The definition of rationality is taken to be something along the lines of "the ability to validly draw conclusions from premises", so a deductive model is a natural result of the desire to see natives as rational agents. Some of the beliefs attributed in this way to a person are taken directly from explicit statements the person has made, whereas others are derived from these primary beliefs much along the lines of the second component: anyone who believes A and B must believe C. Thus, the two components are not sequential steps, but may be utilized in unison.

In the study of small-scale societies, the view that religious meanings are culturally-based is combined with the method of rational reconstruction of beliefs, such that a set of coherent beliefs is attributed not only to individuals, but also to the culture as a whole of which individuals partake.
Unfortunately, both elements are problematic. I have already considered the former, so let me now look at the latter.

It is perfectly reasonable to attribute beliefs to a person even if that person has never enunciated those beliefs explicitly. There is, after all, no reason to think that people have direct access to all their mental representations. But the attribution of a rational system of beliefs is more than just the assumption that some beliefs are subconscious. It assumes that conscious beliefs are the result of deductive chains of reasoning; and further that all (or at least the bulk of) the beliefs a person holds are mutually consistent, in the sense that they can be interrelated by deductive chains of reasoning. There is really no good reason for assuming either of these is true. Often there may be reason to believe that some claim is true, and one simply assumes that if it is true, all the necessary conditions obtain to allow it to be true. When people are forced to justify all their beliefs, they frequently find it difficult to do so, as anyone who has taught a class can readily attest. Throughout his book, Boyer 1994 argues that abductive inference is often more important than deductive inference. Abductive explanations "consist in putting forward conjectural assumptions that, if true, would account for the data observed" (Boyer 1994, 147), and they can create the illusion of a formally deductive system if used carefully: the same assumptions can be conjectured as being responsible for results that appear similar, and the same putative connections between cause and effect can be assumed to hold in each case.

If much of our reasoning is in fact abductive, then the attempt to reconstruct subconscious beliefs to explain formally enunciated doctrine, or ritual actions, or symbolic forms, may prove to be very misguided. And just as the hypothesis of a cultural system of belief may lead to problems, so will the theological model attributed particularly to literate and large-scale societies. Christianity and Judaism, under the influence of Hellenistic philosophy, developed early in their history certain models for formal reasoning to defend and articulate their core doctrines – and to support their religions against the competing faiths of the Roman and Late Antique Mediterranean. This led to another justification for attributing deductive models to subconscious belief systems: because it was now possible to articulate such belief systems, it was assumed that this was how people’s belief systems were actually formed. Of course, there are many people in large-scale societies who are not theological scholars, and
whose mental representations of the divine world do not correspond except in rough outline to the theological systems available to experts. Instead, they hold a certain set of beliefs, and assume that whatever background conditions would result in these beliefs actually obtain. But the difficulty that organized religions have had in stamping out "heresy" should be sufficient evidence that many people do not hold beliefs that can be tied together by strictly deductive links, even if one is free to posit any necessary (subconscious) background assumptions.

In short, both cultural and theological models may be used to justify the standard approach of attributing distinct ritual meanings to individual observers. The fact that a person engages in a ritual is presumed to be sufficient evidence that the person attributes a particular meaning to it, in which case the job of the fieldworker is to ascertain what that meaning is. To deny this is taken to be a devaluation of the subject's rationality. But if we reject the deductive model on which this approach is based, if we assume that it is indeed rational (and, as a practical matter, necessary) for everyone to sometimes act on the basis of abductive inferences, then it no longer is necessary to attribute distinct beliefs about meaning to ritual agents. It might be that agents perform non-pragmatic ritual actions not because they attribute some meaning to the act, which in principle could be articulated in propositional terms, but because the action is valuable to them. It is of course possible in many cases for agents to find meanings to attach to their actions when they are pressed to do so, for example by fieldworkers, but often these putative meanings should be viewed with extreme caution.

2.2. Arguments for Ritual Meaninglessness. I have already discussed Staal's version of the Meaninglessness Thesis at some length, so let me just highlight some of his central contentions here. Staal allows for rituals to be meaningful in a wide variety of ways: rituals commemorate historical events, they serve social functions, they are judged valuable by the people who practice them, they are vessels onto which people project their own intentionality, etc. But there is one important type of meaning that ritual does not have: meaning derived compositionally through the syntactic arrangement of smaller units (rites). Staal focuses on this type of meaning because it is the one true sense of ritual meaning if the language analogy is to be taken seriously. His approach, as I have reconstructed it, may be described as follows: he assumes, as a result of his syntactic study of ritual form and his
presuppositions regarding meaning, that the only true sense of ritual meaning is compositional meaning; he then shows that rituals do not get whatever meaning is attributed to them compositionally; so he concludes that rituals are meaningless, in any important sense. He does allow that rituals get meaning other ways, but none of these is real, true meaning.

We could describe compositional meaning as "internal" because the meaning of a given ritual is located within the ritual, thanks to the elements that compose it and their order of performance. The intentionality of the agent performing the ritual is not relevant, and likewise ritual does not depend on some semiotic code external to the ritual. It might be that whatever meanings get associated with a given ritual are recognized elsewhere in the same cultural environment (for example, when people are observing a ritual performance, they might recognize the representation of some doctrines that they believe to be true independently of any ritual context), but since these are external meanings being imposed on the ritual from outside, they do not really count as meanings of the ritual in any proper sense. A meaning can be attributed to a ritual without the ritual itself becoming meaningful in the process.

Actually, this tentative distinction between internal and external sources of meaning needs clarification, on two counts. First, "internal" may prove ambiguous. We can think of the ritual system as an abstract device that generates meaning, and in this case meaning derives from a source internal to the system itself. Alternatively, we can present a psychological interpretation of Staal's theory, in which case the compositional relations that give well-formed rituals meaning are internal to the mind itself, rather than to some abstract system. This ambiguity, once it is identified, is probably not dangerous, since we know how to treat it once we develop an ontology for rituals. See Chapter 5 below.

Second, potential sources of external meaning can be external to the ritual system and/or the ritual device in the mind-brain. Perhaps people impose through a free act of the will a given meaning on a given ritual at a given time. This would be analogous to the reader response theory of textual meaning, and in such a case ritual meaning would be derivative from the intentionality of the ritual agent rather than resulting naturally from formal properties of the ritual system (or its physical
instantiation). In such a case, any consideration of ritual syntax would be irrelevant to questions of meaning. Viewed this way, the mind can be the source of internal or external meaning: internal if the formal representation of the ritual in the mind is sufficient to account for the meaning that gets attached to ritual actions, external if a free act of the will (deriving from a different part of the mind, if we think of the formal representation of the ritual as restricted to a distinct device within the mind-brain) is required.

Cultural sources of meaning would also constitute external sources of meaning, for example if a given culture possessed a semiotic code or ritual language not fully implemented by the mind-brain of any person within the society. Such a semiotic code might be imminent within the culture, or it might belong to a "collective consciousness". In any case, this would constitute a source of ritual meaning external to the ritual system and external to the mind of any ritual actor.

This terminological distinction allows us to generalize from Staal's particular position and categorize theories of ritual as belonging to two broad classes: those that assume that true ritual meaning must be internal, and those that assume it must be external. Similarly, we could apply the same terms to the actual means in which putative meanings are attributed to rituals. Then we would say, for example, that according to Staal's theory the only meanings that do get attributed to rituals come from external sources, but despite this, rituals are per se meaningless because the only true source of ritual meaning is internal. This provides us with a quick way of classifying theories, which might prove useful; but it has the disadvantage that many theories will have to be reformulated to put them in the requisite form. Further classifications may then be made, for example distinguishing different types of internal or external meaning along the lines I have indicated above. This is, in fact, the procedure I will follow in the rest of this section: I will summarize the theory, consider what it counts as real ritual meaning, classify it as internal or external, and finally look at how it treats meaninglessness.

2.2.1. Dan Sperber. We can compare Staal's theory to one published just a few years earlier by Dan Sperber. Sperber is not concerned with ritual in particular, but rather with all aspects of cultural symbolism. Ritual is one subject among many that have traditionally been treated symbolically, and for which his theory ought to work (Sperber 1975, 111). Sperber presents a cognitive theory, in fact the
first cognitive theory of ritual: in Chapter 5, I will consider this aspect of the theory at greater length, but in order to explain his treatment of meaning I will need to summarize here only a few key points of his argument. After devoting the bulk of his first three chapters to a detailed critique of previous theories, he uses the last two to argue that symbolism is best understood in terms of how the mind-brain handles difficult-to-process information.

Sperber develops a rough model of the mind, in which new information coming from the sense organs is compared to information stored in memory, and processed on the spot whenever possible. In some cases, however, the mind is not sure what to do with new information, so it (in Sperber's terminology) "puts this information in quotes": in other words, it marks it so that in future it will know that it could not properly deal with it. The mind continues to make sense of information that was previously put in quotes, but it is unable to follow determinate rules in doing so. Instead, it relies on "paths of evocation" to, as it were, sample possible interpretations and try to determine which is the best. This is how Sperber explains the fact that there are almost as many interpretations of symbolic material as there are people interpreting it: unlike mundane information, which most people process in essentially the same way, symbolic material is processed in ways that depend heavily on the previous developmental history of the individual, and it is therefore interpreted both idiosyncratically (everyone processes it somewhat differently) and culturally (the more common history two individuals share, the more their interpretations will coincide, ceteris paribus).

Certainly details of Sperber's cognitive model could be improved upon – Toren 1983 presents some cogent critiques – but on the whole Sperber presents a novel argument against the traditional view of cultural (and ritual) meaning, the sort of view illustrated in section 2.1. The key is that symbolism is explained by means of the same cognitive system that is used to process non-symbolic information, and differences in interpretation are explained in terms of differences between individuals. There is not any essential difference between symbolic and non-symbolic materials: instead, the difference is dependent on the mind of the person reflecting on those materials. Symbolism is not, for Sperber, a language in its own right, operating according to its own rules in the same way that mathematics does, independent of
people thinking it. Rather, information is symbolic relative to agents, and it is symbolic as a result of agents' evolutionary and developmental histories.

Just as I presented Staal's theory as first arguing that rituals would be meaningful if they were derived compositionally, and then showing that such compositional relations do not obtain, so we could argue that Sperber's theory presents the traditional model according to which symbolic meanings depend on an external semiotic code or language, only to then show that the mind does not rely on such a code. And just as Staal does allow that "meanings" (not real meanings, but merely putative meanings) can be attached to rituals in other ways, so does Sperber accept that the mind attaches putative meanings to symbols through the ongoing process of evocation.

According to the typology I presented above, we could say that meanings-as-code would be external meanings, but these do not in fact occur. In a different sense, putative meanings-as-evocation that are attached loosely and provisionally to symbols are also external: even though meanings get attached to rituals (and to other cultural products) through processes internal to the mind-brain, there is no indication that the formal properties of the mental representations of rituals are responsible for meaning attributions. The internal/external divide is not marked by the surface of the skull, but rather by the surface of the ritual system, which might itself be no more than a set of mental representations.

I class Sperber's theory among the meaninglessness theories both because it rejects the traditional, meanings-as-code model, and also because the only meanings that he allows for rituals are provisional, putative meanings-as-evocation.

2.2.2. Pascal Boyer. Though Penner 1985 recognized that Sperber had dealt a strong blow to any theory of ritual meaning, and urged his peers to study Sperber's argument, few people in ritual studies have followed his lead. More familiar perhaps than Sperber 1975 is Boyer 1994, a book that follows Sperber both in his critique of cultural meaning and also in his later development of a theory of the "epidemiology of representations". The idea of the latter, in a nutshell, is that anthropology and the other fields that study culture should be interested in representations (beliefs, for example) insofar as they distribute themselves across a population. Scholars have long been puzzled by the relationship between individual and collective representations. For someone like Durkheim, collective
representations are qualitatively different from individual representations: though the latter might depend on the former in some respects they are different kinds of things, with collective representations only presenting themselves to a "collective mind". For Sperber, however, there are only individual minds, so to say that a representation is collective just means that it is widely distributed within a population. It is the goal of an epidemiology of representations to study how ideas become distributed across multiple minds.

Boyer 1994, then, takes these two theories and attempts to build out of them a relatively comprehensive model for how religious ideas can be explained. Though he builds on Sperber, I would argue that his theory differs from Sperber's in significant ways. In particular, he presents different arguments for ritual meaninglessness. I will consider his theory at greater length in Chapter 5, and consider only the arguments for ritual meaninglessness now. Though his treatment of religious ideas aims for comprehensive coverage, his treatment of ritual in particular is more hesitant and preliminary: he recognizes that ritual cognition is quite widely distributed among different parts of the mind, and consequently no single theoretical model will be able to account for all the properties of rituals. His theory focuses on the interaction between ritual action and the other "repertoires", or topics, that he considers at length in his book. His initial treatment of ritual, however, extends beyond this limited focus to look at topics that, though his theory will pay little attention to them, might end up playing some role in a general theory of ritual.

Boyer begins, then, by considering the meaning of a ritual to be the information contained in it. Viewed this way, rituals seem a particularly inefficient way of communicating information. Consider that the properties most often associated with ritual are repetition and stereotypy: identical actions or rites tend to be repeated within and between individual performances, and they tend to be repeated exactly from one performance of a given ritual to the next. It is, admittedly, well known that repetition can serve the end of effective information transfer, for example when there is noise in transmission that needs to be suppleded; but it seems clear that ritual repetition far exceeds the bounds necessitated by information theory. If one's goal is to communicate meaning, Boyer concludes, then ritual seems a particularly poor medium. While he does not entirely reject the possibility that rituals
express meaning (in the sense of containing information), he argues that there are strict limits on the extent to which they can do so. This is consistent with, for example, Staal's position that rituals can be used to reinforce social values, but that such functions are not sufficient to explain the occurrence of ritual. This is typical of Boyer's approach: he does not reject outright the role of information transfer in the operation of ritual, but he does argue that this is not a primary function of ritual.

Boyer then points to two facts that should limit the role of meaning in ritual theory. First, he makes what he calls a "methodological" argument to the effect that rituals are not propositional in form, and hence are unlikely candidates for the expression of meaning. They are, after all, rituals and not explicit statements. This is an approach that, as we will see shortly, comes under direct criticism by Bell. His second argument is that "even if [this] method were sound, it would not guarantee that the 'meanings' extracted by anthropological analysis have anything to do with the representations actually entertained by the participants"^5 (Boyer 1994, 193). This last comment should not be interpreted as a proposal that the conscious understandings of ritual participants are the best interpretations of ritual meaning: instead, it is meant as a rejection of the view that any "meanings" that anthropologists can find in a ritual are necessarily in that ritual. The "representations actually entertained by the participants" are the representations that guide their ritual actions, and may include (as I argue in Chapter 5) representations that map meanings onto actions. But such representations may well be subconscious, or may be such that it is hard or impossible for ritual actors to adequately express them in propositional terms. I should add that the claim rituals represent individual mental representations might be consistent with the view that rituals transmit information: the informational theory is a theory of the content of ritual meaning, whereas the claim that rituals depend on individual representations is a theory of the source of ritual meaning.

With this as a preliminary, let us now look at the main argument of Boyer 1994. Boyer seeks to develop a theory that accounts for how ideas of religious objects are represented in the mind-brain; in each of the four "repertoires" of religious representation that Boyer considers, the basic argument is the same. I will consider this theory in more detail in Chapter 5, but for the present let this suffice. The mind-brain evolved in reaction to environmental factors over many millions of years. Boyer does not
concern himself with reconstructing the pressures that led to this development, but he does look at empirical evidence for the resulting manner in which the mind-brain operates in the present. The content of mental representations differs across time and space, between individuals and between cultures. But the similarity in past and current environments guarantees periodic similarities in cultural content. More to the point, however, the mind-brains of all humans, whatever the details of the contents of their representations, are governed by the same basic constraints, and these constraints allow us to understand the outer bounds of our basic conceptual intuitions. The possibilities for highly abstract thinking are hard to get a handle on, so Boyer's theory does not help with understanding the boundaries of technical theological reasoning, just as it cannot help us understand the boundaries of mathematical genius, but it can help us understand the basic, intuitive concepts of the divine that all people have regardless of their formal theological training.

Boyer develops his detailed theory of religious representation in reaction to the views of Lawson and McCauley 1990. He argues that any mental representation of rituals must have two components: a representation of the sequence of actions that collectively constitute the ritual; and a representation of the connections between these actions and some greater "conceptual scheme". Though the term "conceptual scheme" may seem straightforward to most readers, Boyer generally avoids this term and prefers instead to speak of the "assumptions" embedded in the four "repertoires" that his book focuses on. But the idea is roughly the same regardless of the language used: part of the mental representation of a ritual is a mapping of the sequence of ritual actions onto a set of semantic notions. These semantic notions are cultural – in other words, they are shared broadly by a range of individuals in the local environment of the individual – and they are therefore derived from a source external to the ritual and its representation.

It becomes clear in Boyer's subsequent discussion that the content of religious representations (in particular, the representations of religious objects) is determined by general constraints specific to particular types of subject matter, along with the contingencies of history and of the transmission of representations. In any case, the content of ritual representations derives from the same conceptual base as other religious thoughts: like Sperber, Boyer places his theory of ritual meaning squarely within a
larger theory of representation, but unlike Sperber he treats ritual representation as a specific domain within this larger arena. For Sperber, of course, the larger theory is a theory of symbolism, whereas Boyer puts it specifically within a context of religious symbolism. Staal, in contrast, treats ritual as sui generis, and consequently any cognitive theory extrapolating from Staal's theory is liable to treat ritual representation as a distinct domain or mental device, rather than as a part of a larger semantic environment.

I hope this is clear. The idea is this: Whatever meanings get assigned to rituals are meanings that make sense within the general worldview of the ritual participants. Thus supernatural agents who are held to participate in the ritual are figures who are liable to be the subjects of mythological or religious stories. The objects used in rituals, which are imbued with particular powers, are elsewhere recognized as special: perhaps their collection or creation or use is surrounded by taboos, or perhaps their color or shape is viewed as significant through a process of analogical explanation. In other words, the meanings found in rituals are viewed by Boyer and Sperber as elements of a larger system of meaning that extends throughout the culture. But for Boyer, there are particular and fairly strong constraints that govern certain of these meanings, and these constraints are a viable object of cognitive study.

Boyer's theory could be described as a meaningfulness theory inasmuch as he rejects the traditional approach to ritual meaning (see section 2.1 above), and with it the view that there is a semiotic code somewhere external to the individual mind-brain, which governs individual representations; the latter is taken directly from Sperber's critique. But he offers a significantly stronger role for ritual meaning than either Staal or Sperber do. Part of the representation of a ritual includes a directed relation to a meaning, so meanings are part of the mental representation of the ritual. If the meaning changes, so does that part of the representation. Thus, Boyer provides the critique of ritual meaning theories with which I began this chapter, yet in some sense he does allow for internal ritual meaning (in contrast to Staal and Sperber). As with Sperber's theory, ritual meaning is idiosyncratic and therefore a function of individual representations of the ritual, and not a function either of the ritual action itself, or any larger ritual system.
2.3. Ritual Meaning in Practice. Meaning and structure are topics that need to be considered in the study of any ritual culture, and in principle it should be easy, therefore, to cite examples as a means of clarifying the various methodological problems considered in this dissertation. I follow Staal's lead in my discussions of ritual structure in Chapters 1 and 3 when I look in detail at elements of Vedic śrauta rituals; I also considered one extended example from the Christian tradition in Chapter 1. Unfortunately, the nature of the problems that I am considering do not lend themselves to quickly cited examples, for two reasons. First, Staal's method of syntactic analysis requires us to look at entire ritual systems, and fairly complex ones at that, so it would do no good to look at one short rite abstracted from its context in a larger system. My emphasis on what I call "distributional analysis" simply accentuates this problem. Second, in the case of ritual meaning it would be easy to cite examples (such as the Brahmanas give freely, i.e., statements of the form A MEANS X), but these examples would be rather beside the point, and would not illuminate the important theoretical problems. They would just not be helpful in understanding what we should make of such meaning claims.

Having given these caveats, I will attempt to consider another ritual culture in some detail at the end of the current chapter. To the extent that this does not satisfy the need some readers might have for tangible examples of ritual practice, at least I should be able to illustrate why the sort of inquiry I am engaging in here needs to be theory-heavy and example-light. I should emphasize that detailed treatments of particular ritual systems are still desperately needed: strictly formal and philosophical studies can help to guide historical and ethnographic research to some extent, but such research cannot answer many of the empirical questions about how people represent and perform rituals.

Next, I will consider an annual temple ritual (the odalan) that is well-known in the West thanks to the fieldwork and philological research of a number of scholars, dating back roughly a century. This is perhaps a poor choice as a contrast case to the Vedic rituals examined by Staal, since the odalan belongs to a Hindu (i.e., Indian) religious tradition in Indonesia, and it therefore bears some kindred resemblance to the Vedic ritual culture. But it happens that few of the ethnographies I have consulted provide the sort of data necessary for my purposes, and in this case I am forced to rely on the fact that ritual ethnographies for Indonesia and South Asia tend to be best suited to my purposes. I do not know
whether to attribute this fact to properties of the ritual systems found in South Asia or to traditions of scholarship in those areas. I should add that Staal has recently written a monograph on this religious tradition (Staal 1995), but in contrast to other parts of this dissertation I will pay little attention to Staal's interpretations in this particular case.

It is certainly the case that our best evidence for small-scale societies in different parts of the world comes from different time periods (and consequently they manifest different trends in scholarship): our knowledge of Native American religions in the United States comes from a brief period in "Western expansion" during which the American government systematically encouraged anthropological fieldwork, whereas our comparable evidence from Canada is significantly older; the classical period of African ethnography coincided with British and French colonialism in that continent; good anthropological studies of Pacific islands religions were produced after Westernization had affected the "pristine" traditions in North America and Africa; etc.⁷ In short, any similarities we see among certain religious cultures in South and Southeast Asia, might or might not reflect properties unique to some "Hindu" worldview.

It is also possible that the existence of native ritual manuals has facilitated certain types of ethnographic research in South and Southeast Asia, and that in the future similar studies of ritual manuals in Jewish or Chinese ritual cultures will yield similar insights. But since I have been forced to rely on secondary sources for my examples, I have taken them where I can find them.

2.3.1. Ritual and Meaning in Balinese Temple Ritual. I will not attempt a thorough study of scholarship on the Bali odalan, but rather will refer mostly to one early but important monograph, Belo 1953. Belo provides a good approximation of what I call a "distributional analysis": the core of this work (Chapter III) is a sequential description of the rites that compose the odalan. The three days of preparations for the ritual and the four days of additional rites following the ritual are described in briefer compass (Chapters II and IV), but this is because the monograph focuses on the odalan itself. In Belo's description of the ritual is in many ways much like a Vedic śrauta sūtra in that it provides a comprehensive description of the ritual actions, with somewhat briefer treatment of the words said during the ritual, and on the whole it does not discuss possible interpretations of the ritual actions.
The ritual is modeled on the treatment of guests, and most actions make sense in this context: after initial preparations such as purifications and offerings, the gods are called down or "awakened"; they are then given food and revered; at the end of the ritual the gods are then sent back home, and closing rites are performed roughly corresponding one-to-one to the opening rites. In addition, some rites are repeated in the course of the ritual, for example the sequence of "making holy water" and "symbolic sweeping". The first time this sequence occurs, it follows "ground offering in five directions", and the second time it is preceded by "invocation of the gods from the five directions". The symmetry that Payne pointed to (and which will be discussed in more detail in Chapter 3) is clearly evident, as are the types of asymmetrical repetition we saw in the Christian Offices (see subsection 1.2.3). In short, from a purely formal perspective this ritual looks quite familiar, though of course the concerns about ritual syntax that I will present in Chapter 3 apply here as well. Let us then turn to the problem of meaning.

As I say, Belo does not concern herself too much with the meaning of particular rites. But she does give them all descriptive names; I used quotation marks in the previous paragraph to identify some of these. In the case of most of the rites it seem fairly obvious what the action is intended to be. In the rite of "procession to the bath", the gods (i.e., their images) are taken to the bath. Frequently the words used in the ritual tend to function as glosses on the actions. In "seating the gods", the gods are asked, in ordinary speech, to sit down. Elsewhere mantras composed in Old Javanese are used, or else in "Archeipelago Sanskrit". In the rare cases where the actions seem unusual, symbolic interpretation seems simple. Thus, in the rite called by Belo "contests", various objects laid on a couch are then held opposite each other as though in combat, or else (in the case of a pair of eggs and a pair of coconuts) rolled against each other. Belo explains that "part of the ritual often precedes the cockfights" (54, n. 30) and that "the priest afterward explained, the gods delight to watch any sort of combat just as they enjoy a cockfight" (54). Hooykaas 1977, in the translation of a brief interpretive text, tells us that this symbolic combat may itself be referred to as a "cockfight". Thus, in cases in which the action is represented rather than presented, there seems to be relative agreement on the proper interpretation of the symbolism.

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In short, it is usually not difficult to identify the basic actions that compose the ritual, because either they are straightforwardly pragmatic actions in their own right, or else they are glossed within the ritual itself. The ritual is a reception of the gods, who are viewed as honored guests to be treated with respect. Even if we set aside the rare cases of symbolic action, then, we are presented with the ritualization of readily understandable, pragmatic action. We would be inclined to interpret these actions "literally", to say that their meaning is just what the actions do. Thus Belo's naming of the actions is itself a form of interpretation of the "meaning" of the rites.

Yet the fact that we view these actions as ritual seems significant. Perhaps it would be best to set aside the word "ritual", to say that these are pragmatic actions and therefore no different than anything else that Balinese do on a regular or sporadic basis. It would appear that we are importing our distinction between sacred and profane into a context in which this distinction does not provide us with a key to the nature of the actions in question.

Yet the rituals are clearly important to the participants. We might call them "marked" actions, just as poetry or other ritualized speech is considered "marked" by linguists. Any household in the parish in which the ritual is performed that does not provide an offering for the "going to market offering" is fined ten kepengs (56). This would seem a clear sign that participation in this rite is important for more than its pragmatic function: it is not the sheer number of offerings present that matters, but also who brought them. The fact that these actions are marked would be some indication that they are more than just what they appear to be. We could say, as many scholars would no doubt be inclined to do, that the key difference between greeting any guest and greeting a divine guest is that the guest in the latter case is a god, and this imposes a certain sanctity on all the relevant actions. But in this case "sanctity" merely is used to mark the fact that the recipient of the action is divine: the word is merely a name and has no true explanatory force.

We might hypothesize that there is an implication, as clear to participants as it is to outside observers, that there is something special about these actions, and words like "sacred" or "ritual" are ways of marking this special quality. But it is perhaps not clear to anyone what this special quality entails. This would explain the lack of agreement on even basic problems among scholars of ritual.
Because the actions are marked or special, participants and observers alike are inclined to attribute additional meaning, beyond merely pragmatic meaning, to the individual actions. When there is no clear pragmatic "meaning" to an action, as for example when a water jar faces off against a bottle of arak or eggs are rolled at each other (54), then the meaning attributed to the action is called "symbolic".

What the various meaninglessness theories discussed in this chapter have in common is a shared intuition that this sort of talk of meanings is not really helpful, and perhaps distracts us from the work that needs to be done in understanding such actions. Boyer is probably clearest in arguing roughly along the same lines as I have done. He argues (or at least implies) that scholars do the same thing as ritual participants when they try to attach meanings to rituals: this counts as doing constructive theology rather than helping us understand the rituals themselves. It is explaining one unknown with another, and we should not be surprised that this ends up resulting in an infinite regression that takes us nowhere. Staal, in contrast, tells us so adamantly to focus our attention on the formal structure of the actions that he almost entirely sidesteps the fact that most people have strong intuitions that rituals are meaningful. Whereas Staal's critics believe that he has simply misunderstood basic meaning claims, his position (and Sperber's and Boyer's) is that scholars who attribute meanings to rituals are themselves engaging in an activity that is itself a part of ritual culture, and consequently they are not allowing themselves to perform the task they should perform, namely to try to understand the ritual.

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1 This is one of the three facets around which the program is constructed; the other two are "North American focus" and "multidisciplinary theories and methods". All quotations in this paragraph come from the provisional prospectus for the 2003-2004 year that is posted, as I write, on the program's website.

2 Boyer 1994, 125-134 also discusses the rationality debate in this context.

3 Actually, there is a slight complication here. Staal seems to think that proof of his Meaninglessness Thesis may be found in the fact that no meanings need "go through the minds" of ritual agents during the performance of a ritual, see Staal 1989a, 115. We should not take this to imply, as some people might think it does, that rituals would be meaningful if officiants always had meanings in the forefront of their minds. Rather, we should take it to show that ritual meaning is always independent of intentional meaning, since in some cases it is. In other words, a particular case counts as a counterexample because it refutes the universal claim that ritual meaning is always intentional meaning. As always, Staal does not consider all the possible theoretical alternatives, because he has already discounted some of them. It seems clear that Staal believes there can only be one, primary sense of "ritual", while all the other senses are in some way derivative from it. Thus, the alternative that ritual is sometimes meaningful internally, yet other times externally, does not appear as an option worth refuting. Sometimes rituals do not go through the minds of practitioners, so ritual meaning cannot be just the projection of human intentionality.
In this he is anticipated by the similar argument of Wheelock 1982, whom he does not cite.

It is worth commenting at this point on one of the few places where Boyer 1994, 195, n. 3, considers Staal's theory. He argues that Staal errs when he "oscillates between analytical considerations and the participants' viewpoint", whereas Humphrey and Laidlaw 1994 present a version of Staal's theory that is more refined because they take the participants' viewpoint into consideration. This comparison is, I think, a poor representation of Humphrey and Laidlaw 1994, but it does point to the key difference between Boyer and Staal. Boyer's epidemiological perspective elides the difference between cultural and individual representations in a way that Staal cannot accept; for him, in contrast, a distinction between intentional thought and subconscious processing is necessary.

He uses the term briefly when introducing this topic, then replaces it with his preferred terminology; he takes the term "conceptual scheme" from McCauley and Lawson 1990.

I could have used shock quotes much more freely in this paragraph. While I recognize that there are many political and methodological problems with the ethnocentric assumptions underlying the history of ethnomethodology that I give in this paragraph, these assumptions are a fact of the secondary literature in religious studies from earlier periods.

Note that I do not use the term "distributional" in quite the same way as linguists do: see section 3.3.4.

"Understand" as I use it here does not have the meaning given to it in phenomenological analysis, but on the other hand it is not necessarily synonymous with "explain" either. I do not wish to precisely limit the meaning of this term, at least for the present.
Chapter 3

3.1. The Language Analogy. From the very beginning, I have assumed that when Staal says that ritual has a syntax, he means to argue for a strong analogy between ritual structure and natural language structure, at the level at which words combine into sentences. In other words, I see him asserting that language syntax is the best model for ritual structure, and no model simpler than language structure is sufficient to account for ritual structure. Though there are reasons to think that Staal sometimes might have a weaker criterion in mind, I think his theory is most interesting when he is arguing for this relatively strong claim. I will describe this as the "strong interpretation" of the Syntax Thesis. The general comparison between language and ritual I will call the "language analogy".

The strong interpretation would not be satisfied if we found significant differences between language structure and ritual structure --- though it is not immediately obvious what would constitute a significant difference in such a case. The strong interpretation would also not be satisfied if we found a model of noticeably greater simplicity than natural language that did as good a job as natural language in modeling ritual structure. In the first case, the language analogy is not sufficient to explain ritual structure, and in the second case it is not necessary. The body of this chapter will consider whether the language analogy is a necessary and sufficient part of an adequate theory of ritual.

I will consider three alternative models that are all structured by rule systems simpler than those responsible for natural language (section 3.2): namely, those of diagrams, discourse analysis, and the structure and evolution of religious doctrine. I will also look at potential problems with the language analogy (3.3 - 3.5). I will conclude by looking at how the problem of complexity affects our assessment of the Syntax Thesis (3.6).

3.1.1. The Weak Interpretation. I believe that Staal's theory is best viewed as favoring the strong interpretation of the language analogy. But there are signs that sometimes he has in mind much weaker criteria. It is therefore worthwhile to briefly consider whether Staal intended to argue for a weak interpretation of the analogy, and if so, what the implications of this would be for the Syntax Thesis.
Staal came to the study of language and ritual after studying mathematics and formal logic. He was attracted to the study of natural language due in large part to the rigorous formalism of the generation of Harris and Chomsky, but his interests in language seem always to have been conditioned in part by his knowledge of formal languages. This becomes clear some places where he discusses the meaning of "syntax". For example, in considering the "general symbolic expression" ABDBA, Staal says "if we analyze the same expression in syntactic terms . . . [we] study only the configurations of the letter symbols" (Staal 1986, 37). In contexts like this one, he is interested in distinguishing syntax from semantics, and aspects of syntax that do not emphasize that contrast are deemphasized or not mentioned. To this extent, Staal is perfectly right that the details of natural language syntax are not relevant.

In Chapter 1, I phrased the Syntax Thesis as follows: religious rituals have the same structure as natural language down to the level of their primary constituents, namely rites or words. But if "syntax" for Staal means not "the formal structures found in natural language" but rather just "formal relations without consideration of meaning", then the thesis is much less interesting. This seems to be how Payne reads Staal: it is possible to provide a purely syntactic analysis of ritual, if one is so inclined. But everything has structure, so everything is open to syntactic analysis in this weak sense. Such an analysis could not provide any great insight into the nature or purpose or function of ritual --- since anything is open to syntactic analysis, everything is equally syntactic. "Ritual syntax" would then be a completely shallow motto for a theory of ritual. Staal seems to think that his theory tells us something about what ritual is. It therefore seems that in different places Staal is torn between the strong and weak interpretations. I think he is more interested in the strong interpretation, and in passages like the one I just quoted (Staal 1986, 37), if he does have the weak interpretation in mind, this must simply be a slip of the pen. But I think that in that passage he is just explaining what formal structure is in simple terms, and not making a programmatic statement at all.

3.2. Symmetric Structures. Recall that Richard Payne argued that Staal's theory presented a useful formalism for describing the process of "repetitive symmetry" characteristic of Shingon rituals. Staal identifies symmetry as an important feature of ritual structure, and Payne inclines us to think it is
the key ritual structure. In this section I will consider the symmetrical patterns found in South Asian diagrammatic structures, discourse, and doctrinal structures. The rules governing such patterns are clearly simpler than the syntactic rules of natural language. If these simple models prove to provide the best analogy to the patterns found in Vedic ritual, then we will have good reason to reject the strong interpretation of the language analogy.

Though discourse structures will be the second model that I consider, they provide perhaps the best introduction to a discussion of symmetric structures generally. Michael Witzel and Christopher Minkowski have presented arguments to the effect that the formal properties found in Vedic ritual can also be discovered at the narrative level in Sanskrit texts. They both assume that these structures were originally developed in ritual and then applied to texts, but I wish to consider the possibility that the causal chain could have run the other way, from language to ritual.

Witzel presents a very interesting (though quite casual) critique of Staal's theory that in my opinion is more persuasive than the critiques considered in my Introduction. Let me quote his own words: "Staal, by the way, is wrong in describing the ritual using the well known inverted 'trees' of 19th century Indo-European linguistics or transformational grammar. Frames within frames would present a clearer image of the procedure of Vedic priests . . ." (1987, 413, n. 70). Witzel rejects Staal's theory out of hand, without even the recognition that reasons are necessary, and in its place he proposes a quite different model. This model is, in fact, just the same as the one that Payne finds in Staal's theory, namely the idea that all of the formal similarities between Vedic rituals that Staal considers can be analyzed in terms of a frame, or a combination of opening and closing sequences, sandwiched around a central element.

Thus Witzel accepts one part of Staal's formal analysis, but he rejects the language analogy. More specifically, he accepts what he calls "framing" and what Staal would call1 "embedding without prefixing or suffixing". As this last phrase should make clear, Staal allows for left- and right-affixing and medial insertion under the term "embedding"; none of these count as framing for Witzel. Though I will discuss this at more length later in this chapter, let me briefly illustrate each of these structures,
making use of the single arrow introduced in Chapter 1 (for convenience, affixes are indicated by A, medial insertions by M, and frames by F):

<table>
<thead>
<tr>
<th>Type</th>
<th>Rule</th>
</tr>
</thead>
<tbody>
<tr>
<td>Left-Affixing</td>
<td>$Q \rightarrow A \cdot Q$ (1)</td>
</tr>
<tr>
<td>Right-Affixing</td>
<td>$Q \rightarrow Q \cdot A$ (2)</td>
</tr>
<tr>
<td>Medial Insertion</td>
<td>$R \cdot Q \rightarrow R \cdot M \cdot Q$ (3)</td>
</tr>
<tr>
<td>Framing</td>
<td>$Q \rightarrow F \cdot Q \cdot F$ (4)</td>
</tr>
</tbody>
</table>

To repeat, embedding for Staal includes all of (1) to (4), but framing for Witzel includes only (4). More generally, I will use the term "framing" (and, alternately, "symmetric structures") for any structure that is composed of repeated applications of the rule $Q \rightarrow F \cdot Q \cdot F$.

Witzel proposes that rule (4) is sufficient to account for the structures found in Vedic ritual, and sets his proposal as an alternative to Staal's language analogy. Even if we supplemented rule (4) to allow for slight variations from perfect symmetry, for example by introducing a deletion rule that would allow us to eliminate elements from a sequence of ritual actions, this would still be an incredibly simple rule system, whereas the language analogy implies by comparison an incredibly complex one. In this section I do not want to develop a full-fledged framing model for ritual. What I will do is illustrate some similarities between ritual form and the forms of some systems that are much simpler than languages. The value of beginning with these comparisons will become evident in the middle sections of the chapter, when I look at problems with the language analogy. At the present time, I do not know the best way of describing the formal properties of ritual structure, even for one ritual tradition like the Vedic. There appear to be problems with Staal's language analogy, but it is far from clear that these problems are fatal. Yet even simple systems like those considered in this section (3.2) demonstrate surprising similarities to more complex ones like the system of Vedic ritual. Perhaps the best model for ritual is one unique to ritual. Perhaps the language analogy can be made to work in the end. But the best antidote to a starry-eyed belief in the language analogy is a consideration of the possibility that other models are out there, such as those considered in this section. I begin with diagrammatic structures.
3.2.1. Diagrammatic Structures. I use the term "diagrammatic structures" to describe a variety of formal models that we can see visually manifest in Indian art and architecture, and in some of the physical performances that help constitute ritual action. I begin with these because the structure inherent in them can literally be seen.

In India, as in many other cultures, the universe is seen as being roughly circular in shape and composed of regions forming concentric circles around a core that constitutes not only the physical, but in some sense the spiritual core. Of course, alternative models also are found in India, sometimes as alternatives and other times in conjunction with the circular model. The most common alternative is a vertical model, in which hierarchical relations are indicated by differences in height rather than in the radial distances of the circular models. Though circular views of the cosmos are found in a wide range of South Asian materials, the clearest examples I know of are maps of the universe found in Jain manuscripts. Many of these are collected in Caillat and Kumar 1981.

Such radial, diagrammatic structures are clearly similar in form to the frames model of Witzel and Payne: a ritual of form A R A contains a central core (R) surrounded in every direction with an element of different form (A). Ritual is essentially one-dimensional whereas cosmological models describe a second or even a third dimension, but the basic principle is the same regardless.

For a model that combines all of the elements considered thus far (ritual action, cosmology, and visual form), consider the maṇḍala-shaped containers (maṇḍal in Tibetan) used in Tibetan Buddhist worship. Dagyab describes just such an object:

The most generally used type [of ritual container] consists of a round plate with high sides, on top of which are placed three or four hollow concentric rings of decreasing diameter, held in position by the grain-offering that is placed within. On the top is placed an ornate decoration which usually has the wheel of the dharma in the centre. (Dagyab 1977, 34-5 [vol. 1])

Dagyab 1977 also provides a photograph that illustrates just such a container (vol. 2, plate 19). The assemblage that results from this ritual action is clearly a microscopic representation of the universe at large. The wheel of the dharma at the top is a sign of this, and texts referenced by Dagyab also make
the symbolism clear. Further comparisons are perhaps also helpful, for example the similarity of this assemblage to Hindu and Buddhist temple architecture.

Both the maṇḍal and the temple are three-dimensional, tiered structures that decrease at each higher level. Both are also capped with a tear-dropped shaped object that points upward. As it happens, both also are representations of the cosmos at large, though this similarity is only implicit in the apparent form of these objects.

The maṇḍal is the best image for our purposes, however, because it constructs a three-dimensional framed structure through ritual action, just as the Vedic rituals that Staal considers construct a one-dimensional framed structure through ritual action. And whereas Witzel and Payne describe the process as framing around a central core, the construction of the maṇḍal proceeds through a process of embedding: a smaller, central core is inserted into the center of an existing structure. And because of the nature of the materials, this insertion occurs in the third dimension in the case of the maṇḍal.

In short: the maṇḍal provides the "missing link" between Payne's analysis of Shingon ritual form and the cosmological models I have just described. According to this diagrammatic version of Witzel's critique of Staal, Vedic ritual structure can be described entirely in terms of one particular type of embedding, namely one that results in a mirror-image structure centered around a core element. Whichever way one proceeds from the center in the finished structure, one encounters the same elements in the same sequence. Such symmetrical structures are very much the exception in natural language constructions\(^1\), but (as the previous examples should be sufficient to show) they are quite common in certain types of religious representation. Such demonstrations put the onus on Staal to argue that such a simple model is in fact insufficient, either by showing that such symmetric models are only a small subset of the structures actually observed in Vedic ritual, or by identifying independent arguments in support of his theory. As this chapter proceeds, I will consider both of these approaches in defending Staal's theory. If we repeatedly find in ritual very un-language-like patterns, this will be a strike against the language analogy.
One caveat is perhaps in order. The examples I have given are all taken from traditions with a strong basis in the South Asian subcontinent, but then so is Staal's ritual theory. In any case, the cross-cultural extrapolation of both Staal's syntactic model and the diagrammatic model would follow similar paths, and both would be either supported or disproved as cross-culturally valid theories by an expansion of the data set beyond the confines of South Asia.

3.2.2. Frames, Rings, and Discourse. Both Michael Witzel and Christopher Minkowski have separately developed the theory that the device of "literary framing", known to classical philologists as "ring composition", in Sanskrit literature can be traced back to a similar formal device in Vedic ritual. In doing so, they abide by the chronology of our extant Sanskrit sources, inasmuch as the literary texts that demonstrate framing were all composed after the classic Vedic ritual evolved. But their theories would be more interesting if we could argue just the opposite, namely that Vedic ritual is structured by principles that also structure discourse patterns — i.e., linguistic patterns at a level higher than the sentence level. However, the first job will be to demonstrate the similarity between ritual and discourse structures. Talk of causal relations and historical priority is of secondary importance. I begin with a summary of the arguments of both Witzel and Minkowski.

3.2.3. Witzel. Witzel focuses on middle and late Vedic prose texts, whereas Minkowski is interested in the later Sanskrit epics, especially the Mahābhārata. Witzel's formulations are perhaps more useful for my purposes, so I will consider Witzel's argument first.

I have quoted the key passage from Witzel 1987, 413, n. 70, in section 3.1 above: "Staal, by the way, is wrong in describing the ritual using the well known inverted 'trees' of 19th century Indo-European linguistics or transformational grammar. Frames within frames would present a clearer image of the procedure of Vedic priests..." Let me briefly unpack this. I pointed out in 3.1 that a structure of frames within frames is a symmetric structure: whatever comes first in the series also comes last, whatever comes second also comes next to last, and so on. The image of frames conjures up the image of a painting of a painting. The frame of the represented painting is contained within the outer painting, which is itself contained within its frame. Thus going left to right across the surface one encounters the frame of the outer painting, the painted frame of the inner painting twice, and then the frame of the
outer painting. Witzel's claim is that framing is a significantly simpler structure than syntax, and that framing is sufficient to account not only for the narrative structures that he is focused on, but also for the ritual structures that Staal has studied. Thus, Witzel's analysis of Vedic narrative texts does not provide any support for his critique of Staal's theory of ritual. Rather, he assumes his critique of Staal's analysis stands on its own merits, that it follows from the evidence that Staal himself marshals. It would be more interesting, I will propose, if a study of Vedic narrative structure helped us to understand Vedic ritual structure.

Witzel (1987) is interested in the development, from middle to late Vedic prose, by which narratives get longer and more complex. By examining the legend of Cyavana in a variety of Vedic sources, he demonstrates how the complex version told in Jaiminiya Brāhmaṇa 3.120-128 derives from earlier versions of the story, from the Rg Veda onwards, and how the complex version of the Jaiminiya Brāhmaṇa is structured. He also indicates briefly the future history of the frame pattern in later Sanskrit literature, notably in the epic, which is the subject of Minkowski's research.

The story of Cyavana actually weaves together several distinct plot lines, as follows:

(1) The old man Cyavana gets a young wife.
(2) Cyavana performs a ritual and is paid with 1,000 cows; he also is made young again.
(3) The Aśvins learn the secret of the severed head of the sacrifice from Dadhyaṇe.
(4) The Aśvins get to join the sacrifice of the gods by performing the gods' sacrifice successfully, as their Adhvaryus. They also get to drink Soma, whereas before they had only been allowed to drink hot milk or perhaps honey.

In the version in the Jaiminiya Brāhmaṇa, (1) and (2) form a continuous story and the outer frame of the narrative, and (3) and (4) are embedded within this structure. The order of events within the story follows the sequence (1) (3) (4) (2), and the sequence (3) (4) is incorporated into the outer frame by making Cyavana the one who tells the Aśvins to go talk to Dadhyaṇe. Otherwise, the outer frame
seems to form a distinct story from the inner stories, which are themselves largely distinct. Actually two elements are operative here: ordering the stories in a sequence such that outer elements fit more closely with each other than with the internal elements, and modifying the individual story elements so that they tell a continuous story. Strictly speaking only the former is framing, whereas the latter, which requires the modification of story elements so that they can be combined by framing, is the discourse analogue to morphological alteration.

Whether these plot lines were originally distinct, and only gradually brought together by an evolving Vedic tradition, is not clear. All the main elements of the story occur in the poems of the Rg Veda, but without obvious connections between the major plot elements (some elements only appear in the Yajur Veda). Witzel presents an argument according to which it is possible that the authors of the Rg Veda knew the story as a whole (386-387); the fact that it is not presented as one narrative in the poems is a result of the allusive style of Rg Vedic verse: the poems do not tell complete narratives, but only mention them in the course of praising the relevant gods. If the authors of the Rg Veda knew the narrative sequence that we have in the Jaiminiya Brāhmaṇa even though they do not present it as one continuous story, we ought to perhaps presume that they had other narrative genres that the later Indian tradition has not preserved; this would have provided the literary-cultural background for understanding the Vedic hymns that are extant. Witzel addresses this to some extent: "Note, that it is a moot point whether the fragments of Rgvedic myths were re-composed as YV [Yajur Veda]-Sanhitā/Brāhmaṇa time stories or whether there was a living mythological tradition, in which Rgvedic myths gradually changed until they reached the form they have in the later Br. [Brāhmaṇa] literature." (386, n. 20).

However, he is not interested in the alternative that seems most interesting to me: namely, whether the early Vedic seers possessed narrative genres capable of generating complex syntagmatic chains of story elements such as we see in the extant literature of the later Vedic period. He seems to take such development to be a later innovation.

Here I am assuming that, in order to argue that narrative framing explains ritual framing, we would have to begin by claiming that both occur generally across cultures, and then make some further
extension to this observation. To make the more limited claim for this in the Indian context, we would want to argue that narrative framing preceded ritual framing, and was causally responsible for it, in the Vedic period. Witzel does neither, though either path would constitute a natural extension of what he does accomplish.

In any case, let me now ask a simpler question. What was the motivation for bringing these particular stories together? The answer, according to Witzel, was ritual.

The two elements of the inner frame, namely the justification of including the Aśvins in the rite of the gods and the restoration of the severed head by the Aśvins, at first served as aetiological explanations for two rituals that, as they first evolved, were independent, the Soma rite and the Pravargya. Later the Pravargya became a part of the Soma rite, and the combination of the two aetiological stories served to explain this ritual embedding. Ultimately, then, ritual embedding was the cause of narrative embedding and preceded it in time. This provides a causal chain that is the opposite of what would be necessary to explain Vedic ritual structure. One reason why the more complex version of the Cyavana story did not develop earlier was that the Pravargya did not become part of the Soma rite until fairly late, and thus the narrative change was unmotivated until that time. A second reason (though Witzel is less explicit about this) might be that narrative conventions change somewhat slowly, and the progression from allusive Vedic verse to complex, framed structures itself took time.

Though we do not find a detailed critique of Staal’s theory in Witzel’s "Meaningful Ritual" (1992), as the title might incline us to hope, this paper is relevant to the topic at hand. Here Witzel suggests a reason why frame structures should be considered one "of the main features and structures of Indian thought", namely that an older version (of a story, ritual, etc.), though it has been superseded, nevertheless is often "left unchanged by the new forms" that succeed it in the Indian tradition (781-2).

Let us call this process "conservatism". This is, Witzel would incline us to think, a general form of thought in Indian culture --- in Witzel 1986, 172, n. 34, he speaks of this as "eine der typischen Anordnung und Denkformen Indiens", along with ring composition, concatenation, etc.

Let me be clear. In Witzel 1986, he lists conservatism/inclusivism and ring composition/framing as two distinct "arrangements and forms of thought", but in fact there is a close
connection between them. One of the purposes of framing, in his view, is to provide a mechanism for cultural conservatism to occur. This is a significant claim: it implies that the diachronic history of a ritual can be read off of its synchronic form.

3.2.4. Minkowski. Like Witzel, Minkowski 1989 argues that Vedic ritual provided the model for the recursive embedding characteristic of Indian epic. He focuses his attention on the Mahābhārata, but finds similar structures in the Rāmāyana, Harivamśa, and elsewhere. But though he tries to adopt Staal’s interpretation of Vedic ritual, he sees it through the eyes of his reading of epic. Minkowski combines two types of argumentation, one focused on formal similarities between narrative and ritual structure, and the other (which he has pursued more in subsequent research) asking why rituals are selected as the occasions, within the story of the epic, for the recitation of the epic itself. I am interested only the former of these.

The core of Minkowski 1989 is a careful analysis of the formulaic diction of the Mahābhārata, focusing on junctures between the various episodes that compose the story-line. The Mahābhārata is structured, on the large scale, by a story-within-a-story frame, and consequently the junctures are bridged by verbs of speaking or other references to the tellers of the tales. Minkowski says "by creating two levels in the frame, the embedding tendency becomes unmistakable". In fact, "the embedding of narratives has two characteristic features: hierarchy and embedding" (406), and these features govern the large-scale structure of the narrative. Subnarratives are joined within the epic in two ways: either the same narrator tells one after another, paratactically (i.e., without conjunctions), or a new narrator and new audience are introduced. The latter case would correspond to hypotaxis in traditional grammar, with the introduction of the narrator corresponding to a grammatical subordinating conjunction: as a "when" or a "which" marks the junction of two clauses, so the introduction of a new narrator marks the junction of two tales. These are not mutually exclusive alternatives, and Minkowski considers mixed paratactic-hypotactic models as well.

In discussing the conjunctive links between stories, Minkowski discusses how Sanskrit epic differs from Homeric, and argues that the densest core of this formulaic language occurs in the sections
long recognized as the most archaic. The sūkha meter allows considerable metrical freedom as compared to the Homeric hexameter, and formulaic language tends to move to the beginning or end of the pada (or line of verse\(^3\)). Thus, one does not have the tendency to see the line of verse itself as being constructed almost entirely out of formulas, as one does after reading Milman Parry or Albert Lord on Homeric diction. Rather, Sanskrit epic is constructed out of formulas at a larger level: they structure the episodes of the story, and thus contribute at the level of discourse rather than syntax. And this is precisely what happens in the ritual as well, Minkowski argues.

He then typologizes these formulaic passages, distinguishing cases that mark: the arrival of the narrator, hospitality scenes, questions asked of the storyteller, requests for more information, and the intention of the teller. Each case is illustrated with copious examples from the text, showing that a small number of verbal stems is used for each type of narrative juncture (this is what makes the language formulaic).

The view of formal features as marking off borders applies to the structure of both ritual action and epic narrative, as becomes clear towards the end of the paper, where Minkowski provides an interesting discussion of Staal's theory (1989, 418-420). Minkowski asks how Staal's use of the word "embedding" corresponds to his own use of the same term in describing the structure of epic narrative. He considers every narrative told inside another narrative to be embedded, but clearly this approach is not sufficient in the case of ritual: "every large thing includes identifiable parts", and every element of a rite is a rite itself, down to the atomic actions such as walking to the north or pouring butter into a fire. Not every element of a story (of at least a sentence in length) is itself a story, however, so the parallel between story and ritual is not exact. Ritual theory will have to impose more constraints on what counts as embedding, as compared to narrative, to make up for the fact that ritual action itself imposes fewer constraints. Thus, Minkowski proposes a tripartite treatment of ritual embedding.

Though he focuses primarily on epic, Minkowski actually contributes in the process to ritual theory. Unfortunately, he is far too brief at key points in his argument, and it is not immediately clear what his contribution consists in. He begins his discussion of ritual structure with the words: "embedding manifests itself in the yajñas [rituals] in three distinct ways" (418). He then goes on to
discuss three topics, which I will now consider one at a time. I use the word "topic" here as a vague term; it is not clear to me what Minkowski understands the significance or value of the these three facts about ritual to be. I have treated them as constraints on a theory of ritual, and this is probably how we should understand them. They clearly do not serve the same function as, for example, embedding and transformations do in Staal's theory. The latter are mechanisms that structure ritual sequences. Minkowski's topics are something like observations about such structures, or clarifications that are necessary for his ritual analysis but not for his literary analysis.

Minkowski's three topics: First, rituals retain a certain identity even when they participate in embedding. A large rite "has actions that are distinctive to itself alone", and likewise smaller rites "preserve their own integrity distinct from the actions of the main rite that they form a part of" (418). This topic is a necessary precondition for the third topic (see below), and I will discuss it further in that context. Second, embedding engenders structural symmetry, not always but at least in some cases. The example he gives comes from the Vedic īṣṭi (the simple, or vegetarian sacrifice), and thus it will require a little explanation. I will give a more detailed analysis of the īṣṭi later in this chapter. Minkowski proposes that the basic structure of an īṣṭi may be described as follows (418):
sāmidhenis

āghāras

prayājas

ājyabhāgas

pradhāna 1

upāṃśuyāja

pradhāna 2

sviṣṭakṛt

anuyājas

sāktavāka

śamuvāka

The way this is laid out on the page illustrates the symmetrical embedding that Minkowski finds in the ritual. Pradhāna 1 corresponds to pradhāna 2, as they are the same type of rite, and they precede and follow the upāṃśuyāja. Similarly for each rite at each level of indentation on the page: it corresponds to the rite at the same level of indentation, and between them they form a frame for the rites that are performed between them (the sequence for performance of the rites in the iṣṭi is top to bottom).

Unfortunately, Minkowski says very little about this example. In particular, he does not say why parallel rites should be seen as examples of the same type. In the case of the two pradhānas, the identity of the corresponding rites is clear, as they are instances of the same rite. Similarly for the prayājas (literally, fore-offerings) and the anuyājas (after-offerings), which clearly form a matched pair, as their names indicate. But it is not so clear why the sviṣṭakṛt corresponds to the earlier ājyabhāga, and not the prayāja. Perhaps Minkowski is thinking about the internal structure of these rites, or the gods to
whom offering is made in each. For example, the central rites (from ājyabhāga through sviṣṭakṛt)
include a purnuvākyā (invitatory prayer) as well as a yājyā, whereas the outer rites include only the
latter but not the former. This gives us reason to think that the sviṣṭakṛt is more likely to correspond to
the ājyabhāga than to the prayāja, but it does not give us reason to think the sviṣṭakṛt is more closely
matched to the ājyabhāga than to the pradhāna or to the upāmsūya. Of course, assuming that there is
one rite in the iṣṭi that corresponds most closely to the sviṣṭakṛt is ultimately question-begging, since it
presumes the symmetrical structure that we have set out to prove.

Similarly, in the ājyabhāga offerings are made to the gods Agni and Soma, and in the sviṣṭakṛt
offerings are made to Agni Sviṣṭakṛt. Thus, the recipients of these rites are the same, if we take Agni
Sviṣṭakṛt to be a form of Agni and ignore Soma as a recipient of the ājyabhāga. In the prayāja, in
contrast, offerings are made to samidhs, tanānapāt, iḍa, barhis, and svāhākāra --- which would seem to
be personifications of elements in the rite itself. Samidhs, iḍa, and barhis are all things that are offered
into the fire during the course of the rite; the svāhākāra is the utterance of "svāhā", which ends many
mantras used in Vedic ritual; and tanānapāt literally means "son of himself", i.e., the ritual fire. But
Agni is the offering fire as well being a god; and Soma is the chief offering in the soma rituals, as well
as being a god. Furthermore, according to at least one early source (Nirukta 8.22) the five recipients of
offerings in the prayāja are in fact just the god Agni anyway, identified under five different names.
Whether or not we take the Nirukta’s interpretation seriously in this particular case (and we might well
choose not to), this example illustrates the general direction of my complaint. The god to which
offering is made is probably not a good criterion by which to individuate rites for the the purpose of
identifying underlying ritual structure.

Minkowski gives little indication of how he has determined this symmetrical structure. He
refers to Frederick Smith's 1984 dissertation (published as Smith 1987), and in note 89 (418) he points
out that this is a structural analysis only of those rites that include offerings accompanied by recitation.
It is necessarily, then, only a partial structural description at best. This is the reason for my caveat when
I began to discuss this topic: "embedding engenders structural symmetry, not always but at least in some cases."

In short, an examination of the structure of işti rituals leads me to question whether this ritual exhibits as much symmetry as Minkowski indicates. It seems to me that Minkowski, like Payne, finds symmetry to some extent because he is looking for it. Some symmetry clearly exists, but perhaps not nearly as much as these scholars would have us believe.

More to the point, I am not convinced that symmetry is an important property for understanding ritual embedding. It seems more likely that there are processes that result in certain amounts of symmetry, but without selecting precisely for this symmetry. A rule of the form:

$$S \rightarrow aSa$$

(5)

will certainly generate a symmetrical output, and applied recursively it can generate the sort of structure Minkowski finds in the Darṣapūrṇamāsa. But any complex sequence can be described as instantiating a wide range of structures, and apparent structural symmetry is not necessarily the result of such recursive rules with symmetrical output. Again, I will return to all of these issues later and in more detail. But for the present I wish to point out that Minkowski's analysis is not as clear-cut as he makes it out to be. And for a comparison to Staal's theory of ritual, attention must be paid not only to the surface structure of a rite, but also to its generative history.

Third, unitary rites will be interrupted by "digressive" action sequences. "The yajñas include what can only be considered ritual 'episodes', where the activity of one embedding rite is actually interrupted in order to carry out another, 'digressive' rite" (419). Here Minkowski is using the original sense of the word "episode", as used in the Greek ἐπεισόδιον and current in English as recently as the original Oxford English Dictionary; not the looser, current sense of "an incident or event forming one part of a sequence"; to give the first definition found in the Oxford American Dictionary of 1980.

According to Aristotle (Poetics 1459a36), a composition like the Iliad or the Odyssey is a unitary work,
but the poet "diversifies the work by means of other [elements, namely] episodes,"¹⁰ such as the Catalogue of Ships, that break up the action of the main story. This is just the sense in which Minkowski uses the term. Any digressive rite within another rite --- any rite that begins before the previous one ended --- is an episode.

I emphasize the subtleties of Minkowski's terminology here because it clarifies a point that is less obvious in Staal's formulation of the notion of embedding --- though it is also one of Staal's assumptions, as I will show. When Staal presents the following analysis of the paśubandha ritual (Staal 1979a, 17):

\[
P \rightarrow p_1 \ D \ p_2 \ D \ p_3 \ p_4
\]

he is interested in the fact that the paśubandha contains two performances of darṣapūrtamāsa as proper subparts. He also wishes to point out that the darṣapūrtamāsa is performed slightly differently in these two cases, with different numbers of sāmidhenī verses being recited. Minkowski, on the other hand, emphasizes the fact that the episodes of the paśubandha, barring the two darṣapūrtamāsas, form a natural sequence or progression, that in some way they make sense as a sequence even without the intrusive darṣapūrtamāsas. This depends on his first observation about embedding, namely that rites have action sequences that are distinctive to themselves.

In the case of a narrative, we have a strong intuitive sense of what counts as a natural progression of actions, and what counts as a break in the sequence. It is this intuition that allows Aristotle to find just one story in the Iliad, perhaps two in the Odyssey¹¹, and more than eight in the Little Iliad (1459b1-7). And likewise, it allows Minkowski to separate out the various frames of the Mahābhārata. But it is not clear that the same intuitions are possible in the case of ritual action sequences. Staal's Meaninglessness Thesis would tend to militate against such intuitions. Perhaps we should provide a purely formal criterion for natural progressions of actions, and consequently for intrusive episodes. This would introduce a certain circularity, since we need the criterion in order to
determine the proper formal analysis of the action sequence to begin with, but despite the circularity this might be the best approach available. It seems to be the one that Minkowski favors, at least to a point.

Minkowski holds that "at the beginning [and end] of every yajña a series of actions is performed that . . . mark what kind of a rite it is" (1989, 419). Thus, the formal properties of the ritual sequence help to individuate particular rites within a ritual (i.e., mark their boundaries). But such an approach can never be used to individuate atomic ritual actions, since (as the quotation from Minkowski makes clear) this approach can only be applied to series of multiple actions. His example (the Agniṣṭoma) makes it clear that he has in mind only the largest rituals as being marked in this way.

If Minkowski is correct, we can say that some ritual structures (the larger rites, or major subcomponents of the chief rituals) have clearly marked beginnings and endings. This would allow us to clearly and unequivocally identify digressive embeddings when one chief ritual (e.g., iṣṭi, soma ritual, satra) is embedded in another, but it would not help with identifying digressive embedding at a smaller scale.

It is worthwhile in this context to also consider how Staal treats digressive embedding. He says: "an embedded ritual may be interrupted, once or several times, by the ritual in which it is embedded, to be continued or completed afterwards" (Staal 1989a, 108). Interestingly, while this is an instance of digressive embedding, it is in fact a special case of it. Consider Staal's example. An instance of the paśubandha is embedded in the agniṣṭoma. This means that we have a sequence of actions as follows:¹³

\[
a_1, a_2, P, a_3, a_4, . . .
\]

(7)

Staal is interested in cases in which elements of the agniṣṭoma intrude digressively into the paśubandha, which gives us the following picture:
With these various structures before our eyes, we can see that (6) counts as an instance of digressive embedding for Minkowski, but not for Staal (or at least he does not draw attention to the digression inherent in (6)). In contrast, (7) counts as an instance of digressive embedding for both Minkowski and Staal. But Staal does not identify this as a separate phenomenon; he calls it "another property" and seems to think of it as a corollary to his main argument about embedding.

In any case, both Staal and Minkowski are clear that embedded rites are not, in some important sense, part of the rituals in which they are embedded. After all, how do we know that \( a_5 \) in (8) is a part of the agniṣṭoma but not the paśubandha? How do we know to call it "\( a_5 \)" and not "\( p_5 \)? By Minkowski's analysis, it does not have the internal parts to let us call it a second-order embedded rite, so his criterion is not sufficient to answer this question definitively.

In this particular case, we might think that we have an even easier answer, since \( a_3 \) is a natural part of the agniṣṭoma, which by itself is structured as follows:

\[
a_1 \ a_2 \ a_3 \ a_4 \ldots \quad (9)
\]

But this answer begs the question, since we never find an agniṣṭoma without a paśubandha embedded in it. This does raise a larger question, however, namely how we can distinguish parts of a ritual itself from parts of a rite embedded in that ritual. Both Staal and Minkowski assume the very question makes sense, but since we never find an agniṣṭoma without paśubandha parts within it, it is not clear that we can distinguish our \( p_h \)s (i.e., parts of the paśubandha) from our \( a_h \)s (parts of the agniṣṭoma). Traditions may provide ways of distinguishing ritual classes, but the distinctions they make may not always be sufficient for ritual analysis. In the Vedic tradition, for instance, one immediately knows upon looking at a soma performance that it is not a mere Ḭṛṣṭi or paśubandha, because the latter rites can never require more than 6 priests, but any soma ritual requires a full complement of 16 officiants. But this is an
insufficient criterion by which to determine whether a particular act is part of an embedded işti, since all
16 priests will be present at a soma ritual even if some are unoccupied at a given moment.

The most obvious approach is to observe that the paśubandha occurs as a free-standing ritual
as well as a rite embedded in the agniṣṭoma. Thus, we can say that ritual elements or sequences that are
parts of an embedded rite (e.g., the paśubandha) are not parts of the ritual in which that rite is embedded
(this gives us a criterion for something being a paśa); the remaining ritual elements are parts of the larger
ritual (this gives us a criterion, in the case of the agniṣṭoma, for something being an aha).

Another observation by Minkowski: The three structural features that Minkowski 1989
examines are all interesting, though they do not necessarily point to mechanisms that drive the
patterned of ritual action. His examination of the language of junctures in epic is interesting not only
for the light it sheds on the individuation of rites and the rituals they are embedded within, but also
because similar imperative and precative language is used in both epic and ritual. Minkowski argues
that such language does not occur at junctures between rites, but rather within rites. This contrasts with
the case of epic, where, as he shows, a variety of requests, commands, and statements regarding the
actions of the narrator function to signal the change from one story to another. He does not, however,
point out that such statements frequently serve to determine the identity of rites, at least according to the
analysis implicit in the Vedic ritual texts. Some examples from epic and ritual should clarify this
point.

I have already listed the categories under which Minkowski classifies his discussion of the
diction of linking passages. Junctures between stories can be marked by a statement describing the
arrival of a new narrator or by hospitality scenes (since guests frequently tell or are entertained by
stories in ancient traditions, for example the Odyssey), a request can be made for the narrative to be
resumed, or the storyteller can announce his intention as a prelude to presenting his tale. Similarly, the
spoken components of Vedic rituals rely heavily on commands that actions be performed, followed
invariably by the performance of that action (the satisfaction of the command). For example, before the
Hoṛ priest says the yajya (itself an action that is performed by speaking), he is instructed to do this by
another priest. This instruction is called the praïṣa, which itself constitutes a discrete rite within the ritual. As one reads through the description of any Vedic śrauta ritual, sequences like this occur with regularity: one priest tells another to say something, or to do something, and that action is then performed. This is not an indication that the latter priest does not know what to do; rather, it is a prescribed part of the ritual, as necessary as any other action. In the case of the praïṣa, the action of instructing another priest is itself a rite, i.e., an element of a larger ritual. In fact, this rite itself is nothing but the issuing of an instruction, and the sequence praïṣa-yajya forms a pair, the one preceding the other by necessity of the internal logic of the ritual.

Thus, at a general level instructions help to structure Vedic ritual just as they help structure Sanskrit epic. For Minkowski, stories in the epic correspond to entire Vedic rituals, and while these are marked by formal devices, they tend not to be instructions. However, if we cast our net wider, perhaps instructions do mark some transitions in the epic. I proposed that we think of the pair of speech actions praïṣa-yajya as a ritual unit: here clearly the praïṣa indicates that the yajya will follow, and therefore marks the beginning of this small unit. If we think of the command issued by the Ādvaryu/Maitravaruna as not just an instruction to the Hotṛ, but also as part of a performance for an audience (the gods, perhaps?), then the praïṣa could signal that the audience should prepare for the yajya. Even if we reject the notion of an audience for the ritual, the praïṣa may still be viewed as marking a juncture in the ritual, even if it does not mark the juncture for anyone involved in the ritual.

3.2.5. Causal Links between Discourse and Ritual Structures. Both Witzel and Minkowski focus on discourse patterns found in extant Sanskrit literature, and since the rituals described in the sūtras precede the works of literature that these scholars examine, they assume that any causal connection must go from ritual structure to discourse structure. I have already indicated that there might be another way to read the evidence from the literary texts. Staal argues that ritual structures form the model for linguistic syntax, but from the cognitive view that I prefer his theory could easily be taken to argue that the same mental skills are used in both ritual and syntactic
processing. My approach would then reject the sort of causal story that Staal, Witzel, and Minkowski champion, in which the influence of one genre or modality on another can be traced in historical (or, for Staal, prehistorical) time. Though a careful discussion of my cognitive approach will have to wait until Chapter 5, a few words on the proper use of the written evidence from antiquity is perhaps warranted now.

Look at it this way. The story that Witzel and Minkowski tell is almost certainly correct as far as it goes: the early Vedic poems are composed in a peculiarly (though far from unique in the ancient world) allusive style that excels at particular functions and presumes a broad, shared background of cultural knowledge without which they are almost unintelligible. The late Vedic prose texts then developed a variety of new genres, including framing patterns that were developed on a massive scale only in the epic. But it is not at all clear what we should make of this story.

One hypothesis would propose that oral tradition exhibited framing at an early period. The early poems are very allusive --- they do not tell extended narratives, but rather mention individual episodes --- and if (mortal) consumers of these poems were to make sense of them, they must have learned the context of these episodes from stories composed in a very different and more narrative style. The extant poems were preserved in an archaic and fixed form because of their use in ritual, and thanks to metrical and generic conventions. It is of course not clear the nature of the compositions that did not leave any historical trace, whether they were metrical and what similarities they bore to later genres or to compositions in other Indo-European traditions. Speculation on these matters would therefore be fruitless. But it is not impossible that these exhibited some of the framing structures discussed by Witzel and Minkowski; if they did, then direct literary influence on extant literature is certainly possible. But let us set this possibility aside for a minute, and ask a more general question: does such literary influence provide the best framework for understanding the similarities between ritual and narrative structure?

Even if we could unearth evidence of an earlier, oral tradition that exhibited framing, literary influence would still only be a possibility --- just as the influence of the Yajurvedic Brâhmaṇas on the narrative structuring of the epic, assumed by both Witzel and Minkowski, is only hypothetical. The
logic of the argument made by these two authors goes as follows: complex narrative structures are likely to have an equally complex history; this history might be documented in extant texts, or it might not; if we can find earlier texts that would have been available to later authors, that exhibit simpler instances of these structures, then we may presume a likely causal chain connecting the earlier and later texts. This line of reasoning is fine, and indeed it does present one likely story to account for the facts as we have them. But it presumes that the structures in question are somehow counterintuitive (I used the word "complex" to indicate this) or themselves in need of explanation.

The alternative is that the structure in question (framing) is really quite mundane. In this case, no explanation is necessary for the development of framing, in ritual or in narrative. There might be a causal story to tell, and it might be historical in some way, but it would be a story of the development of framing as a general strategy, not framing in narrative or framing in ritual. We could argue that framing is a structure that our peculiar mental apparatus inclines us to (the cognitive approach) or we may trace it to the structure of everyday action sequences and the very nature of narrative itself, since narrative is just a rendition of everyday action sequences. We can also combine these approaches, and argue that the mind evolved for life in a world that is structured by certain patterns, and therefore it evolved to find and use those patterns in its direction of action out in the world. Thus, if one enters a building, or a sacred enclosure, or a grove, or a cave, one is also liable to leave it. If one cooks food, presumably one is liable to then eat it. Just as all things come into being, so they pass away. A lot of framing patterns seem unavoidable, and while postmodern approaches to narrative might seek to do away with the normal logic of human existence, rituals that are marked in relation to everyday life (and therefore necessarily must have beginnings and endings) are unlikely to rely too heavily on such postmodern techniques.

I began this chapter by saying that diagrammatic, discourse, and doctrinal structures provide an alternative to Staal's insistence on syntactic structures as the closest relatives to ritual structure. In that case, the question is whether the complexity of syntactic structures is necessary to understand the nature of ritual structures, or if simpler models will suffice. The question of causal history is a different one. It asks not what the best model is, but how the model is related to the thing modeled. Both questions
are relevant to Staal's theory, as the discussion yet to follow should make clear. Briefly, the ethology thesis is intended to answer the causal question, and the attention Staal devotes to this thesis is testament to his interest in this question. Similarly, Witzel and Minkowski are interested in this question. Staal's concern with the problem of complexity drives much of his syntactic analysis, as will be made clear in this chapter. Given Staal's emphasis on syntactic models, then, the question of complexity will receive the lion's share of attention. Chapter 5 will look briefly at the causal question.

3.2.6. Doctrinal Structures. Harunaga Isaacson has pointed out to me an interesting parallel to the argument for symmetric structure in the realm of what I will call "doctrinal structures". Not only does this doctrinal model provide one further model through which we can conceive of ritual structure, its similarity of form to diagrammatic structures may in itself be worthy of note: the more areas in which symmetric structures predominate, the more we should expect to find upon further inquiry, according to a naive process of inductive generalization. More to the point, we should not be surprised if the formal structures we find in texts are paralleled in the doctrine that they give expression to, or if the rituals that manifest these doctrines likewise exhibit a similar structure.

Within South Asian traditions, one common means of adopting new elements and adapting them to a current doctrinal scheme is simply to add them without further modification, thus creating a more complex whole that thus presents the history of that scheme within its formal structure. This could be thought of in the sense of Hacker's inclusivism (see note 6, above), but it need not be. In such a case, diachronic structure (history) may be manifest in synchronic structure (form) in a transparent fashion. Of course, it is not necessarily possible to accurately read diachronic structure from synchronic structure without some detailed knowledge of the historical evolution of the doctrine in question, but once the relevant history is known independently, diachrony can be seen to be explicitly present in synchrony. This is, from the perspective of language evolution, a very unusual situation, as diachronic changes in language generally have compensatory effects elsewhere, which alter the synchronic form of language in complex ways that cannot be treated under highly general rules. But as we will see, the synchrony-diachrony distinction might well play out differently in ritual structure than it does in language structure.
An example will probably clarify what I have in mind. Consider the evolution of Śaiva Tantra. Śaivasiddhānta may be considered the most normative of the Tantric traditions that focus their worship on the god Śiva, but a variety of other traditions also are attested, among them the Bhairava (of which Vāma and Dakṣina are schools) and Trika traditions. Tantra is largely an esoteric tradition among South Asian religions, and the practice of Tantra has traditionally begun with the performance of initiation and consequent instruction in secret doctrine at the hands of a guru; on the other hand, Tantric practices have spread themselves widely through Hindu and Buddhist traditions, for example in Hindu temple ritual, and consequently are familiar to many people in some form or another.

One of the chief characteristics of Tantra is internalized worship: the practitioner sanctifies himself (becomes, in one sense or another, the god who will receive worship) and visualizes the cosmos (itself represented symbolically by the images of a trident, lotus, and the "throne" of the god that they collectively constitute) inside his own body. Sanderson (1986, 187) presents a diagram that illustrates the version of this visualization presented in Abhinavagupta's Tantraloka: a trident pierces the practitioner's body, its base a distance of four fingers' width below the navel and the three prongs rising out of his head. At the base of the head of the trident is the eight-pedaled lotus flower of knowledge, which is associated with the form of the god known as Īśvara ("lord" in Sanskrit). At its center is Śadaśiva, another form of the god, who is represented as an emaciated corpse looking upward to the higher light above. At the top are three white lotuses. Each of these levels represents a higher level of existence (the word for these levels is tattva, "thatness" or "essence"), such as "consciousness" and the "transmental".

Now I wish to focus on the interpretation that Abhinavagupta gives for this visualization. With regard to the "superiority of all other schools", Abhinavagupta says:

In the (Śaiva-) Siddhānta the throne culminates in the (nine) Powers of Gnosis. In the Vāma and Dakṣina schools it is extended to incorporate Sadāśiva. In the Malayāmala it rises above Sadāśiva to end in the Consciousness (samanā). Here in the Trika it goes even further, ending
only in the Transmental (unmanā). That is why (the Mālinīvijayottaratantra) calls this throne "the supreme". (Sanderson 1986, 181)

While Śaivasiddhānta is esoteric from the perspective of Vedic/Brahmanical orthodoxy, it counts as exoteric in comparison to later and more "advanced" forms of Śaiva Tantra. Thus, Abhinavagupta says, followers of Siddhānta terminate their throne (the object of their visualization) with the Lotus of Gnosis. The god Śiva sits on this throne in the guise of Sadāśiva. The Bhairava schools of Vāma and Dakṣina, in contrast, transform Sadāśiva into the prostrate corpse in the description given above, and this corpse forms the throne for a still more esoteric form of the god, Svacchandabhairava. In the Kali worship of the Krama school, the same process is again repeated, and Svacchandabhairava forms a corpse-throne for a form of the goddess Kali.

Thus, as the Tantric worship of Śiva progresses, old schools become increasingly well known and their doctrines are viewed less and less as esoteric; new schools evolve and develop their own secret worship. The older symbols are not abandoned in favor of the new, however, but rather are retained, and their interpretation is accordingly modified. The cosmology of Bhairava and Trika Śaivism is an extension of the earlier Śaivasiddhānta model, and the structure of this cosmology, as represented in the internal worship of adherents, should now look familiar. It is just a frame structure that extends in only one direction: the base of the cosmos remains the same phenomenal world, but increasingly higher levels of reality are added at the top with the development of each new school's theology.

3.3. Basic Properties of Language as They Relate to the Ritual Analogy. Section 3.1 considered several types of evidence that could be cited by critics of Staal's theory of ritual. Criticisms based on such evidence would all attack Staal's theory from the same perspective: namely, they would all argue that linguistic structure is significantly more complex than ritual structure, and that simpler, non-linguistic models would provide the same explanatory power with less apparatus. Non-linguistic models would, in this case, provide better theories of ritual structure.
In the current section (3.3), I will look a little at some basic properties of natural languages and at the formal models that linguists have developed to capture these properties. This will prepare the way for section 3.4, which will consider Staal's syntactic analysis, picking up where I left off in Chapter 1. These discussions of linguistic and ritual structure (sections 3.3 and 3.4) will lead to an examination, in section 3.5, of the prospects for a ritual theory based substantively and not merely metaphorically on models of language structure. Thus, whereas section 3.2 considered arguments in favor of alternative models, the current section and the two following will consider the case in favor of Staal's linguistic model of ritual. Section 3.6 will look at some arguments lying in the background of Staal's theory; these do not seem to have been widely understood by previous critics, and are necessary for understanding the arguments Staal makes in support of his position.

3.3.1. Levels of Description and Duality of Patterning. Natural languages are constructed from a number of different kinds of parts, which form a series of levels of description of language. This seems to be odd and perhaps unnecessary at first glance, yet since all natural languages are constructed in this manner it is likely that this structural complexity is necessary for languages to function the way they do. The following levels are customarily distinguished from one another: discourse, syntax, morphology, phonology, and semantics. These are mostly presented in the order from highest to lowest level, in other words elements towards the end of the list combine to create elements closer to the beginning. The only exception is semantics: phonemes are not meaningful units (i.e., no semantic analysis of the phonological level is possible), and while meaning can be attributed to forms at the other levels, semantics is a qualitatively different thing than the other elements of this list. At the next level, words, prefixes, suffixes, and infixes may be defined as meaningful sequences of sounds: in other words, the individual elements at the level of morphology may be given a phonological description, and also a semantic description. Likewise, the elements of sentences are morphemes, and sentences likewise are meaningful. "Discourse" is used to refer to sequences longer than a single sentence.

Thus, at the formal level (i.e., setting aside meaning for the moment) words, sentences, and discourse are all increasingly long and complex series of phonemes. It should therefore not be surprising that traditional grammars of particular languages differ as to where they draw the boundary
between these units. A few examples should suffice here. First, the boundary between words and other sub-sentential units is not always clear. The padapatha text of the Rg Veda (which implicitly contains the earliest known theory of language) treats as separate words some formations that moderns would view as clearly suffixes (e.g., instrumental plural -bhis). Similarly, Chinese is commonly described as monosyllabic: a word like jiǎo-yuán, parallel in formation to English teach-er, may be thought of as a compound of two separate words, though the English construction would never be described in such a way (the example is taken from DeFrancis 1984, 46). Second, a sentence may be composed of one word, either in a case like Latin amo, where the subject is "contained within" the verb, or in the case of Sanskrit poetry, which sometimes makes use of highly complex, compound words.

Regardless of such liminal ambiguities in linguistic description, the fact remains that all meaningful utterances are structured as linear sequences of phonemes. Different sets of rules operate on sequences at different levels (syntactic rules operate on sentences and morphological rules operate on words), and perhaps it is best to identify units such as words and sentences not by their components but rather by the rules that allow those components to combine. Thus, we would say that "this cow is white" is a sentence not because it is composed of words, but rather because its parts are subject and predicate (i.e., its structure is defined in terms of rules like S → NP VP).

Although all meaningful units of speech are composed of phonemes, the phonemes themselves are not bearers of meaning. "The cat is on the mat" and "the cat is not a dog" have similar meanings (both are sentences about cats) because both contain the word "cat"; but a similar argument cannot be made with regard to the words "paddle" and "addle", even though both contain the sequence "-ad-". This fact about natural languages is known as duality of patterning. It is one of the more mysterious properties of language, but it clearly is helpful in making natural languages infinitely generative.

3.3.2. Infinity. Any language has a finite number of phonemes, and yet can express an indefinitely large number of ideas. The set of utterances of a particular length is finite so long as the number of phonemes is finite, so if there is (as a practical matter) an upper limit on the length of utterances then there is a finite number of sentences that can ever be generated. However, linguists treat language as infinitely generative, and in many respects it is so.
Consider that, in practice, one sentence can express a variety of meanings depending on the context of its use. Take all English sentences of no more than three morphemes in length. "John is here" is one of these sentences, but depending on the John referred to and the context of use of the demonstrative "here", this sentence can express a variety of meanings. This is cheating because the sentence can mean either "John Wayne is here" or "John Cheever is here". If we want to disambiguate our language, we can legislate that only the latter sentences are proper, and thus "John is here" does not count as a sentence of only three morphemes in length. The resolution of such ambiguities is a part of linguistic pragmatics, and thus not a function of formal properties of language alone. Yet pragmatics is an essential element of any satisfactory theory of language. Having observed this fact, let us set aside for now the issue of linguistic pragmatics.

There is no clear and firm upper limit to the length of utterances in a language. It might be that there are sentences that no one can keep in mind, perhaps because of constraints imposed by memory limitations. But it is possible to generate sentences that no one, as a result of such practical constraints, can ever understand, yet they still should count as proper sentences of a language if they are properly formed. If we think of written language rather than spoken language, we can see this problem disappearing readily. We could write a sentence that takes a thousand pages to write, which no one could possibly maintain enough attention to understand in any useful sense. Yet it would be possible to study that sentence and eventually generate a parse of it (an understanding of its grammatical structure). Thus we can say that there is no real upper limit on the length of sentences in any natural language. The rules that govern natural languages evolved under the constraints of human processing abilities, but a sentence can be grammatical even if there is no person who can comprehend it --- once the rules have evolved, they determine what constitutes a grammatical sentence.

The way that indefinitely long sentences can be generated is through the use of recursive rules, which can themselves be applied indefinitely many times. For example, take any well-formed sentence in English. There is another (and longer) sentence in English in which a prepositional phrase is attached to the beginning of that sentence. This rule (S → PP S, henceforth "the prepositional-phrase affix rule") is alone sufficient to generate an infinite number of sentences. Any actual sentence will be
of finite length (there are no infinitely long sentences), but the fact that no matter how many sentences
we have, we can get another one by the application of the prepositional-phrase affix rule assures us that
the total number of possible sentences in the language is infinite. All natural languages have a variety
of recursive rules, so sentences do not become unmanageably long too quickly.

As a practical matter, however, linguists are generally concerned with relatively short
sentences, and sentences of a thousand pages in length do not actually occur in normal discourse. So all
that most linguists need to know is that languages are indefinitely generative: in other words, it is easy
to come up with well-formed sentences of a practical length that have probably never before been
uttered by anyone. Chomsky's now famous sentence "colorless green ideas sleep furiously" was, when
first stated, such a sentence. But it is a curious one because it is hard to find a context in which it would
have meaning. The first two words, for example, seem to directly contradict one another, since by
definition nothing that is green is colorless. But this may be a problem with reality, as it were, and not
with language. Plus, there are occasions in which one wishes to use such self-contradictory language
(in fact, the name of such a construction, "oxymoron", is itself etymologically an example of an
oxymoron). A better example might be found in an episode of the television show *Gilmore Girls:
"Imagine a very large basketball player between us". Here the meaning of the sentence is perfectly
clear, but there are probably few situations in which it would occur to anyone to utter the sentence (the
model sentence was in fact proposed as a rough means of envisioning a distance of ten feet).

In any case, the problems with "colorless green ideas sleep furiously" seem to be semantic, and
given the tripartition of linguistic theory that I have assumed up until now (into pragmatic, semantic,
and formal theories) semantics is quite separate from formally constructed rule systems like the
syntactic grammar of a language. Unfortunately, it is not always as easy to separate pragmatic,
semantic, and formal considerations. The examples I have given already should go some way towards
supporting this claim. But to give another example of how semantics is intertwined with formal
properties of language, consider Kennedy's famous misstatement "Ich bin ein Berliner". In English,
predicate adjectives denoting nationality take an indefinite article, but this is an exception to the more
common situation in which predicate adjectives take no article: thus, "I am an American" vs. "I am
tired". The same rule for adjectives of nationality does not apply in German, so the idiomatic statement in German would be "Ich bin Berliner". There is no way to identify adjectives of nationality without examining their meaning, and thus in this case semantics affects the distributional properties of syntax.

3.3.3. **Object and Method in Linguistics.** The variety of levels of description of language makes it difficult to develop one comprehensive theory in linguistics, and in fact the current state of linguistics is (and here I speak very much as an outsider to the discipline) one of small fiefdoms. On the one hand, many linguists focus on one type of phenomenon or one method of analysis --- phonology, syntax, historical linguistics, computerized parsing, pragmatics, etc. --- and on the other, there are a variety of theories within each of these areas. Theories often share enough in common that they are in general agreement on many basic principles, and this allows the sharing of many research findings. But these fractures in the discipline, while not inhibiting the advance of linguistics as a discipline, do make it difficult for students of non-linguistic social phenomena (e.g., ritual) to adapt linguistic models to their own purposes. Staal's theory is a good case in point. His theory was developed in the mid-1970's while he was working on the data collected by his Agni project, and it was first published in 1979. But even though he was publishing on ritual well into the 1990's, the linguistics on which he bases his analyses never requires knowledge of advances after Chomsky's *Syntactic Structures* (1957).

Perhaps this minimalistic approach to theory is a good one, since the argument could be made that the earliest foundations of contemporary linguistics are liable to be the most widely shared by different schools in the present. But in fact this is not the case, and Staal's choice of a very simple linguistic model runs the risk of at once being too underdeveloped (it does not have a sufficiently developed apparatus to handle the complexity of ritual structure) and too overdeveloped (it intentionally cuts off methods that might prove necessary for the analysis of ritual structure). Whether it does in fact suffer both failings is hard to prove, and would require substantial further research. Tentatively, I will say that I do think it does suffer in both ways. The ritual study that can flow from Staal's theory should serve to identify the sorts of formal models that are necessary in ritual studies, and this should in turn
help direct scholars of religion to the linguistic theories that are most useful for their subject. I will now 
mention one topic that is particularly relevant to ritual analysis: the role of corpus-based studies.

The earliest modern European linguists were in agreement that a theory of language should be 
defined relative to a set corpus of texts; perhaps the clearest exponent of this position was William 
Dwight Whitney (1827-1894). The corpus of texts in classical Sanskrit, or Greek, or Latin, was closed 
at the end of the ancient period of these languages, even though composition in these languages has 
continued up to the modern period, and historical grammars of these languages are chockfull of rules 
clarifying exceptions to more general rules --- all in the service of accounting for every variation in 
speech known from the classical texts and inscriptions. The psychological turn in linguistics 
occasioned by Chomsky rejected this style of corpus-based study through its emphasis on competence 
as the ability to follow internalized generative rules. Thus the current emphasis on the infinite 
generative capacity of language: a proper grammar of classical Greek, under this new paradigm, should 
account not only for all the extant texts, but also for all the texts that were not preserved and all the texts 
that could have been written, but were not. Chomsky's early work was divided between positive 
contributions to a new model of linguistic work and critiques of models widely accepted among 
linguists in the 1930's through the 1950's.

Particularly singled out for critique were arguments that natural language production was 
equivalent to a finite-state Markov process (MM). Although Chomsky's attack quickly put an end to a 
line of research that had gained particular speed in the linguistics community after the publication of 
Shannon 1948, Markov models began to gain power again starting in the 1970's (the same time that the 
incipient consensus on linguistic methods was shattered by the rise of generative semantics), especially 
among engineers whose interests focused more on the implementation of working computational 
devices for linguistic processing than on purely theoretical work. I will return to this topic shortly when 
I discuss the problem of linguistic complexity, but for the present it should be emphasized that 
engineering approaches to language processes brought a renewed emphasis on the use of linguistic 
corpora --- but with a difference. It was acknowledged that the set of all possible sentences in any 
natural language was infinite, but corpora were needed to train programs, and to define the success of
particular parsing algorithms (a particular program might have a significantly higher success rate when applied to one corpus than to another, especially when the two corpora differed in vocabulary, sentence length, style, etc.). The problem of corpus use takes on yet another role in the study of ritual structure, as will be made clear shortly. But I should emphasize that Staal's reliance on early Chomskian linguistics, while ignoring thirty or forty years of development in the field, also sidesteps alternative models that might actually prove quite helpful in the study of ritual structure.

3.3.4. Distributional and Phrase Structure Models. In this dissertation I use the word "distributional" in two, related senses. First, I use the word to refer to refer to any study focusing on the basic elements of a ritual and their ordered sequence of performance. This is the primary sense in which I have used the term thus far, and the primary sense in which I will use it in reference to ritual. Most of the theories of ritual that use syntactic models pay relatively little attention to the fine-grained structure of ritual, but rather look at the relationship between dominant symbols; and often they ignore the actual order of performance of actions in favor of what we might call the logical order of elements. My first use of the term is somewhat of a neologism --- since there have been few if any true distributional studies of ritual up until now, there has been no need for a name for this approach to ritual.

The second sense of the term is the one that is used in linguistics, and it is the source of the first sense of the term. When linguists talk about a distributional analysis, they mean a study of all of the linguistic environments in which an element (a sound, a word, etc.) may occur. The distribution of an element is, therefore, the set of all these environments. Thus, in English an adjective may occur before a noun, after an article, at word-initial position in a sentence (for emphasis, typically), between adjectives in a series, etc. Any particular adjective may not occur in all these positions, and sometimes a given adjective may occur in positions not typical for adjectives --- for example, "general" may occur after "surgeon" (i.e., in French word order) but not after "anaesthetic".

One main purpose of distributional analyses was to provide a means of explaining language structure without resort to semantics. Just as in the study of ritual, so in the study of language meaning is a slippery issue, and one of the main goals of twentieth century linguistics was to explain as many
facts about language as possible without any reference to meaning. Distributional analyses were based in the same, early corpus-based approach that I have already referred to, and were very positivistic in some respects. If we think of a language as a set of texts, for example all Greek texts from Homer until the beginning of the Hellenistic period, then we can find all the occurrences of a given word, and chart all the contexts in which this word occurs. This allows for a concise description of the language, which is more than simply an edition of the corpus. It also avoids all reference to the function or meaning of words, and therefore accords with a positivist theory of linguistics as a science.

Clearly, then, my initial characterization of (the second sense of) distributional analysis was somewhat misleading, since I made reference to the contexts in which we find adjectives. A distributional approach to language will initially chart the distributional properties of each morpheme in the language under study, and might at a later stage define a part of speech (like adjective) as a set of words that share certain distributional characteristics. The categorization into parts of speech is the result of a distributional analysis: it is not antecedent to analysis, and it is only possible to the extent that the evidence of the corpus under study supports it. Many generalizations based on an intuitive understanding of a natural language will not be supported by the evidence of any given corpus of texts, even a fairly large one; and equally, many generalizations that are supported by a given corpus will be disproved by a larger corpus.

In addition to this theoretically-driven approach, distributional analysis has also been important for philologists, who combine formal descriptions of language with a careful study of the semantic and contextual environment in which that language occurs in texts. Thus, while rejecting the positivism of linguistic distributional analysis, they have utilized some of the same methods in developing a method of language study that is as much an art as a science. One of the key genres of reference publication among philologists was, until computer technology made such publications unnecessary, the concordance. A concordance is an edition of a text in which each line of text is printed not once and in the traditional order, but rather as many times as there are words in the line, and alphabetically according to each word it contains. One uses a concordance to locate all the occurrences of a given word, and to inspect the immediate textual environment in which it occurs. Frequently recourse will be
made to both a concordance and a traditional version of the text, using the former to locate passages that are then examined in more detail thanks to the latter.

The process by which a distributional analysis proceeds is the partitioning of texts into progressively smaller and smaller units, until elementary units are reached. This process is called "immediate constituent analysis". In the corpus of Shakespeare's plays, for example, one finds the following lines, all of which contain the word "laid".

Plots have I laid, inductions dangerous,
  King Richard III, Act I, Scene I, line 32

Till she had laid it and conjured it down;
  Romeo And Juliet, Act II, Scene I, line 26

our plot is a good plot as ever was laid: our
  1 King Henry IV, Act II, Scene III, line 17

... his ceremonies/
  laid by, in his nakedness he appears but a man; and
  King Henry V, Act IV, Scene I, line 105

Not all these, laid in bed majestical,
  King Henry V, Act IV, Scene I, line 267

The plot is laid: if all things fall out right,
  1 King Henry VI, Act II, Scene III, line 4

Since Shakespeare's verse is structured at a number of different levels --- for example, it is divided into both lines of verse and English sentences, each of which can be identified on purely formal grounds --- we can (if we choose) analyze the text for distributional characteristics of a given word within the line in which it occurs, or alternatively look at distributional patterns across an entire sentence. In the above examples, I have given only one line, except in the one case in which the clause in which "laid" was contained extended into the previous line.

These examples provide several immediate contexts (verbal phrases) for "laid": "laid" simpliciter, "is laid", "was laid", "had/have laid". In addition, it provides the alternate word orders "have I laid" and "she had laid". An elementary knowledge of traditional English grammar is sufficient to recognize the similarity between these two clauses, but in a distributional analysis the similarity is less
obvious (though perhaps no less real). We only know that "had/have" goes with "laid" once we
determine that these are alternate forms of the same verb ("have" and "had" are pronounced and spelled
differently, so absent a relatively sophisticated analysis it is no more clear that they are forms of the
same word than it is clear that "hat" and "hammer" are not). And since "had/have" is not contiguous
with "laid" in both cases, it is not immediately obvious that "laid" is more closely connected with
"have" (together they constitute a past tense form of the verb "lay") than with "I". Similarly, the forms
of "I" and "she" have no phonological similarity, and while they are both listed in traditional grammars
as forms of the personal pronoun, it would take a great deal of analysis of English texts to identify the
distributional similarities that sustain this categorization. While I have limited my examples to verb
forms and the simple subject-verb unit, a complete distributional analysis would be much more
thorough.

In summary, the distribution of words in their linguistic environment was the preferred method
of linguistic study in the 1930s through 1950s, through the application of immediate constituent (IC)
analysis. In addition, it was used much earlier in a less "scientific" fashion by philologists, and
continues to be used in this way to the present. If we think of distributional analysis less in the highly
technical, positivist sense of the mid-twentieth century, and more as a general concern with the
elements of a sentence and their sequential ordering, then we have the model for my proposed
distributional study of religious ritual.

A distributional analysis can usefully be contrasted with a phrase structure analysis. The
phrase structure component of a grammar describes the structure of a sentence not as a linear sequence,
with increasingly small constituent structure, but rather in hierarchical terms and with reference to
grammatical relations. Grammatical relations in a distributional analysis are subsequent to study of a
corpus, and depend on the outcome of the analysis. In a phrase structural study, in contrast, they are
theoretical primitives, and while the theory as a whole can be rejected if the presumed grammatical
relations are found not to correspond to the structures occurring in the language, they are nevertheless
determined in advance of analysis. The grammatical rules that Staal presumes for Vedic ritual, for
example (6) above, $P \rightarrow p_1 \ D \ p_2 \ D \ p_3 \ p_4$, and their linguistic analogues, such as $S \rightarrow \ NP \ VP$, presume

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grammatical categories like noun phrase and grammatical relations like agreement in number. They also underwrite the structuring of sentences into phrases. When Staal uses tree structures to describe ritual syntax, he is adopting a phrase-structure model.

IC analysis recognized hierarchical relations, just like phrase structure grammars do, but IC analysis was used only as a tool for the identification of distributional regularities. A distributional model need not focus attention on hierarchical relations between phrases. In contrast, hierarchical relations are an essential fact of phrase structure grammars.

As terminology for the study of ritual structure, I wish to treat distributional analysis as a general category: any analysis that focuses attention on the serial ordering of actions within a ritual is distributional. Phrase structural analysis (as practiced, for example, by Staal) is, therefore, a subcategory of distributional analysis. Whereas the terms are used in linguistics to identify contrasting approaches, I treat them as more versus less general. I also sometimes use the word "distributional" in a more limited sense that is closer to its use in linguistics. If we have reason to define a formal class without any reference to meaning relations, we can define that class solely in terms of the distributional contexts of elements that we wish to have belong to that class. An example of this usage will be found in section 3.4.3. But generally I use the word in the more general, and less theory-laden, sense.

3.4. Syntactic Analysis. Chapter 1 presented a detailed analysis of the syntactic argument made in Staal's foundational paper "The Meaninglessness of Ritual" (Staal 1979a). I now wish to return to the point where that analysis left off. Let me begin with a brief overview of the topics that I have already discussed.

Staal presents a large-scale analysis of the Vedic śrauta rituals. By large-scale I mean that he is looking at the most complex rituals within a very sophisticated ritual tradition. The rites that he identifies as elements of larger rituals are themselves composed of very, very many smaller elements and the analysis of these larger rites down to the level of individual actions is a complex task that would take many hundreds of pages to lay out in detail. The interested reader may compare the first volume of Agni (Staal 1983), in which only a partial analysis of the Agnicayana down to the level of individual
rites takes up approximately 500 pages, to the brief characterization of the structure of this ritual given, for example, on page 101 of Staal 1989a:

In C [the Agnicayana], a performance of A [Agniṣṭoma], fourteen performances of P [Paśubandha] and numerous performances of D [Darṣapūṭamāśa], some of which already embedded in A and P, are embedded.

There is not necessarily anything wrong with the choice to focus (as Staal does in this passage) on the largest objects within this ritual tradition. After all, Staal's claim is that embedding and transformation occur at all levels, and his detailed study of the Agnicayana, made the ritual an obvious example for his theory. But some confusions over his theory might have resulted from this choice --- confusions on the part of his critics, and perhaps also on his part.

Staal's ritual notation relies on three different formalisms: sequences, arrow-structures, and trees. A sequence is the listing of actions or groups of actions that are traditionally performed in a particular order. Arrow-structures and trees are two formalisms that capture the same phenomenon, namely rules, though trees also contain additional information. Single arrows relate one or more symbols on the left-hand side to one or more symbols on the right-hand side, and are used by Staal for what he calls "embeddings"; double arrows relate trees to trees, and are used for "transformations". Trees can also be used to describe the repeated application of rules; in this case, they are equivalent to a series of single-arrow structures. Individual alphabetical symbols, with or without indices (subscripts and superscripts), are used to denote actions or sequences of actions. An example from the use of tree-structures in linguistics illustrates what these formalisms are intended to accomplish: the complete structure of a ritual can be described by a tree-diagram, just as the complete structure of a sentence can be described by "diagramming it", i.e., by drawing the appropriate tree-structure. A brief review of the diagrams in Chapter 1 should be sufficient to bring these points to mind.

Finally, there is an explicit distinction between terminal and non-terminal characters. In the case of a natural language sentence diagram, the terminal characters would be words and non-terminal characters would be abbreviations for parts of speech. Thus a sentence S would be composed of a noun phrase (NP) serving as subject and a verb phrase (VP) serving as predicate. The noun phrase might
contain an article and a single noun (the book); the verb phrase might contain a verb in the passive (V) and prepositional phrase (PP); the latter might be composed of a proper noun (N) as agent following the preposition itself (P) (was written by Staal). The corresponding tree diagram would look as follows:

```
S
  NP  VP
    ART  N  V  PP
       the  book  by  Staal

was written
```

In the case of the tree diagram for a ritual, there may not be an equivalent distinction (as I pointed out in Chapter 1). I stopped the discussion at that point to avoid complicating my presentation too much. Now is the time to redress that problem, and to consider all the difficulties with Staal's application of linguistic formalisms --- and to see if it is possible to salvage his approach.

3.4.1. Staal's Analysis of Ritual I (Terminals). I will begin with this problem of terminal and non-terminal characters. Clearly Staal gets into trouble because he is dealing with very complex rituals; this allows him to get away without providing any symbolic representation for individual actions. I will argue that this causes problems for his analysis because terminal characters play an important role in any syntactic parse. In the tree diagrams that he draws, even the final line of the derivation lists complex sequences of actions. In the following diagram (Staal 1989a, 107):
Staal tells us that the symbol $d_1$ indicates the recitation of 15 sāmidhenī verses, whereas $d_1^*$ indicates the recitation of 17 sāmidhenī verses. He is not clear as to exactly what $d_2$ and $d_3$ represent, but between them the three symbols represent the entire performance of the darṣapūrṇamāsa īṣṭi, a rite that typically takes two full days to perform. Thus each symbol represents a complex series of actions, and even though $d_1$ and $d_1^*$ represent small portions of the total ritual, they nevertheless are not unitary ritual actions.

It seems to me that terminal characters in a ritual analysis must correspond to unitary ritual actions (I will clarify what I mean by a unitary action in the next section). This is not an assumption Staal ever makes, but it seems an obvious result of his appropriation of linguistic methods. After all, the primary elements of sentences are words, and rituals are composed similarly of actions (including speech actions).

If we agree that the complete analysis of any ritual will only terminate when all the tree branches end in unitary actions, then we will recognize that what Staal presents as ritual analyses are simply "model[s] which exhibit abstract ritual structure" (Staal 1989a, 102). In other words, he is presenting simplified models that do not represent all the complex details of particular rituals, but which accurately represent the processes that structure those rituals. As he points out, the natural sciences do this all the time. Friction with the air always limits the ability of objects near the earth to achieve the freefall entailed by the action of gravity, but elementary physics textbooks omit any treatment of friction because it would only complicate their presentation. The action of gravity exactly follows from
theory, even if sufficiently careful experiments will find that no body outside of a vacuum will fall in accordance with that theory alone. 24

There is, however, always the danger that simple models will not be sufficient to capture the basic properties of the object of study. In such a case, not only does the model not exactly describe observable properties (this happens in the case of freefall, but is irrelevant to the accuracy of the theory), but rather it does not properly model the phenomenon under study (unlike the gravitational case). I am afraid that Staal's simplifications are not as innocuous as he would have us believe.

The problem of modeling structure is not an easy one. On the one hand, any complex construction may be broken into parts in a number of different ways, and hence exhibits different structures at once. On the other hand, some structural analyses are more important than others in identifying important principles that underlie the complex in question. Staal understands all of this, though many scholars do not. To give an example, let us assume that we wish to interpret ritual structure cognitively on the analogy to Chomskian linguistics: then the ritual structure we wish to locate is the one that corresponds to the competence of ritual performers. On the linguistic side, we can imagine a speaker thinking as follows 25: "what I have just said ('the book', to take the example from (9) above) counts as a complete noun phrase (NP) because it is composed of an article and a noun (ART and N), so I need a predicate next; I want to talk about the generation of the book and the agent of its generation, so I want a verb phrase (VP) that allows for the mention of an agent; that means I need a passive verb and a prepositional phrase introduced by the word 'by'..." In other words, the way that the speaker knows what word comes next is because he has a mental representation that contains the same information as (9). This means that the structural analysis presented in (9) has privileged status, since it is the analysis that the mind makes in the process of sentence generation. The ritual analogy should be exact here. The best analysis for a science of ritual is one that corresponds to the mental process followed by a competent ritualist.

This leads us to the reason why an analysis that does not end in terminal characters is suspect. Such a partial analysis is not perspicuous. One way we can check an analysis like (9) is to see if it terminates with the right sequence of words, and thus it allows us to verify that it is an analysis of the
sentence we are examining. The danger is not that this is not the best analysis among alternatives, but rather that this is not a proper analysis of the sentence in question --- perhaps it is not even the correct analysis of any sentence. Staal gives us independent reasons for this particular worry.

The choice of $d_1$ and $d_1^*$ as elements of $D$ in (11) results from the fact that Staal has already chosen the kindling (sāmidhenī) verses of the darṣapūrṇāmaṣa as an example of the process of embedding (Staal 1989a, 91-92). Thus his analysis in (11) is heuristic: it is chosen to make a particular point, and there is no special reason to think that it corresponds to the actual cognitive structure of the ritual.

Several morals can be drawn at this point. First, a necessary feature of any complete ritual parse is that each of its branches end in a terminal character.

Second, Staal's examples of ritual structure are intended to emphasize particular features of ritual structure, and they are not intended as segments of complete ritual parses.

More generally: the accuracy of one part of a ritual analysis is underwritten by the accuracy of the analysis of the ritual as a whole. I will be able to argue for this principle in more technical language after I introduce the notion of phrase-structure, but for now it should suffice to say that an analysis driven by heuristic motivations has no particular claim to being the correct cognitive analysis.

3.4.2. Unitary Actions. I assumed above that it is clear when a tree structure has reached the point of terminal characters. In point of fact, this is not as straightforward as it would appear. Earlier I gave examples in which it is not clear what constitutes a single word in the analysis of particular languages, and the problem is similar here. Theories, in any field, have a certain holistic quality to them, and there is always a certain circularity in developing them. The key is to ensure that any holism is mostly harmless. In the case of natural language, I proposed that individual words can be identified (to some extent) in terms of the operation of linguistic rules, and I think the same approach should work in the case of ritual. As a practical matter, we can assume that, if the parse of a given ritual is full and complete, then the ends of tree structures will be terminal elements. This does not guarantee that the resultant theory is a correct cognitive theory of ritual competence, of course, but there is in principle the
possibility of empirical confirmation of the correctness of a given theory of ritual structure. And while observation underdetermines theory, it does provide grounds for choosing between particular theories.

In any case, there should be such a thing as a "unitary", or atomic, action, which is defined relative to a theory of ritual structure. The action might be anatomic (i.e., composed of parts) in some other sense, just as words are composed of phonemes, but to the extent that words are atomic from the perspective of language syntax, unitary actions should be atomic from the perspective of ritual syntax.

Since Staal's theory has been developed only to a very limited extent, it would be hard to definitively rule on the accuracy of his ritual analysis --- though in the next section I will make a tentative attempt to do so. Another means of guarding against dangerous theoretical circularity is also available. We can rely to some extent on our intuitions of what constitutes a single or unitary action. But most of all, such debates might be moot in many cases. The lower down the tree our analysis leads us, the greater the chance that any mistakes we make will not affect the overall success of our analysis. Consider the example I used earlier of the word "teacher". Perhaps we should analyze this as two words. There is little reason to do so, given the structure of English, but let us assume that for some reason the possibility worries us that "teacher" is not a single word. Even if we do choose to separate the suffix from its stem at the final level of our parse, it is unlikely that "-er" will end up being associated more closely with anything else in the sentence than with "teach-", so our analysis of this (possibly compound) noun will not affect our parse of anything else in the sentence. Perhaps a better example would be sentence-final prepositions, which if they are particles should probably be treated as adverbs, or if true prepositions should be associated closely with a noun. In any case, this choice would be little more than a small detail and would likely affect our overall analysis very little.

In short, my approach leaves me potentially open to criticisms similar to those I leveled against Staal, but this is no reason to doubt my approach as a whole. It does mean that any theory of ritual structure needs to be examined carefully and critically.

3.4.3. Staal's Analysis of Ritual II (Categories). If we set aside consideration of terminal elements and look just at medial elements, we will find other reasons to criticize the ritual analyses that Staal provides. A few examples should help to clarify the issues at stake here. In the dārṣaptīramāsa,
which serves as the model for all īṣṭi rites, eleven verses are recited while sticks of firewood (sāmidha) are placed on the altar. But the number of these sāmidhenī verses increases to thirteen when the darṣapūrṇamāsa is embedded in a prototypical animal sacrifice (paśu bandha), and this number increases to twenty four when the object of an animal sacrifice is the god Vāyu. Staal describes the insertion of the sacrifice to Vāyu (Vāyavyaṃ Paśu) in the Agnicayana as follows:

\[ AC \rightarrow ABC \quad (12) \]

B represents the Vāyavyaṃ Paśu itself, A represents the preparation of the ukhā pots, and C the selection of priests. Likewise, B itself has an internal structure, which may be represented as follows (where E represents the recitation of thirteen sāmidhenī verses and D and F represent the actions that precede and follow this):

\[ B \rightarrow DEF \quad (13) \]

Clearly, ABC does not represent the entire ritual of the Agnicayana, but rather only a small part of the performance on the first of twelve days. To get an idea of how much goes on during this day, consider Staal's "bird's-eye" description of the first day of the ritual (after Staal 1983, 56):

yajamāna and his priests enter the ritual enclosure

ukhā pot (A) and heads of a horse, a man, a bull, a ram, a goat and bricks are made from clay

Vāyasyaṃ Paśu (B)

chief priests are selected (C)

fire is produced by friction

dikṣaṇīyeṣṭi performed

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dikṣa of yajamāna performed

yajamāna crawls on antelope skin and takes up a turban and a staff

yajamāna takes up the ukhā pot, which is filled with fire, and takes three steps with it

We should therefore take ABC to represent only one segment of the complete ritual, which has been treated as a complete unit for the sake of convenience. Some notation like the following would make this clear:

\[ \ldots AC \ldots \rightarrow \ldots ABC \ldots \] (14)

Staal introduces the idea of transformations by reference to the Vāyavyaṃ Paśu as well. In (15) (Staal 1989a, 96), he derives the Vāyavyaṃ Paśu (to the right of the double arrow) from the regular animal sacrifice (to the left of the double arrow) as follows:

\[ \begin{array}{c}
\text{A} \\
\text{D} \\
\text{E} \\
\text{F}
\end{array} \quad \Rightarrow \quad \begin{array}{c}
\text{A} \\
\text{D} \\
\text{G} \\
\text{F}
\end{array} \quad \begin{array}{c}
\text{B} \\
\text{C}
\end{array} \quad \begin{array}{c}
\text{A} \\
\text{B} \\
\text{C}
\end{array} \]

(15)

B includes the recitation of thirteen sāmidhenī verses (E) in the regular pašubandha, and twenty four verses (G) in the Vāyavyaṃ Paśu, which counts as a modification of the basic pašubandha. Staal includes transformations in his ritual theory alongside embeddings because we find variations on a common pattern in different rituals --- in this case, different numbers of sāmidhenī verses in different contexts within the Vedic ritual system. To say that (12) describes the recitation of sāmidhenī verses in different rituals would not be correct, since this would mean that all performances of the pašubandha

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contain the recitation of precisely thirteen sāmīdhēṇī verses (remember that this is what E represents). (11) gives an instance where different versions of the dārṣapūrṇaṃśa are embedded in the same performance of a larger ritual (in this case, the Agniṣṭoma). In the latter case, d₁ represents the recitation of fifteen sāmīdhēṇī verses, and d₁⁺ represents the recitation of seventeen such verses.

Now, we have thus far seen partial analyses of two different rituals, the Agnicayana and the Agniṣṭoma. We also have seen four versions of the recitation of sāmīdhēṇī verses --- with thirteen, fifteen, seventeen, and twenty four verses --- not to mention the basic model of sāmīdhēṇī recitation, in which eleven verses are recited. Since he has discussed two different rituals, Staal has not made the effort to keep his use of symbols consistent, using B, G, d₁, and d₁⁺ to represent the four variations on the model recitation of eleven sāṃīdhēṇī verses. Even if he had been more consistent, for example using d₁, d₂, d₃, and d₄ to represent the four variants, there would still be something puzzling about his choice of symbols. The difference between the symbols d₁, d₂, d₃, and d₄ does not indicate the most significant difference between these rites, namely that each is an iteration of one basic rite, namely the recitation of a sāṃīdhēṇī verse. This similarity seems important for understanding the principles that structure Vedic ritual, but Staal’s notation does not emphasize it at all.

Admittedly, the same verse is not recited over and over again, so each dₙ is not an exact duplication of the preceding dₙ₋₁. But nevertheless, Staal has failed to capture an important regularity, and it is worthwhile asking whether this omission is significant as well as being curious. Let us introduce a further level of analysis to the one that Staal provides in the righthand side of (15). Let us say that G is composed of twenty four instances of sāmīdhēṇī recitation, g₁ to g₂₄:

\[
\begin{align*}
g₁ & \quad g₂ \quad g₃ \quad g₄ \quad g₅ \quad g₆ \quad g₇ \quad g₈ \quad g₉ \quad g₁₀ \quad g₁₁ \quad g₁₂ \quad g₁₃ \quad g₁₄ \quad g₁₅ \quad g₁₆ \quad g₁₇ \quad g₁₈ \quad g₁₉ \quad g₂₀ \quad g₂₁ \quad g₂₂ \quad g₂₃ \quad g₂₄
\end{align*}
\] (16)

I used lower-case letters because, following Staal’s approach, I treated these as atomic actions, or terminal characters. Yet this analysis does not capture an important point: every gₙ is essentially the
same as every other \( g_n \). I say "essentially" because each is the recitation of a different verse. But they are all recitations of sāmīdhēni verses. So we would be justified in pursuing an alternate analysis, according to which (16) was generated by repeated application of the following rule:\(^2^7\):

\[ X \rightarrow XX \quad (17) \]

According to this analysis, we can say that every \( g_n \) is an example of a general class of action, namely the recitation of a sāmīdhēni verse. Then we would expect the parse of the ritual in question (Vāyavyaṃ Paśu) to contain two steps after what Staal provides in the righthand side of (15). First, we would get a sequence of twenty four identical capital letters under the G symbol, by rule (17), and then we would substitute a \( g_n \) for every X and end up with (16) on the final line of the parse. The resulting parse would look like this:\(^2^6\):

\[ \text{Diagram} \]

This is simply applying Staal's established approach in order to provide a finer-grained analysis than Staal gives, for one part of the Vāyavyaṃ Paśu. But we can go further. Rule (17) produces a sequence of exact duplicates, and we get the sequence of recitations that actually occurs in the ritual (see Staal 1989a, 92) in the final step in the parse, when we replace each X with a different \( g_n \). This analysis assumes that all the \( g_n \)'s are instances of the same type of recitation, namely the recitation that accompanies the offering of a sāmīdh. This corresponds to the case in natural language where
morphemes (words) only occur as terminals in a parse: elsewhere we find classes of words, or rather positions in a sentence that may be occupied either by other class symbols, or by words chosen from certain lists. Thus in (10) above the symbol N may be replaced by any terminal item from the list [book, Staal, ...]. ART may be replaced from [the, a], and NP may be replaced by [ART N, ART ADJ N, N, ...]29. I proposed earlier that Staal's theory, if taken to a natural conclusion, will have to identify all of the unitary actions that may serve as parts of ritual performances; these will be represented in a ritual parse by terminal characters. All other characters --- "medial characters" in my usage --- will be like these part-of-speech classes. The parse will not be complete until they are replaced, in other words when terminals are substituted for all medial characters.

The process of identifying the possible classes of ritual action is therefore a necessary part of any syntactic theory of ritual, and unfortunately it is one that Staal has not devoted any attention to. As it happens, sāmidhāṇī verse is probably not a good category; verse recitation from the Rg Veda is liable to be a better choice; or perhaps (as in the case of the Vāyavyaṃ Paśu) verse recitation from the Rg Veda or Taittirīya Saṃhitā; or saṃhitās generally. This is because a verse recitation is identified as a sāmidhāṇī recitation if, while the Hoṭr priest is reciting that verse, the Adhvaryu priest is offering a sāmidh into the fire. For the sake of convenience, I have made the assumption that we can describe a ritual as a linear sequence of actions, knowing full well that many rituals, in the Vedic and other traditions, are more like symphonies, with different actors performing different actions simultaneously. The score to a symphony shows simultaneity by printing the linear sequence of notes for each performer on a separate line. If we wanted to treat sāmidhāṇī recitation as a class of action, we would have two alternatives. Either we could parse the action sequences of multiple actors in parallel, which would introduce a level of complexity not found in linguistic analysis, or we would have to find distributional features in the actions of the Hoṭr that would be sufficient to differentiate such recitations from recitations of other types. Whether the latter choice is a possibility depends on how the Vedic ritual system is constructed, and therefore cannot be stipulated or determined at the outset of analysis.
But it seems preferable to analyze ritual action, if possible, in terms of a linear sequence, and not to have to correlate multiple strings.

A careful study of the Vedic ritual tradition will probably allow us to identify a number of classes of action, and may allow us to determine a list of rules that govern the sequencing of such action classes. If the syntax of a ritual system is actually like the syntax of a language system, then this will have to be possible. Put differently, rituals are not really like languages if we cannot identify a reasonably small set of ritual action classes that account for the sequences of actions we find in actual rituals within one ritual tradition.

In the Vedic Ṣṭṭi rituals, for example, we find a number of components that are repeated, with slight variations, both within and between various Ṣṭṭis. Earlier in this chapter I presented Minkowski's analysis of the Ṣṭṭi. The major units of the ritual (looking only at the middle section) form the following sequence

\[ \text{prayāja -- ājyabhāga -- pradhāna -- upāṇśuyāja -- pradhāna -- sviṣṭakṛt -- anuyāja} \]  \[(19)\]

The central element of each of these rites is a yājyā, a recitation by the Hotṛ that accompanies a libation by the Adhvaryu. In the middle rites of this sequence (ājyabhāga through sviṣṭakṛt), the yājyā is preceded by an invitational prayer (puronuvākyā/anuvākyā), but the invitational prayer is missing in the prayāja and the anuyāja. If we follow Āstall's lead and look only at the sequence of certain actions within a ritual sequence, then we find the following order of actions at the core of an Ṣṭṭi:\[30\]:

\[ \text{Y IY IY IY IY Y} \]  \[(20)\]

More properly, we should at least indicate where other actions intervene:
In any case, it seems clear that this is a case of the more general sequence of seven instances of $S$ (i.e., offerings of ghee, or clarified butter, accompanied by recitations):

\[ S \rightarrow S \rightarrow S \rightarrow S \rightarrow S \rightarrow S \rightarrow S \quad (21) \]

(21) can be generated from a rule similar in form to (17):

\[ S \rightarrow SS \quad (22) \]

Furthermore, (20) can be derived from (21) by application of the following two rules:

\[ S \rightarrow Y \quad (23) \]
\[ S \rightarrow IY \quad (24) \]

In short, it would appear to be the case that a well-formed Vedic ritual can contain a sequence of ghee offerings, which are composed of either a yājya or a yājya preceded by an invitatory prayer. If I were to fill in the ellipses in (20) and attempt a complete parse of the īṣṭi, probably rules (23) and (24) would have to be replaced by rules with longer sequences to the right of the single arrow. But otherwise this provides a good outline of the sort of rules that are used in constructing a Vedic ritual. In the case of the īṣṭi, the sequence $Y/IY$ is typically preceded by an āśrāvaṇa and a pratyāśrāvaṇa, and followed by a tyāga. Thus as alternatives to (23) and (24), we could write:

\[ S \rightarrow RPYT \quad (25) \]
\[ S \rightarrow RPIYT \quad (26) \]
As we supplement rules (25) and (26) still further, we will find that some sequences are not as universal as we had suspected: just as IY is not an invariant sequence, but alternates with a simple Y, so it might happen that RP is not an invariant sequence, or it is but it only optionally precedes Y. In any case, the basic approach should be clear even if these rules are, as stated, quite insufficient to accurately describe any Vedic ritual.

The point remains, however, that Staal's assumption that ritual is structured syntactically like language assumes that (approximately) the same degree of regularity in structure is found in both. If rituals are such that it is virtually impossible to anticipate the next action to be performed based on what was done previously in the rite, then a linguistic model will prove unsatisfactory for ritual theory. This impossibility would amount to a case in which the number of rules in a ritual tradition is much larger than the number of rules in a natural language, either because there are no sequences that occur with regularity in different contexts (so that virtually no useful generalizations can be made at all) or that the rules that can be identified have so many exceptions that they are scarcely rules at all. The actual judgment as to whether a linguistic model will prove unsatisfactory for ritual theory cannot be made until significant effort has been devoted to identifying the rules underlying ritual sequences in at least one ritual tradition, and this in its own right would require extensive research.

If ritual is structured by truly syntactic rules, we would expect their categories to be hierarchical as in the case of natural language. Then we might find, for example, that there is a general category of prayers, and within this the category of prayers that precede action, and another category of prayers that are said while actions are underway. Prayers that precede action might then be subdivided in turn, etc. To extend the analysis of the Vedic īṣṭi, for example, the yājya is preceded immediately by a command to perform the yājya; earlier, but not always immediately before, the divinity to whom offering will be made is called upon. These would seem to be distinct types of commands, since both the addressee differs in category (human priest versus god) and also because the nature of the command is different (giver versus receiver of sacrifice); yet they seem to be similar in another respect, since in both cases one priest or another is ensuring that a participant in the sacrifice will perform his role in the
actions that follow. The invitatatory prayer or call to the god is given a name in the Vedic tradition (puronuvākyā/anuvākyā), but the command to the priest does not seem to have its own name, from what I can tell from my secondary sources. And even the traditional terminology for the invitatatory prayer seems somewhat confusing: two terms are used yet this distinction does not seem to correspond to two distinct types of invitatatory prayer. Renou’s definition of the anuvākyā begins "or more precisely puronuvākyā" (Renou 1954, 12).

3.4.4. Notation for Rules. Note that Staal does not really develop rules for Vedic ritual, despite his assertions to the contrary: what he actually does is illustrate the form that such rules would take. Thus, when he states the rule that allows for the insertion of the animal sacrifice into the Agnicayana (AC → ABC), what he really is doing is giving the general form of any rule for medial embedding. He could make this clear in his ritual notation in any number of ways, for example by stating the general form of rules using Greek rather than Roman letters, αγ → αβγ, and by doing so he would remove a significant source of potential confusion for his readers. Setting aside the question of notation, however, it is indeed significant that Staal may not in fact provide an accurate, albeit partial, analysis of Vedic ritual. This is important because his language frequently implies that this is indeed what he is doing. Rather, he is concerned to illustrate the types of rules that are operative in the Vedic tradition, without getting into the details of particular rites. And generally his examples from particular rituals are intended to emphasize a particular feature of ritual structure generally rather than deal adequately with the details of the actual example that he uses.

By itself, Staal’s approach is innocuous once we have recognized that he provides only a schematic overview of what the rules of Vedic ritual actually look like. But such simplifications do leave one cause for worry. The process of developing the actually rules of ritual is liable to introduce not only quantitative problems (rituals are long, and the analysis of them would be unmanageably hard to actually present), but also qualitative problems. I have attempted to illustrate some of these problems. Section 3.5 continues this line of argument, but with a difference.
3.5. Differences between Ritual and Language. In the last section I already have pointed to two ways in which Staal's syntactic analysis is insufficient for the facts of Vedic ritual. This introduces the larger problem, which section 3.4 side-tracked, of whether the style of syntactic analysis that Staal envisioned really is appropriate to ritual. In other words, section 3.4 assumed that Staal's program was essentially valid, and critiqued his application of syntactic methods to ritual. Now I wish to ask the larger question, whether any sort of syntactic analysis is the best way to deal with the phenomena of ritual. Here I will finish the discussion begun in section 3.3 by looking at the ritual analogues of the linguistic issues addressed in that section.

3.5.1. The Size of the Ritual Corpus. I discussed in section 3.3.2 what it means to say that language is infinitely generative. According to the dominant view of language today, the set of all sentences in any natural language is truly infinite. This contrasts to the approach current in the nineteenth century, when languages were defined in terms of finite textual corpora. Staal argues that the set of all Vedic rituals is infinite, and points to the sāstras, or rituals that last a year or longer, as examples of the infinitely generative capacity of ritual. But it is not clear that rituals are infinite in the same sense that languages are.

Take the Vedic ritual system. It has a finite number of rituals. Most if not all of those rituals have variations as described in the ritual sātras, but even if we treat each variant of a ritual as an independent ritual in its own right, the total number of rituals is finite. And in comparison to the number of sentences a person is liable to encounter in the course of a lifetime, the number of separate rituals one encounters will be quite small, even if we treat each variant as a ritual all its own. Of course, the number of sentences a person encounters will also be finite, and Staal's point (which I suspect a lot of people do not fully appreciate) is that ritual traditions form generative systems, and these systems can be used to create novel rituals (ones that have never before been practiced) or to verify that novel rituals are in fact well-formed. Even if novel rituals never are created, the system defines what would count as a well-formed ritual, counterfactually.

Thus, Staal's linguistic analogy opens up a new way of thinking about rituals. But the full import of his revisioning of how rituals work is perhaps not clear. Which is more important (which tells
us more about what rituals are and how they work): the fact that rituals form a closed and relatively small set; or the fact that infinitely many new rituals could, but won't, be created in the future? Staal leads us to ask such questions, but he does not attempt to answer them.

Perhaps I have not made quite clear the full power of Staal's proposal. If not, I should do so now. Think of the Vedic ritual corpus (the total number of rituals described or mentioned in the Śrauta Sutras) as being the union of two sets: what I will call "named rituals" and what I will call "ritual variations". Named rituals are rituals that the sūtras gives individual names to, for example the Agnicayana, the Darśapūrṇamāsa, the Sarpa Sattra \(^{32}\), the Agniṣṭoma, etc. Ritual variations are named rituals that have had their form (the sequence of actions that constitutes the ritual) modified in some way so that they do not meet the normal specifications for any named ritual.

Perhaps this is not the best way of bipartitioning the Vedic ritual corpus. According to this criterion, the Vāyavyaṃ Paśu would count as a named ritual, yet it is just a traditional animal sacrifice modified due to the particular god to whom it is dedicated. The problem with this approach is that it might cede too much authority to the sūtras, assuming that their choice of language to designate rituals makes principled distinctions consistently, when perhaps it does not. Perhaps it would be better to treat only the prototypes of each general type of ritual as falling into the first class \(^{33}\), and all the rest as members of the second. Or perhaps we could extend the first class to include basic variations on the prototypical rituals, but not extend it to include all the named rituals (thus include the Agniḥotra but not the Sarpa Sattra or the Vāyavyaṃ Paśu). How exactly we choose to partition the class of Vedic rituals does not matter as much as the fact that we make some sort of partition into two groups, along roughly the lines that Staal inclines us to do.

Consider Staal's treatment of the Agnicayana. He views it as composed of smaller rites (Darśapūrṇamāsa, Paśubandha, and Agniṣṭoma) plus elements unique to the Agnicayana alone (see Staal 1989a, 101). This is the model for my first class of rituals. The Agnicayana and the Darśapūrṇamāsa, for example, are entirely distinct rituals according to Staal's analysis. Yet the former
contains multiple performances of the latter as proper parts. The difference between the prototypical Paśubhandha and the Vāyavyaṃ Paśu serves as my model for the second category. The difference between these two rituals (let us assume for the moment that this is the only difference) is charted in (15) above. In other words, the difference between these two classes of ritual is defined in terms of the twin operations of embedding and modification.

In any case, the two categories of ritual are not important for their own sake. What is important is that the Vedic rituals do not merely form a motley set, they form a system much the way the sentences of a language form a system. We can form a set of rituals in any way we might choose: we can make a list of the "world religions" and pick one ritual from each, for example. But this set would not form a system. There would be nothing holding the members of the set together besides a stipulated set membership. The Vedic rituals form a system because they are generated by a set of rules, all in the same way and with different rituals relying on the same rules. In Chapter 1, I briefly considered the structure of the medieval Christian Divine Office, and if this type of analysis were extended much further we could (following Staal's theory) develop a list of the rules that structure Christian rituals. Those rules would almost certainly be different than the rules that structure Vedic rituals, just as the rules of Latin grammar are different from the rules of Sanskrit grammar. There might be similarities, because of a shared heritage (as with Latin and Sanskrit, both of which are Indo-European languages) or because of ritual universals (analogous to Universal Grammar in linguistics), but the Christian and Vedic ritual systems would probably be as different from one another as Sanskrit grammar is from Latin.

This is all part of Staal's linguistic analogy. The structural analysis of Vedic ritual that he champions is underwritten by the claim that the rules he identifies are *the actual rules* that structure those rituals, and they therefore occupy a privileged place among all the possible structural analyses that could be produced for this set of rituals. And these are the actual rules because they are the ones that define the system of Vedic rituals. This is where the notion of infinity comes into play.

The system of Vedic rituals includes not only all the rituals described in the sūtras, or all the Vedic rituals that have actually been performed, but also all the rituals that accord with the rules of the
Vedic ritual system but have never been performed or described. Of course, put in these terms the argument is clearly circular. Something else is necessary to allow us to separate out counterfactual Vedic rituals (those that belong to the system, but have never been performed or described) from counterfactual non-Vedic rituals (those that do not belong to the system). According to the linguistic analogy, the obvious candidate is the intuitive judgment of competent ritualists. Arguments for ritual competence have been made explicitly on the analogy of linguistic competence by, e.g., McCauley and Lawson 1990 and Lawson and McCauley 2002. In any case, it is not much of a jump from here to the cognitive perspective according to which the rules that structure the Vedic rituals are encoded in the minds of ritualists, and it is in virtue of this that they are able to perform rituals. Then there would be a physical (neurological, albeit perhaps token-token) substratum underlying the rules that constitute the Vedic ritual system, and this would be sufficient to underwrite the "reality" of the system.

This is how Staal's theory should work. But the language analogy is thus far only a hypothesis, and while it is (I wish to emphasize) a very appealing hypothesis, there is evidence that might argue against it. To a great extent, the purpose of language is communication. Certainly, it also serves to reinforce social bonds, as most talk of weather and sports makes eminently clear. And much of language is "ritualistic" in the sense of being stereotypical. But the communicative function of language cannot be discounted entirely, and there is perhaps reason to believe that the infinite generativity of language is related to its practical, communicative function. An argument could be made, even on largely a priori grounds (without the detailed study of even one ritual tradition) that a meaningless system is not liable to be infinitely generative.

Such an intuition is supported by the well-known fact about rituals that they tend to be repeated, over and over, in more or less the exact same form again and again. Such situations are as much the exception as the rule in the case of language. And even when we confine our attention to relatively stereotypical language such as greetings ("hello", "what's up?") and language governed by social conventions ("may I ask a question?"), we find that an explanation for such behavior in the on-the-spot generation of sentences rather than in the recital of memorized phonemic sequences. A study of variations in language governed by social conventions may serve to make this point. We might find
that a simple sentence like "may I ask a question?" in practice varies with similar but distinct sentences like "I have a question" or "I'm sorry, but . . . " Students who have learned set phrases in a foreign language will repeat the same statement reliably in a given context, but someone who has internalized the grammar of a given language will accidentally produce variations even when trying to say the same thing repeatedly. Yet every variation will be idiomatic.

In the language case, we are comfortable saying that the intuitions of native speakers help us to distinguish grammatical variations on a sentence from ill-formed pseudo-sentences. But it is not as clear that there are similar intuitions that could help competent ritualists distinguish well- and ill-formed sequences of ritual actions. There has not, to my knowledge, been any research specifically devoted to this problem (though it would be a worthy subject for empirical investigation) but such evidence as is available may argue against the assumption of ritual competence.

Consider the problem of ritual error. The Vedic ritual tradition is known for a set of rites known as प्रायास्तित्तास, or (as Renou 1954, 116, defines them) "expiatory acts, aimed at eliminating ritual error". Practical actions can be done incorrectly, and it is good to have ways of correcting mistakes in the realm of everyday activity --- so it should not be surprising that ritual systems also include means for eliminating errors. But scholars have focused particularly on rituals that are intended to correct errors that are not known to have been committed. One example of such a rite is the Pavitra Festival, which I have discussed elsewhere.

Such rites appear puzzling because a ritualist who feels the need to make up for any possible error, despite the fact that he thinks none has been committed, may seem somewhat paranoid. Or at least this has been the worry of many Western scholars. In any case, whatever the psychological import of the Pavitra and similar प्रायास्तित्तास, they are interesting for our purposes because they might be evidence against the claim that competent ritualists possess reliable intuitions of ritual well-formedness. After all, a competent ritualist knows what actions he needs to perform, and when and how he should perform them. Therefore he has one check on the success of his performance, namely his recollection of what he did during a ritual. If he also has an intuitive sense of well-formedness, then he is liable to catch a mistake during its commission, even if he cannot immediately tell wherein the mistake lies. The linguistic analogue would be cases in which people
accidentally misspeak, then stop and catch themselves. Ritual fieldworkers should be able to identify some such situations if they do occur, but unless they are alert to the possibility they might not attend to the fact and ask the necessary follow-up questions of their informants.

In any case, I have briefly presented arguments both for and against treating the Vedic ritual corpus (and, by analogy, perhaps corpora in other traditions as well) as infinitely generative. The extant corpus is in fact finite, and indeed small as compared to the number of sentences that actually have been generated in any natural language. But it currently remains an open question whether we should look not at the corpus of actually occurring rituals, but rather at the infinite number of potential rituals that are consistent with the rules that structure actual rituals. After all, the number of sentences actually generated in any natural language at a given point in time is also finite, though very large. I have proposed that we should treat the ritual system as infinitely generative if the cognitive processes by which ritualists actually practice complex rituals are isomorphic with regularities that can be seen in observed ritual structure. Perhaps there are alternatives to this approach, however.

A further, potential problem with the linguistic analogue appears as well. Linguistic rules can be neatly divided into synchronic and diachronic. At any given time, a language is a system described by synchronic rules. Over time, some of those rules will change and consequently the language described by them will change as well. We would expect, if the analogy to ritual were strong, that a similar division in ritual rules would obtain as well. But the only time that we see new rituals generated according to the rules that structure a particular ritual system (either through changes in existing rituals or the creation of novel rituals), the system as a whole changes at the same time. So it would seem that ritual rules are necessarily diachronic --- they cannot be used to generate novel ritual structures without changing the system that they constitute. Yet from a linguistic perspective, it seems odd to have no synchronic rules.

The problem of infinite generativity is an important one for any syntactic theory of ritual, however we choose to resolve it. I have sketched out one solution, which accords with my own intuitions and the evidence I am familiar with, but by no means have I exhausted the subject.
3.5.2. Sattras. Staal presents an argument that he thinks supports his position that ritual rules are infinitely generative, which he attributes originally to the grammarian Patañjali.

The relevant statement from Patañjali goes as follows: "there are indeed linguistic expressions which are never used . . . even though they are not used, they have of necessity to be laid down by rules, just like protracted sattras" (Mahābhāṣya I: 8, 23; 9, 15; Staal’s translation). Though brief, this is quite interesting, and it accords well with Staal’s theory. Patañjali is, of course, interested in language, and his view of ritual is mostly found implicitly in his analogy to language. Let me paraphrase the passage. In speaking of words that are not said (aprayuktā, which I think we should take concessively, "though they are not said"); Staal’s translation "never used" could be read in the sense of "which never will be said", but the Sanskrit does not imply that possibility), he says that it is "by necessity" (avasyaṃ) that they accord with rules. In other words, it is part of the nature of language to be structured according to rules. Presumably he thinks that all the properties he is attributing to language also apply to ritual. Yet it is not clear that he thinks of "protracted sattras" as being (in Staal’s words) "purely theoretical", never in practice performed on the analogy of aprayuktā.

I have not fully explicated Patañjali’s position at this point in the Mahābhāṣya, and I do not intend to do so here. At the very least, Patañjali seems to agree with Staal that rituals are structured in accordance with rules, and he might also see similarities between those rules, just as Staal does. In fact, the technical language used among Sanskrit ritualists and grammarians is quite similar, as are the rule systems that they describe. Renou 1941-2 pointed these similarities out several decades ago, and argued that the grammarians were indebted to the ritualists. At this level, then, the argument of Staal and Patañjali is just what we would expect. But the comparison to sattras in particular deserves closer attention.

Sattras are particularly long rituals, lasting no less than twelve days and in some cases lasting a year or more. We are even told of rituals that take a thousand years to perform (Staal 1979a, 586), and these are what Staal means when he says that some of the sattras discussed in the ritual sutras are
"purely theoretical". Now, the Agnicayana is an incredibly complex ritual, and it lasts only twelve days. Each day is unique, and the ritual texts provide us with copious details about each day of the ritual. They do not describe the sattras in comparable detail, because that would be completely impractical in an oral and/or manuscript (i.e., non-print or non-digital) culture, since the reproduction of descriptions of the rituals would be quite cost-prohibitive. In fact, there is by no means a constant linear relation for all rituals between the complexity of the ritual and its length of performance. The long rituals that we know to have been performed historically, such as the Aśvamedha, tend to be composed of many reduplications of smaller parts (A → AA applied recursively) with very few internal transformations to make each iteration of the basic structure unique and different from the others.

Staal’s position is that the Vedic ritualists recognized the generative quality of their ritual system, and this is what allowed them to conceive of immensely long rituals. But people can conceive of immense periods of time, or distances that dwarf anything perceptible with the naked eye, and they seem to be able to do so simply by mastering a number system that is itself infinitely generative, and attributing a very high number to the length or breadth of something. Thus it is possible to posit immensely long rituals even in a ritual system that is not generative, and consequently there seems little reason to think that comments such as Patañjali’s offer substantive evidence for the infinite generativity of the Vedic ritual system.

3.5.3. Mantras. A substantial amount of Staal’s research on the Vedic ritual system has focused on mantras. I have very little to say about this topic, and this short subsection is intended to justify my lack of attention to this significant and important topic. Let me say that I think the problem of Vedic ritual language is important and deep, and I think much difficult research still needs to be done. At the same time, I think that many of Staal’s basic assumptions about mantras do not mesh well with his general theory of ritual. If anything, mantras either do not accord in many cases with the rules that structure other parts of the ritual system, or require supplemental rules not seen elsewhere in the system. I will not argue this here, however, but merely summarize Staal’s position.

Now, what are mantras? Staal 1989b, 48, defines “Vedic mantras” as “bits and pieces of the Vedas put to ritual use”, and this could be extrapolated into a general definition of mantras. But of
course the means of extrapolation used makes all the difference. Staal 1986, 44, asserts more generally that "a mantra is a ritual sound expression that may or may not be derived from an expression of language." The key is that mantras sometimes can be interpreted according to the rules of a natural language, but nevertheless they are not, *qua* mantras, linguistic expressions.

Most generally, mantras are sounds or sound sequences. Sometimes they correspond to words in a language, and historically they might descend from certain words or phrases. But the mantra is not the word that it sounds like. To assume that they are the same, just because they sound the same, is to make a mistake analogous to the etymological fallacy (1986, 62): "the etymology of a word may throw light on its original meaning, but it may have no connection with its actual meaning or use." Just as words can lose or change meaning over time, thus becoming different words, so sounds can become incorporated into ritual, losing their status as words in the process and becoming instead speech acts. Here I do not mean by "speech acts" the technical term used by Austin and Searle, but rather a more natural sense of the phrase, actions that are performed by making verbalized (word-like) sounds. Thus mantras are a type of ritual element, and the use of mantras is a type of ritual action.

Let me present the following definition of mantra: a mantra is a sound sequence used as an action, sometimes derived from and homophones to a word or sequence of words.

Staal finds mantras interesting for two reasons. First, mantras play a vital role in Vedic ritual. There is a general rule (paribhāṣā) that, ceteris paribus, every physical action is accompanied by a mantra (Bhāradvāja Śrauta Sūtra 1.1.20, Āpastamba Śrauta Sūtra 24.1.38; see also Chakrabarti 1980, 156-160). One might think that this is an indication that mantras are meaningful, since the mantra might be taken as a gloss on the action performed. But the problem is not this simple. Consider an example taken more or less at random. When the two hundred and fourth brick is consecrated in the Agnicayana, the accompanying mantra is "may sky, seed discharging, give me seed; may my seed procreate" (Baudhāyana Śrauta Sūtra 10.46: 46.19-47.1) (Staal 1983, 505). This would seem to be a reasonable wish, to be able to have children, and it seems a reasonable reason for performing a religious rite. It would appear at first sight that the purpose of laying this brick is to facilitate fathering a son.
But, Staal would argue, this is not the correct interpretation. For one thing, the yajamana may have plenty of children, and he might not wish any more offspring. In any case, the purpose of the Agnicayana (whatever that might be) may turn out to be quite different from the purpose of this one rite, and it would be odd for such a small element of a ritual to have such a major purpose, which could be at odds with the rest of the ceremony.

The second reason mantras are interesting, therefore, is that they present in especially clear relief the problem of ritual meaninglessness. Words are meaningful, and mantras that are homophonous with words thus also appear meaningful. But Staal's position is that the process of turning a word into a ritual element necessarily strips that word of its linguistic properties, such as meaning, and leaves it with just the acoustic properties that it possesses as a sound. An interesting corollary is that there sometimes is a semantic story behind the choice of a particular mantra, and even when this is not the case people will sometimes manufacture a connection between any given ritual action and the meaning of the verse used as a source for the corresponding mantra. Thus it is not uncommon for people to attribute meanings to mantras; sometimes these interpretations will even achieve wide-spread acceptance. But the situation here is the same as for any other ritual element: a ritual action cannot be meaningful per se, even if it is a sound production homophonous to a linguistic utterance.

The relationship between a mantra and a Vedic verse needs further clarification, however. A mantra can make use of any sound that the human vocal apparatus is capable of producing. That said, many Vedic mantras derive from verses in the Vedic sanāhitā corpus, most often the Rg Veda Sanhitā (though they usually are repeated, in different form, in the Sāma Veda). I will not discuss the details of these texts or how they are related. Details can be found in Staal 1986 and in many other works on the Vedic texts. The substance of Staal's argument, however, can be summarized as follows (chart adapted from Staal 1986, 64):
What this illustrates is how one verse from the *Rg Veda* (9.1.1) is transformed into different mantras by means of a variety of modifications to its musical properties (syllabification, accentuation, pitch, etc.) and then embedded in the Agnicayana at particular points in the action. Each stage corresponds to a line in the chart: verse (line 1), mantras (line 2), and action that is performed while the mantra is recited (line 3). The modification of the verse (so as to turn it into mantras) is done by means of formal, phonological rules that correspond in their basic forms to the syntactic rules that structure both ritual and natural language. These mantras are then embedded in ritual structures according to the syntactic rules that structure ritual sequences. It is in the move from line 1 to line 2 that the sound sequences lose their meaning.

In addition to these processes of transforming language into mantras, some mantric elements are sonic but non-linguistic from the very beginning. The syllable *om* is perhaps the best known example, but there are many others. In the case of Vedic mantras, such sounds take the form of syllables that are embedded in Vedic verses in the process of transforming them into mantras; the technical term for such syllables is "stobha". Other terms are used in other South Asian contexts, however (for example, "bija" and "dhāraṇī"), and one way of defining "mantra" is conjunctively by means of these more specialized words; this is the approach followed by Alper 1989, 3-5. After the addition of non-linguistic syllables and the modification of formerly linguistic ones, many Vedic mantras bear little resemblance to natural language. If we did not have the extensive documentation of the Vedic sanhītās, we would probably be unable to distinguish non- and formerly linguistic sounds in
a great many cases. Thus, the Vedic corpus provides excellent documentation for the view of mantras proposed by Staal.

To sum up: from Staal's perspective, mantras are in no way privileged elements of rituals, though they do perhaps illustrate better than ritual actions the nature of ritualization. My own intuition is that there is more that can be said about mantras, but this is neither the time nor the place to pursue these intuitions. Let this therefore suffice on the subject of mantras.

1 Though Staal does not use this phrase, it is constructed from language he prefers (see Staal 1989a, 97).
2 Setting aside, as I have been doing throughout, the fact that rituals can be constructed of actions performed simultaneously, often in different modalities (action, speech, music, etc.). Sometimes multiple actions will be conducted by different officiants simultaneously, and a complete ritual analysis will have to take a second or third dimension of structure into account. For the present, however, it is hard enough to determine an account of simpler, one-dimensional rituals, so these will be the focus of my discussion in what follows.
3 This is true with regard to surface structure. Natural languages actually tend to show a lot of center-embedding at deeper levels.
4 Minkowski 1989, 401-402, discusses Witzel's theory in some detail, but he says that he originally developed the theory on his own, and modified his paper to include discussion of Witzel's research after it became known to him.
5 Witzel has devoted considerable attention to the mutual interrelations between the various Vedic texts and schools, considering both chronological developments and also the geographical spread of different schools at any given time. Though this research is ongoing, good summaries may be found in "Vedic Hinduism", which he wrote with Stephanie Jamison (1992) (as yet unpublished, but currently available online) and in "The Development of the Vedic Canon and its Schools: The Social and Political Milieu" (1997). In brief, Jamison and Witzel 1992, 5, divide the Vedic literature into five layers: 1. the family books of the Rg Veda, followed by the later books I and X, 2. the Atharva Veda, Rg Veda Khila, Sāma Veda, and mantras of the Yajur Veda, 3. expository prose of the Yajur Veda samhitās, 4. brāhmaṇa prose, and 5. late Vedic sūtras. For my purposes, the significant difference is between early Vedic verse (Rg Veda) and the middle-to-late periods (#3 - 4 in Jamison and Witzel's scheme).
6 Though I will not consider this further, Witzel also provides a careful analysis of the story of Cyavana in the Śatapatha Brāhmaṇa. In this context, he makes some comments, not altogether favorable, about Wendy Doniger's treatment of these passages.
7 Witzel suggests that this process might be similar to Hacker's inclusivism, which may be defined as follows: "claiming for, and thus including in, one's own religion what really belongs to an alien sect" (Halbfass, ed., 1995, 244). Witzel does not propose a name for the process.
8 Traditionally the śloka is written as a two line stanza, in which case a octosyllabic pāda is actually a half-line. I should add that Minkowski's exposition could be better here. The first four syllables of the pāda of a śloka are anceps (of indifferent length), so one would expect the formulaic language to occur mostly in the latter half of each pāda, where the meter is, though not dictated by the meter, at least regulated to some extent: the śloka is a rather free meter, and the metrical scheme even at the end of the pāda can only be defined in statistical terms, tending towards an iambic meter in even pādas and towards a first epitrite or an antispast (i.e., short-long-long-anceps) in odd pādas. (The last syllable is
also free in each pāda, so really only three syllables out of eight are regulated to any substantial degree, analogously to the verse end anceps in Greek verse.)

10 Translating ἐλλοχὲς ἐπιστολάκες οἷς διαλυμένα τὴν ποίησιν. Minkowski does not discuss Aristotle, but the Poetics has been a major source for Western literary criticism, and I cite it here as a locus classicus to explain Minkowski's use of the word "episode", which clearly has particular force as a technical term of literary criticism.
11 I assume that Aristotle sees the Telemachia as distinct from the Nostos, and that he does not see two components similarly in the Iliad. But I might be misunderstanding the force of this comment.
12 To clarify: I have already compared Staal's use of "embedding" with Minkowski's. Here I focus on Staal's treatment of the phenomenon for which Minkowski adopts the term "episode", or "digressive rite".
13 Small letters denote rites that form part of the ritual named with the corresponding capital letter. Thus, the agniṣṭoma is denoted A, and parts of it that are not also parts of the paśubandha are denoted a-
15 I do not mean this in the grammatical sense of imperative, as a particular form in Sanskrit.
Sometimes stories are linked by imperatives (e.g., ṣṛpu, page 410), but in fact a wide variety of forms is used. Rather, I have in mind the variety of grammatical forms that Minkowski identifies in his discussion of the "diction of linking passages" (407-411).
16 Although I will not pursue the topic at this point, this is one area in which speech act theory, which has been applied to the Vedic ritual tradition in other contexts --- notably by Wade Wheelock 1980 and 1985 --- might be especially appropriate.
17 This is the Adhvaryu in Ḫṣīṣ (see Kane 1941-1975, vol. 2, pt. 2, page 1057) and the Maitravaruna in the paśubandha (Kane, page 1110).
18 Some Vedic literature (notably the Rg Veda) is clearly older than the developed forms of Vedic ritual, and this oldest stratum clearly does not exhibit the discourse structures that Witzel and Minkowski describe.
19 I see strong similarities between the poems of the Rg Veda and those of Pindar, for example, which probably derive in part from a shared Indo-European poetic tradition. But this is getting far beyond the scope of the current study and would require very extensive research, in addition to presenting sophisticated methodological challenges.
20 Many of the sources for Śaivasiddhānta are untranslated, or even unpublished, and the material is difficult even for experts. Sanderson 1986 is my source for the following discussion, along with a Spring 2003 class on Tantra taught at the University of Pennsylvania by Dr. Isaacson.
21 To take one simple case: for sequences of up to m phonemes in a language with n distinct phonemes, the upper limit on the number of possible utterances (in other words, if every combination of phonemes is a well-formed utterance) is \( n + n^2 + n^3 + \ldots + n^m \). It should be noted that the value of such a sequence gets to be very large very quickly.
22 This is, of course, only a partial list. It was generated from the online Shakespeare Concordance at http://www.languid.org/cgi-bin/shakespeare. Line numbers were taken from the Riverside Shakespeare, since they are not given in the online edition.
23 A sentence is marked in its traditional script by a capital letter at the beginning and a period at the end. In linguistic terms, it is defined as the junction of a subject and predicate. A line of verse is defined in terms of its metrical structure.
24 Staal references physical theories in general, but the details of this example are my own.
25 What follows is misleading, in that I am treating subconscious processes as though they were conscious, but this is purely for the sake of ease of presentation. Nowhere in what follows do I make reference to intentional action.
26 In other words: the basic (model) sāmīdhēnī recitation contains eleven verses. In some contexts, thirteen, fifteen, seventeen, and twenty four verses occur instead of eleven, and these are treated (by
Staal and by the Vedic ritualists) as variations on the basic form of the eleven-verse recitation. The double arrow in (15) indicates just this: that the animal sacrifice to Vayu derives from the model sacrifice in that in the former only eleven verses are recited (this is represented by E), but in the latter twenty four are (G). B is an animal sacrifice in both cases, but it has different components in the two versions.

Note that, while my criticism of Staal hinges in part on a carelessness in the symbolism used in his rules, I will also be somewhat careless in what follows. But there is a difference. Rule (17) is clearly a rule type rather than a rule specific to the Vedic system. Shortly I will emphasize this difference. Consequently (18), which relies on rule (17), is not quite properly formulated. But my goal here is not to formulate rules sufficient to analyze the Vedic ritual system. My position is that such an analysis is currently a desideratum, but it is a separate problem than the methodological one that I am concerned with here. I am concerned with reviewing Staal’s theory, and I am adapting my symbolism as much to his as I can, while diverging from it to point to its weaknesses. When my notation is careless, it is careless in the same way Staal’s is. But I also point out where it is careless, so hopefully no reader will be confused.

Please note that in what follows I ignore what linguists call "ordering constraints". It will become clear in a minute that the various g, s tend to all differ from one another in some regard (hence the different subscripts), and the grammar should account in some way for why one we find them in the order we actually get and not some other. This, it turns out, is very hard to do, and Staal’s theory is incomplete here just where most theories of linguistic syntax also have trouble.

In other words, a noun phrase may be composed of an article followed by a noun ("the book"), an article followed by an adjective followed by a noun ("the brown book"), a bare noun ("Australia"), and in other ways as well.

The abbreviations to be used in what follows are: I for invitational prayer, Y for yājya, R for āśrāṇa, P for pratyāśrāṇa, and T for tyāga.

Note that the structure in (20) exhibits a pattern that is not encompassed by these rules: Y’s occur at the beginning and end of the sequence, but IY’s occur medially.

See Minkowski 1989, 413-416 for references to the Vedic sources on this ritual.

This could be done in at least two ways. The easiest division of Vedic rituals into classes is into Havirya-jñas, Somaya-jñas, and perhaps Pasuya-jñas. The next division would set thirteen rituals in the first category rather than three (see the list given by Chakrabarti 1980, 136).

See the ms. paper "Error, Change, and Rule-Following", delivered at the 2002 meeting of the American Academy of Religion in Toronto. My main authority in this paper was Fuller 1984, 1993, and 1997.

Clearly the actions constitutive of simple rituals can simply and reliably be memorized. So it is only in the case of highly complex rituals, where the memory of practitioners would be severely challenged, that we can be sure ritual structures are not simply memorized but rather generated (to some extent) anew during ritual practice.

Staal does not provide any references for this claim. One place where such rites are mentioned is Śāṅkhāyana Śrauta Sūtra 13.28.8.

By "complexity" I mean the number of specifically prescribed actions at any point in a ritual.
Chapter 4

The notion of rule is key to Staal's theory. Rules constitute the primary structures that allow for the syntactic patterning of ritual, and it is the rule-governed nature of ritual that accounts for its alleged meaninglessness. Staal is aware of this, and in attempting to clarify the significance of rules for his theory, he considers in some detail all the usual suspects, all the trends in previous scholarship that bear on the role of rule-governed action in language --- and, according to his language analogy, in ritual as well.

In my treatment of Staal's theory of ritual, then, some consideration of rules and rule-governed action is necessary. The problems I will touch on are sometimes vast, and all I will attempt to do here is point in the direction of problems and their possible solution. But treatment of some points is essential, in part because Staal's treatment sidesteps the most important issues, even while he systematically approaches them in places. Rules Without Meaning, which seems intended to be a thorough discussion of Staal's theory, suffers from several failings. First, the central chapters are largely reprintings of earlier articles, without sufficient effort at reformulating the argument in a more consistent fashion. And many of the methodological problems are discussed in the opening chapters, before Staal's theory has been presented. This approach tends either to scare off students of ritual studies, who are looking for ritual theory, or else encourages the reader to skip right to the meat of the second and third parts. I have decided to follow the opposite course, to withhold discussion of more general concerns until after my discussion of the theory, in the hope that readers will now be eager to think about the intellectual context of the theory.

Staal's treatment of the problem of rule-following is primarily historical. He considers four stages in scholarly thought on the problem: the Indian tradition (Panini and the Prātiṣākyas), nineteenth century linguistics (especially Whitney), Chomsky, and Wittgenstein. I wish instead to provide a more problem-oriented approach. First I will consider general problems relating to rule-following, with a focus on the critique attributed to Wittgenstein; then I will return to the problem of linguistic
generalizations considered in Chapter 3, in the light of the treatment of natural laws by recent
philosophy of science; and finally, I will look at the reasons for thinking that ritual systems are
structured by rules, using the Vedic ritual system as an extended example. I therefore will cover much
of the ground that Staal does, but from a different direction. The discussion that follows should provide
a useful adjunct to Chapter 6 of Rules Without Meaning.

4.1. Rule-Following in General. Among philosophers and some linguists, the topic of rule-
following is primarily interesting because of what I will call the problem of "Wittgensteinian sceptical
doubts." Rather than attempting to solve this problem, I will aim in the early part of this chapter for a
clear explanation of whence the problem comes. Staal attempts to do this in Chapter 6B of Rules
Without Meaning, but his exposition falls short of clarity. I also will tentatively point to some reasons
why details of the topic might not be worthy of further consideration at all. Most generally, it is not
clear what the epistemic status of rules is, or the ontological status of the mechanisms they require.
Therefore, the problem of rule-following in linguistics, and in ritual, touches on larger questions in the
philosophy of science. I consider these two topics in turn in this section. Please note that while
sections 4.2 and 4.3 are important in linking the argument of Chapter 3 with that of Chapter 5, but the
current section will mostly be of interest to readers already familiar with the debate over
Wittgensteinian scepticism about rules.

4.1.1. Wittgensteinian Scepticism. The source of much debate and a voluminous literature
on rule-following is Saul Kripke's brief 1982 monograph Wittgenstein on Rules and Private Language.
In that volume, Kripke presented an argument (or rather a set of arguments) that he found contained in
Wittgenstein's major posthumous publication, the Philosophical Investigations (Wittgenstein 1958a).
The argument is not explicit in Wittgenstein's work, and it is not fully consistent with Kripke's own
philosophical orientation. Kripke 1982 thus appears to be a contribution to the history of philosophy,
but for Kripke's caveat that "the present paper [sic] should be thought of as expounding neither
'Wittgenstein's argument' nor 'Kripke's'" (5). In short, it seems possible that there is no one, and never
has been, who is persuaded by the arguments on which subsequent debate has built. And because of
this, it has been hard to identify what exactly the argument is. Wittgenstein throughout his career

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rejected the standard traditions of philosophical argumentation, and even Kripke acknowledges that "to attempt to present Wittgenstein's argument precisely is to some extent to falsify it" (5). Clearly it is harder to attack a moving target than a fixed one, and therefore the unclarity of the problem, combined with the philosophical brilliance of both Wittgenstein and Kripke, has made the problem very hard to get a handle on.

A second reason for the persistence of this debate is the substantial attempt at refutation of Wittgensteinian scepticism by Noam Chomsky (1986, 221-275). Kripke had indicated in several places that Wittgenstein's view of rule-following might present substantial problems for the approach to language that was advocated in contemporary linguistics, and Chomsky felt the need to tackle this criticism head-on in a semi-popular work that has been perhaps his most oft-read book. Both Kripke's and Chomsky's arguments are concisely and clearly presented, and I do not feel the need to present them in detail. Instead, I will limit myself to identifying some of the sources for this debate, and presenting reasons why I do not think the subject merits sustained treatment.

As will become clear shortly, my position is that the Kripkean sceptical approach need not concern us, however we may choose to formulate it, and consequently a detailed discussion of possible means of formulating it would be counterproductive in a discussion of ritual rules. But it would be odd to refute a theory without saying anything about its content, so let a few words suffice here. The prototype of Kripkean sceptical argument focuses on arithmetic sequences. It would appear that an arithmetic sequence could be clearly identified by a representation like the following: \([2, 4, 6, 8, \ldots]\). The ellipsis indicates that the sequence extends infinitely, and the numbers in the sequence that are listed are typically viewed as sufficient to identify the rule from which the complete sequence is derived. In this case, it may seem quite clear that the sequence \([2, 4, 6, 8, \ldots]\) is derived from the general rule:

\[ n_{x+1} = n_x + 2 \quad (1) \]

Yet Wittgenstein seems to question this. Just as there is an arithmetic sequence \([2, 4, 6, 8, \ldots, 1000, 1002, 1004, \ldots]\), so there is also an arithmetic sequence \([2, 4, 6, 8, \ldots, 1000, 1004, 1008, \ldots]\),
and the opening segments of both sequences are identical. How can we ever be certain, Wittgenstein asks, which sequence (and more importantly, which underlying rule) is determined by the representation \[2, 4, 6, 8, \ldots\]?

There are, of course, a number of ways to understand this basic problem, and I have attempted to state the problem in such a way as to not privilege one approach over the others. In what follows, my discussion will lead towards a sort of "Wittgensteinian" solution to this Wittgensteinian-Kripkean problem. What I mean is that I will attempt to point to a way out of the problem, without solving it. If we posit a given rule, which we understand, then we are immediately clear on what its product (output) will be; we only get into trouble when we begin with the product and try to work back to the underlying rule. If the rule is empirical (i.e., the sequence represents measures of quantities), then filling in the ellipsis becomes prediction. And in such a case we already know that theory is underdetermined by evidence. In any case, the problem is not one of understanding rules; rather, we are faced with the problem of performing successful induction. This solution is "Wittgensteinian" in that it resolves the stated problem without solving it, by simply changing the terms of the argument in such a way that the problem dissolves of its own accord. (This is roughly the approach that Chomsky takes in responding to Kripke.)

Note that according to Chomsky's view of language, the brain itself follows an inductive method during the process of language acquisition. The infant's brain is structured at birth in a way that facilitates language learning, and as a result the infant never has to consider language structures analogous to \[2, 4, 6, 8, \ldots, 1000, 1004, 1008, \ldots\]; \(^1\) rather, it immediately assumes that rule (1) applies, and consequently that the sequence \[2, 4, 6, 8, \ldots, 1000, 1002, 1004, \ldots\] is the natural expansion (induction) of \[2, 4, 6, 8\]. In other words, the infant is naturally inclined to accept rules like (1) that do not result in sharp discontinuities at some point in the output. The process followed by the linguist in trying to get to the rules that are hard- and soft-wired into the brain by observation of the output of speech is similar but different: the linguist has to work back to the initial state with which the infant is provided thanks to genetics and developmental processes (to determine the properties of the initial state of the language device), as well as work back to the language-specific features of particular
languages. Consequently, a Wittgensteinian linguist (i.e., one who focuses too much on the problem of induction) would err by failing to understand that the infant’s options for induction range over a finite set of rule types.

4.1.2. Wittgenstein: Style and Methods. I have mentioned the two direct sources for the debate over rule-following, namely Kripke and Chomsky. Let me now return to Wittgenstein himself. Though one of the most influential philosophers of the twentieth century, he published very little during his lifetime: his dissertation (the Tractatus Logico-Philosophicus), a few minor pieces in journals, and a textbook for German elementary schools. Yet he left substantial quantities of unpublished materials, which are now known collectively as the Nachlass, and entrusted their publication to three friends of his\(^2\). Perhaps the only philosopher to exert greater influence while publishing less than Wittgenstein was Socrates; perhaps the only modern author to publish more books posthumously than Wittgenstein was L. Ron Hubbard.

In any case, the textual problems relating to Wittgenstein 1958a and other posthumous books are substantial: the editors have often taken great liberties in selecting and organizing manuscripts and presenting them as finished publications, and they have given far too little information about their editorial methods or the sources of their publications. Stern 1992 and 1996 has presented two exceptionally useful overviews of the history of the corpus, and Pichler 1994 has gone back to the Nachlass and identified in detail the manuscript sources for each of Wittgenstein's published works. A microfilm copy of the Wittgenstein manuscripts was made by the Cornell library and sold subsequently to other research libraries; in 2000, Oxford University Press, in conjunction with IntelLex Corporation and the Wittgenstein archives at the University of Bergen, completed electronic publication in four volumes of all the Nachlass, in both facsimile and searchable digital format, based on the catalogue contained in von Wright 1982. A few manuscripts only became known after the Cornell microfilms were made in 1967, and some were intentionally omitted. Thus the materials are available for a thorough scholarly understanding of Wittgenstein's thoughts on rule-following, but the history of the Nachlass is such that most philosophers have based their understanding of Wittgenstein's legacy largely on the basis of the published text of the Philosophical Investigations --- and perhaps a few other
publications like the *Blue and Brown Books* (Wittgenstein 1958b), which was circulated in manuscript during Wittgenstein's lifetime.

Text-critical problems are, in short, one source of confusion regarding Wittgenstein's philosophy. Another is the manner of expression which he gave to his writings. Kripke's statement that "to attempt to present Wittgenstein's argument precisely is to some extent to falsify it" is a reflection of this. The *Philosophical Investigations* and other "late" writings are written in the form of "remarks" (*Bemerkungen*), short units of anywhere from a couple of sentences to a page or two in length. The major segments of the *Philosophical Investigations* range over roughly eight hundred remarks. Most of the remarks in a given segment treat the same topic(s), yet each is a self-contained unit. This means that there is no clear, over-arching structure to the work as a whole, and no formal argument or thesis in need of proof. Many scholars follow the same approach as Kripke and attempt to extract formal arguments from Wittgenstein's remarks, yet clearly to do so is a violation of Wittgenstein's basic attitude towards philosophy. This means that "Wittgensteinian", when applied to philosophical theories, is nearly always ambiguous. On the one hand it implies that Wittgenstein did or would have assented to those theories, or that the theory is an extension of a theory that Wittgenstein did hold to; yet any theory that can be expressed in the standard form of philosophical discourse is almost by necessity not something that Wittgenstein could have assented to. If a scholar calls himself a "Wittgensteinian" and presents novel theories inspired by the writings of Wittgenstein, then he is perhaps on firm ground. But in other contexts the adjective is almost certainly problematic.

Wittgenstein acknowledges that the remark format is essential to the expression of his doctrine, yet at the same time he recognizes that in a way this is a failure: "after several unsuccessful attempts to weld my thoughts together in such a whole, I realized that I should never succeed. The best that I could write would never be more than philosophical remarks; my thoughts were soon crippled if I tried to force them in any single direction against their natural inclination" (Wittgenstein 1958a, ix*). This implies that Wittgenstein recognized the likely fate of his legacy: if he was unable to weld his thoughts into a linear, deductive argument, it might have seemed unlikely that others would be able to do better. The history of Friedrich Waismann's unpublished book on Wittgenstein's thought (see Monk 1990, 284)
is a case in point: Wittgenstein never felt that Waismann was able to capture his thought properly, and eventually he "withdrew his co-operation" from the project, in Monk's words. Other evidence for his attitude toward his legacy can be intuited from his legal will, which treats his *Nachlass* as follows (quoted in Stern 1996, 454):

"I GIVE to Mr. R. Rhees G.E.M. Anscombe, and Professor G.H. von Wright of Trinity College, Cambridge. All the copyright in all my unpublished writings and also the manuscripts and typescripts thereof to dispose of as they think best but subject to any claim by anybody else to the custody of the manuscripts and typescripts".

After working for most of his productive life on these manuscripts, trying in vain to get them into a form that he was willing to publish, and regretting the early publication of the *Tractatus* (which expressed positions he later distanced himself from), he then left his manuscripts for posthumous publication with no instructions for his literary executors except to do what they thought best. Von Wright 1993, 501 even says that "it was, in fact, not until after his death that I learned that he had named me in his will as one of his literary executors". How could Wittgenstein expect von Wright to follow his own wishes if he never communicated them to him? Wittgenstein must have known that the publications of his *Nachlass* would not have met his stringent criteria, had he been alive. And a few years before he died he had many of his papers burned (von Wright 1993, 480), indicating that he was entirely unwilling to consign at least some of his later work to posterity.

4.1.3. Wittgenstein on Language. Thus the style of Wittgenstein's writings is closely tied to his conception of philosophy, and the divergence of his style and method from standard norms in Anglo-American philosophy makes the interpretation of his work especially difficult. If we can set aside all these hermeneutical dangers, however, can we reasonably expect a Wittgensteinian approach to be sympathetic to the general program of contemporary linguistics and cognitive science? I will argue that the assumptions underlying a Wittgensteinian approach to language differ so much from the assumptions at the root of modern linguistics and cognitive science that a Wittgensteinian scholar must first demonstrate the relevance of his approach to "scientific" approaches to language, and only once he
has done this should we attend to his specific criticisms. Otherwise, we will not know what to make of his criticisms --- in fact, this is precisely the fate that befell Kripke's sceptical arguments.

To get an idea of Wittgenstein's general understanding of language, let us look at three passages from the *Philosophical Investigations* (Wittgenstein 1958a). First, let us look at two cases where, on the surface, Wittgenstein appears to have a view of language that accords with contemporary linguistics. Chomsky assumes that humans are hardwired with many of the psychological structures necessary to process language, and that the process of learning the syntax of a language can be thought of as training an innate cognitive mechanism. In I, 496 Wittgenstein seems to anticipate this view (but see below): "... here I am looking at learning German as adjusting a mechanism to respond to a certain kind of influence". In I, 559 Wittgenstein uses the mechanism analogy differently. Here he says:

"One would like to speak of the function of a word in this sentence. As if the sentence were a mechanism in which the word had a particular function. But what does this function consist in? How does it come to light? For there isn't anything hidden --- don't we see the whole sentence? The function must come out in operating with the word. ((Meaning-body.))"

In some ways this remark is hard to follow. I think it is fair to assume that this is not a finished expression of Wittgenstein's meaning, and that further revision would have been advantageous. For example, the word "meaning-body" (Bedeutungskörper) only occurs here in the *Philosophical Investigations*, and the context does not help us much in understanding the meaning of this particular compound. However, I think we can conclude two things about this remark. First, the second sentence perhaps seems reminiscent of the processing model of language that we encountered in section 3.6. More precisely, it brings to mind my discussion of compositionality as a means of explaining the relation between form and meaning in language or ritual. We may think of the sentence as a mechanism for generating propositional meaning from the meaning of its component words. But what, Wittgenstein asks, is the function that gets us from sentence meaning to propositional meaning? Unlike in a Turing machine or a finite state automaton, the sentence is not generated by rule-governed operations --- and the meaning may not be determined by running those rules in reverse, by parsing the sentence down to its elements and their constituent structure. Rather, the sentence is seen as all there at

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once, as an indivisible unit that can be used for the sake of communication (in a "language game" or Sprachspiel, as Wittgenstein says elsewhere). It would appear, though the remark itself is far from transparent, that the word contributes to the meaning of the sentence in which it is embedded not compositionally, not as a result of the syntax of the sentence, but rather in use (pragmatically).

Most interpreters would at this point go on to talk about the use-theory of meaning that is found in the Philosophical Investigations, but for reasons I have given in the last section I will not follow this approach, namely one that tries to reconstruct a theory from Wittgenstein's remarks. Rather, I want to call attention to Wittgenstein's conception of language. It is, in many respects, a prescientific view. Certainly pragmatics plays a role in the meaning of any sentence as it is used in speech. But the fact that language has a pragmatic aspect tells us nothing about its syntax, or its phonology, or its history. Wittgenstein is interested in finding the right way of thinking about language, and for him there is an either/or choice between pragmatic and syntactic approaches. If context plays a role in meaning, then context is all we should attend to. The background to such a view of language is, of course, a concern with the basic problems of philosophy. The position of the mature Wittgenstein was that philosophical problems will be seen to dissipate once we view them rightly, and our use of language is the chief culprit responsible for philosophical confusion.

Thus, when he speaks of language, Wittgenstein is not thinking of the same object as the linguist. He is thinking, I believe, of particular sentences that are ambiguous in certain predictable ways. And the meaning he wishes to recover is not the full linguistic meaning of those sentences, but rather a particular component of the philosophical meaning of the sentence. Perhaps this was not doctrine for Wittgenstein. Perhaps his own thought slid from prototypical sentences of philosophical interest to sentences generally; the model sentences in the early remarks of the Philosophical Investigations do not limit themselves to Russellian statements about sense-data, for example. But it is important to keep in mind that Wittgenstein was not a linguist in the general sense.

This does not demonstrate that Wittgenstein's remarks are not relevant to linguistic inquiry, but only that his explicit concerns were of a more limited scope than the linguist's. Let us therefore look at a longer remark, namely I, 157. I will not quote the entire remark, as I did with I, 559. Wittgenstein
begins by asking us to consider "reading machines" as a general class, whether they be humans or other creatures or artifacts like the pianola. The topic he wishes to consider is the relation between performance and ability, and he asks us to consider a case in which the machine pronounces the sounds corresponding to the text that is to be read sometimes but not always ("here and there it happens 'accidentally' to be roughly right"). Gradually the apparent accuracy of reading increases. Wittgenstein then asks when it is that the machine can be said to be really reading. This is the sort of case that we find throughout the *Philosophical Investigations*: Wittgenstein wants us to make our judgments (in this case, a judgment of when reading occurs) from the outside, solely on the basis of observation.

I said earlier that in I, 496 Wittgenstein seems to anticipate a Chomskian perspective, but clearly we can only say that if we take (as I did earlier) short passages out of context from Wittgenstein's work. If we look at the general thrust of the arguments in entire remarks, we see an explicit rejection of a Cartesian preference for introspective evidence, and a willingness to judge even internal facts on the basis of publicly accessible data. Consider the following from *Remarks on Colour*: "People sometimes say (though mistakenly), 'Only I can know what I see'. But not: 'Only I can know whether I am colour-blind'. (Nor again 'Only I can know whether I see or am blind')." (Wittgenstein 1977, I, 83). The first of the three cases mentioned in this remark seems clear enough: people are mistaken when they say that they know (through introspective knowledge) what they see. This is used in the remark to support the next contention, which apparently implies that color vision works the same way: subjective experience is not sufficient to warrant the claim that one is, or is not, colorblind.

Remark I, 84 clarifies the third element of I, 83: assertions that one is sighted and that one is color-sighted "are not logically of the same sort", and such claims find support from different sorts of criteria.

Let us return to the reading machine. In the case of an artifactual machine, we would be warranted in saying "the machine read only after such-and-such had happened to it --- after such-and-such parts had been connected by wires". "In the case of the living reading-machine [however] 'reading' meant reacting to written signs in such-and-such ways. This concept was therefore quite independent of that of a mental or other mechanism"(Wittgenstein 1958a, I, 158). It seems clear that for Wittgenstein, reference to internal mechanisms is no more sufficient grounds than reference to internal introspective
evidence when we wish to answer a question like "is this machine actually reading?"; in other words, when we wish to answer questions relating to the performance of functions.

More generally, I wish to propose that Wittgenstein's commitment to a behaviorist psychology runs deep indeed, and that it seems unlikely that we can (as many are wont to do) simply reformulate arguments that we find in Wittgenstein's writings while shearing them of their behaviorism. I have not demonstrated that this is impossible, certainly, but I do believe that the burden of proof is on those presenting sceptical Wittgensteinian critiques of mechanistic models in cognitive science.

4.1.4. Staal on the Rule-Following Debate. Let me briefly summarize the argument of section 4.1 up to this point. Interest in rule-following as a subject for philosophical inquiry was jumpstarted by Kripke 1982. In this work he alluded in several places to a potential problem at the foundation of modern linguistics; this challenge was taken up at some length in Chomsky 1986, but this did not settle the matter and the result of Chomsky's intervention was that the debate picked up energy. I have studiously avoided a direct treatment of the content of the Kripke-Chomsky debate precisely because this would require a digression far larger than I think it merits, at least in the context of understanding Staal's theory. On the other hand, to not mention the debate at all would seem odd to anyone familiar with it; as Staal 1989a, 52-60 has done, therefore, I have chosen to consider the debate, but not to provide a thorough treatment of it. But the treatment I have provided differed from that given by Staal.

I have focused on reasons for scepticism regarding Kripke's sceptical reading of Wittgenstein, and these reasons have been divided into two groups. First, I gave reasons for scepticism regarding any interpretation of Wittgenstein, some text-critical and some relating to the nature of Wittgenstein's style and method. Second, I looked briefly at some passages that are not typically referred to in the rule-following debate, to demonstrate that Wittgenstein's writings might be a poor place to look for evidence relevant to linguistic (or ritual) rule-following. I did so in a way that is, I hope, in keeping with my hermeneutical concerns as expressed in section 4.1.2. This is far from a refutation of the Kripkean reading of Wittgenstein, but it does go some way towards shifting the burden of proof onto those who wish to present Kripkean arguments.

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Let me now say a few words about Staal's treatment of this topic. Then I will move on to larger questions in the philosophy of science. Staal says this of Wittgenstein 1989a, 55: "Wittgenstein was sceptical about the possibility and the precise meaning of 'following a rule'. His account of these problems is meandering and confusing, but it starts out clearly." He then proceeds to give an outline of Wittgenstein's sceptical argument regarding arithmetic sequences. This is, more properly, the problem with which Kripke 1982 begins, so as for Kripke "Wittgenstein" means "Kripke's reading of Wittgenstein", so for Staal "Wittgenstein" means "Kripke's book on Wittgenstein". After saying that he does not wish to add to the literature on this subject, and without indicating how a debate over arithmetical sequences could be generalized, he moves on to a consideration of Chomsky's response. He provides a reasonable interpretation Chomsky's conclusions: Kripkean-Wittgensteinian sceptical doubts do not warrant scepticism concerning the fact that certain acts are rule-governed.

Staal then goes on to make a more general point, namely that scientific theories are by nature provisional. Thus we might be sceptical whether a given action is in accordance with one particular rule, or rather in accordance with another, similar rule with which it might readily be mistaken. If the Wittgensteinian critique is simply intended to show that we cannot have absolute certainty in the subsumption of particular actions by particular rules, then clearly that critique is unobjectionable and uninteresting. I am far from certain that this is the best way to interpret the Kripke-Wittgenstein position. But setting that aside, I think it is important to consider the question of linguistic (and ritual) rule-following in the context of more general debates over the proper scope and form of scientific theories. It is to this that I now turn; I will return to this general topic again in Chapter 5, after considering Vedic topics in section 4.3.

4.2. Generalizations and Social Facts. As Staal points out, a theory of language must be able to make generalizations about language use and structure. "Rules are important and interesting because they express regularities" (Staal 1989a, 58), so a study of linguistic rules is really a study of the regularities that underlie our use of language. Not all generalizations are equally useful, admittedly, and not all generalizations are equally important to us. At a first pass, let me categorize generalizations as falling into three types: contingent generalizations, ontological generalizations, governing
generalizations. The following discussion will be somewhat lengthy, but I think it is necessary in order to clarify the significance of rules for a theory like Staal's. In addition, this section will provide a slow and somewhat intuitive introduction to problems that will be more thoroughly discussed in Chapter 5.

An example of a contingent generalization would be *all the roses in this garden are red*. In other words, the generalization might help guide our actions in the future, for example if we were in search of red roses to cut for a bouquet and wanted to know where to look. But such generalizations do not give us much general knowledge about roses. This generalization tells us that some roses are red (assuming we also know there are at least some roses in the garden), but it does not tell us that all roses are red. Ontological generalizations provide more general knowledge than corresponding contingent generalizations: *all roses are flowers*, for example, lets us infer statements that generalize to all roses (*all roses are plants*), whereas contingent generalizations do not, absent further information.

When we consider knowledge of the natural world, there tend to be ontological generalizations that we treat as forming the basis of scientific knowledge. *All roses are flowers* tells us something about the essence of roses. It tells us something about what it is to be a rose. *All the roses in this garden are red* tells us something about some roses, but it tells us little about what it is to be a rose. But the distinction between these two categories is far from absolute. Let us say that all roses in the world right now are red: does this warrant us in saying that all roses in the future will be red, or that no non-red flower can be a rose? Not necessarily. Perhaps someone burned all the non-red roses in a massive fire, but forgot to burn all the seeds of non-red roses. Biologists, in fact, encounter problems like this all the time, since the possible, morphological variations between individual animals are almost unconstrained, and yet animals do seem to group fairly neatly into distinct species.

In the best cases in the natural sciences, ontological generalizations succeed in capturing principles that structure the physical world. *All things fall*, for example, is the evidence for the existence of gravity, a force that we are not able to perceive directly with any of our senses. Apparent exceptions (nothing falls in a void, thoughts about summer vacation do not fall) can be resolved by a proper clarification what we mean by "fall" and "thing". And many generalizations, even if their exact status is unclear, can still be very helpful in guiding our actions (*sharks eat people*).
One major culprit responsible for eliding the boundary between these two types of generalization is the fact that it is often not clear when a statement beginning with "all" is of truly universal scope. A limiting phrase like "in this garden" clearly limits the scope of the "all" to a local area, and hence to a finite number of roses, but it need not be the case that universally valid generalizations will apply to an infinite number of cases. Even without that limiting phrase "all roses" still refers, at any time, to a finite number of objects. And in all of time there will be, probably, only a finite number of roses. As my choice of terminology should make clear, ontological generalizations tend to give us information about essences or ontological classes, whereas contingent generalizations tend not to. But the situation is not as clear as we would like it to be, especially since we do not get ontological classes without committing to a prior metaphysics.

When we move from the natural sciences to the social and human sciences, the situation is roughly the same. It appears to be very difficult to find what philosophers call strictly deductive-nomological (D-N) generalizations in sociology or history, for example, and those we do have tend not to be very informative. Forms of social life tend to be conditioned by human nature, but they also are conditioned by choices that people make and by contingent environmental factors. The traditional explanation has been that in principle D-N generalizations are possible in the social and human sciences, but due to the complexities of human social life we rarely if ever see clear instances of particular generalizations.

It is in this context that governing generalizations come into play. There might not be anything in the world that necessitates that marriage be a bond between one man and one woman, but in most places in this country it is a true generalization that all American marriages are bonds between one man and one woman. After the Mormons outlawed polygamy and before states began to recognize gay marriages, the generalization might have been universally true. This example differs from previous examples because it is very nearly a logical necessity that it be true. Ontological generalizations are very nearly physical necessities, at least in prototypical cases, and of course contingencies are not necessary at all. All American marriages are bonds between one man and one woman is similar to all bachelors are unmarried: in the latter case, the meaning of the predicate follows from the meaning of
the subject. But we are not thinking of the former as a sentence, and asking why the sentence is true. Rather, we are thinking of it as a fact, and asking why the fact is true. *All bachelors are unmarried* is not true just because it is a fact that all bachelors are unmarried, as it might be the case that *all Quakers football players are unmarried* because of a contingent fact about football players at the University of Pennsylvania. *All bachelors are unmarried* is true for a stronger reason: any married male cannot be called a bachelor. "Bachelor" means "unmarried male", so "All bachelors are unmarried" is synonymous with "all unmarried males are unmarried".

The best explanation, then, for the fact that all bachelors are unmarried is that there is something more than just a contingent fact. We do not need to take a census to determine the fact of the matter. All we need is a dictionary, and then we can assert not only that all bachelors are unmarried, but also that all bachelors will always be unmarried. The fact that all Quakers football players are unmarried, in contrast, is only contingently true, and thought it is (let us assume) true this season, it may well be false next season.

The fact that all American marriages are between a man and a woman is not just a contingent fact, but the best explanation for its being true has nothing to do with definitions of words. Instead, marriage is a social fact. The state of being married (American-style) is constituted by being a bond between a man and a woman. This last statement is phrased very much like the case of analytical truth, but it points to a fact about the world, like the case of ontological truth. The claim that all American marriages are between a man and a woman cannot be verified in the same manner as the claim about Quaker football players. The latter is satisfied in a straightforward way: pick any player you like; for that player it will be found to be true, when his personal circumstances are examined, that he is unmarried.

The reason why social facts create a space for generalizations unlike analytic or contingent truths has to do with the way that they are constituted. There are ways of being married, and anyone who undergoes the proper treatment is thus married. There could be a rule according to which no one may marry more than once. In this case, subsequent actions of marrying would simply be impossible, even if one underwent rites that, to the external observer, seemed identical to true acts of marrying.
But American marriage is not constituted in this fashion, so polygamy is possible. There might be a rule that polygamy is not allowed, but this only obliges punishment for polygamists. It does not prohibit the possibility of polygamy. We would then say that no rule prohibiting polygamy is constitutive of American marriage. More generally, social facts are constituted not by individual facts in collection (as in the case of Quaker footballers) nor by something external to the social fact itself (the idea of American marriage, for example). They are facts in their own right, which supervene on individual facts in collection without reducing to them. In fact, the identity of individual facts might be dependent on social facts, even as those individual facts help to contribute to the facticity of the social fact itself. A satisfactory account of social facts is not, at present, available, but my brief discussion here should indicate some of the basic contours that such an account should exhibit.

4.2.1. Generalizations and Rules. Let me briefly summarize the key points of this discussion thus far. Our view of the natural sciences presumes that scientific generalizations tell us something about how the world actually is; the sciences are useful for their predictive power, certainly, but most people (instrumentalists aside) would argue that successful scientific theories can be taken as support for certain metaphysical claims over against other such claims. It is, in practice, quite difficult to make principled distinctions between generalizations that lend support to scientific claims and those that do not (in other words, between contingent and ontological generalizations), but the effort seems worthwhile nonetheless. Furthermore, there seems to be good evidence that social facts deserve a somewhat different manner of treatment than physical facts. Social facts appear to be dependent on physical facts in a way that physical facts depend on nothing else, yet it seems a mistake to assume that social facts can be reduced purely to physical facts: social facts maintain a degree of facticity all their own. We may conveniently label this view of the social as presenting us with a “Durkheimian” picture. And further, we may ask how a Durkheimian view of ritual can be reconciled with a cognitive theory of the type that Staal presents.

Computational rules of ritual govern the physical processes of the mind-brain that allow for ritual action, but they do not seem to be reduced to physical predicates. In this regard they appear similar to social generalizations. Will it turn out to be the case that social generalizations, while they
cannot be reduced to physical predicates, can be reduced to computational or other types of psychological predicates? If this is the case, it would allow us to simplify our overall ontology by reducing the total number of irreducible objects that we have to posit. If it is not the case, then we will find that an increasingly large portion of our ontology is taken up by objects that supervene on the physical.

Compare my treatment of marriages in the last section with my discussion of ritual categories in section 3.4.3. Clearly the former cannot be reduced to the latter, because a marriage is different than the rite that brings it into being, so a general theory of marriage will never fall under a general theory of ritual. But if all ritual classes can be found to be items over which syntactic rules operate, then perhaps we can generalize and argue that all irreducible social generalizations can be found instantiated in individual minds. The last century of social theory indicates that this is probably not the case, because some social phenomena, for example the behavior of mobs, cannot be accounting for in terms of individual psychology. A mob might act as though it is acting to achieve some goal, even though no individual within the mob can be found to want the end towards which the mob is aiming. Thus we may speak of a “mob psychology” different from the psychology of any individual. It thus would appear that our ontology is bound to contain a fairly large number of non-physical, supervenient objects, some computational and others irreducibly social. Nevertheless, it would be a valuable exercise to try to identify any social facts that could be reduced to computational predicates in individual minds. The ritual categories considered in section 3.4.3, for example, may prove to be the same as categories presumed by strictly sociological analysis. As a concrete example, consider Humphrey and Laidlaw 1994’s analysis of Jain anga puja.

4.2.2. Rules, Constitutive and Generative. Anga puja is the element of Jain worship in which an idol representing the god is bathed, anointed, and decorated. When Humphrey and Laidlaw learned to perform worship in a Jain temple, they were told such things as “This is anga puja, you take sandalwood paste on your finger like this, and do this with it” (page 117). Adopting Searle’s analysis of speech acts to ritual, they consider imperatives of this form to be constitutive rules. I will consider in a minute what constitutive rules are, but for now let me consider the general approach that Humphrey
and Laidlaw follow. Their book is a detailed study of “the Jain rite of worship”, namely the Jain form of Indic puja. Hindus also practice puja, and puja can be considered one of the basic elements of religious worship throughout South Asia — this is why I have alluded to a generic, “Indic” form of puja as the general class of which the Jain rite of worship is a type. Hindu puja is similar to Jain puja, though of course there are differences in execution as well as interpretation. Humphrey and Laidlaw consider the differences by comparing the rites they observed to the rites observed at a major south Indian temple by C.J. Fuller (pages 122-126). The chief alleged difference has to do with whether there is a minimum number of actions that are necessary elements of puja, and whether there is a necessary order in which those elements must be performed. Fuller argues that Hindu puja has both necessary parts and necessary order, though he allows that in practice these necessary elements are not always found. Jain ritual is traditionally viewed as being more free than Hindu ritual, but Humphrey and Laidlaw argue that in fact puja is free in both traditions.

The significance of this debate for our purposes is that scholars of Indian religions speak freely of puja as a type of ritual (what I have called “Indic puja”), and Hindu or Jain puja as specific subtypes of this ritual. But, we might be inclined to ask, what is puja? We can be instrumentalists and answer that there need be no necessary or sufficient conditions for a ritual to be an instance of puja; the label is appropriate just to the extent that it is helpful. But we can also choose not to be instrumentalists, or to hold instrumentalism out as a last resort. Then we will need a theory that allows us to say what constitutes puja. Humphrey and Laidlaw argue that rituals like puja are created by what they call “constitutive rules”. In other words, the problem of whether we should create an ontology of rituals, with Jain puja as one element of our list of rituals, becomes a problem of whether there are rules that determine what counts as an instance of puja. And this problem is similar to the problem of whether we should create an ontology of marriages, including American marriage as an instance. Such problems are not problems of how to typologize, but rather problems of whether things that we speak of as being objects in their own right are really objects with their own claim to facticity: for example, should be take “the marriage failed” on analogy with “the deck on the back of the house failed”, where we take “fail” to mean the same thing in both cases, and not to be merely metaphorical in the former case?
If it can be established that rituals have a claim to being facts in their own right, then we can ask the further question how the sociological approach to ritual may be reconciled with a cognitive view of ritual. But first we should ask what the sociological approach entails, and this means considering what constitutive rules are. Since Humphrey and Laidlaw adopt the term from Searle, I will begin by considering the ways that Searle characterizes them. Constitutive rules are to be distinguished from regulative rules: “We might say that regulative rules regulate antecedently or independently existing forms of behavior . . . but constitutive rules do not merely regulate, they create or define new forms of behavior” (Searle 1969, 33). We can take this almost as a definition: the key feature of constitutive rules is that they create forms of behavior. It is a functional definition, certainly, since it tells us what constitutive rules are by telling us what they do. Since a functional characterization is perhaps not the best way to define something, Searle begins his discussion with a caveat: “I am fairly confident about the distinction [between constitutive and regulative rules], though I do not find it easy to clarify” (Searle 1969, 33).

Searle is concerned primarily with language, so for him the most important thing about these rules is that they allow certain sorts of speech. But, following Wittgenstein, he explains his idea with reference to games, and in a similar vein Humphrey and Laidlaw apply the idea to rituals as well. Twenty-two people could run around a field, but unless there were such things as the rules of football, it would be impossible for those twenty-two people to be playing football. Thus, “the rules of football or chess . . . create the very possibility of playing such games” (Searle 1969, 33). More generally, “constitutive rules have the form “X counts as Y” or “X counts as Y in context C”” (Searle 1969, 35). For example, applying sandalwood paste to an idol (in a Jain temple) counts as puja precisely because there is a rule that says “applying sandalwood paste to an idol [X] counts as puja [Y] in a Jain temple [C]”. It is clear how the particular is subsumed under the general rule, since the form of the rule is such as to state that subsumption occurs, but no reason is given why applying sandalwood counts as puja: in fact, this is precisely the point of the rule, so no further explanation is necessary (but see below). The rule is presented as the explanation for why the application of sandalwood counts as puja.
One might object that this is a little too convenient, that positing a constitutive rule is rather like defining a dormative property as “any property that causes one to sleep”. It seems as though no real explanation is going on, and constitutive rules are simply standing in for real explanations until they can be found. This is why I have introduced the notion of constitutive rule where I have: the Durkheimian framework in which I see much of social theory as operating would seem to provide a good explanation for why it is that constitutive rules serve the function they do. Under this interpretation, constitutive rules are simply the covering generalizations that derive from the fact that individual instances of religious ritual, or game playing, or language use, contribute to the independent existence of social facts that in turn play irreducible roles in social scientific explanations. Strictly speaking, as I am proposing, it is wrong to say (as I did a moment ago) that constitutive rules explain the subsumption of action under a social category; rather, we should say that constitutive rules are statements of such subsumption.

In any case, I have introduced this notion of constitutive rule, which has already been imported into the ritual studies literature, to present a model according to which social scientific explanation is accomplished by rules, without any reference to rules of language. We can think of the scientific observer as approaching ritual from one of two directions: down, from an observation of particular rituals; or up, from a particular computational model. Of course, the value of any computational analysis will be underwritten by the study of particular rituals, but once we follow Staal in arguing that all ritual has a common syntactic core, then we can take shortcuts and inductively derive a syntactic parse from a limited amount of observational evidence and then work upwards to generate a possible ritual sequence containing these observed elements.

If it is correct that all social generalizations about ritual can be reduced to purely psychological predicates, then it makes no difference in principle if we work entirely down from observation or rely in part on reasoning upward from cognitive structures. In such a case, then for all practical purposes a cognitive theory of ritual will count also a general theory of ritual. But it seems clear that cognitive theories can account for useful results even if this is not the case. Further, it seems fair to say that, to the extent that social generalizations can be explained in more basic psychological terms, to that extent
we can say that constitutive rules reduce to computational rules. To the extent that social
generalizations are independent of underlying psychological generalizations, they depend on the social
facts that underwrite relevant constitutive rules and not simply on the rules themselves.7

4.2.3. Summary. Having begun this chapter with a rather extensive digression, I have now
come back to rituals. In section 4.3 I will look at how Staal treats the rules underlying the Vedic ritual
system, but in that section and in Chapter 5 I will return also to topics that were considered in sections
4.1 - 4.2. Staal talks frequently about ritual rules, but he jumps from there to conclusions about the
scientific nature of the Vedic ritual system without providing the intermediate link, namely a general
discussion of rules. What I have provided thus far in this chapter is a compromise between a thorough
discussion of rules (which would be a very extensive digression if done properly) and the cursory
treatment that Staal provides. I have had to fill in gaps myself in cases where Staal does not provide
guidance, and if my readers wish to reject some of what I have said, then at least this brief treatment
may lay the foundation for further debate.

4.3. Rule Systems in Ritual Theory. Staal considers ritual rules primarily in two contexts:
his language analogy, and his studies of Indian science. I will consider the former here, and the latter in
Chapter 5. The two contexts are not unrelated, however, because Staal considers both linguistics and
ritual studies to be special sciences. My treatment of these topics, having begun in sections 4.1 - 4.2
with very general considerations, will therefore move from particular comparisons between language
and ritual (section 4.3) back to more general considerations in Chapter 5.

The inspiration for the language analogy is the similarity between the Vedic ritual texts and the
Sanskrit grammatical texts. The former provided the formal model for the latter, and the latter
perfected the sutra style found in the former (see sections 0.2.4 - 0.2.5 above). Staal assumes that both
types of text provide accurate descriptions of their respective subject matters, and consequently he
believes that this tells us something important about what ritual is and how it works. "There is nothing
in contemporary research that resembles or even approaches the scientific achievements of the Śrauta
Sūtras unless it is derived from these sūtras", so it seems reasonable for contemporary students of ritual
to look to those sūtras in developing their own theories. Staal therefore bases his syntactic theory of

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ritual on the analysis he finds in the Śrauta Sūtras, and he offers this approach as a model for other scholars as well. The similarities between the two types of sūtra text were sketched over a half century ago by Renou, as were the dependence of the grammatical sūtras on the ritual sūtras.

But for Staal's argument to be persuasive, more is necessary than just this genera-historical story. There need to be ritual rules that serve the same function in the construction of the Vedic ritual system as grammatical rules serve in the Sanskrit linguistic system, and ritual rules need to be as powerful as the corresponding linguistic rules. All of this requires careful attention to the textual evidence provided by the various sūtras, and by research into contemporary Vedic ritual traditions (the subfield sometimes called "Vedic fieldwork").

It is possible that the Sanskrit grammatical texts achieve a degree of scientific adequacy far exceeding that of the Vedic ritual texts. If this is the case, then Staal's position will probably be weakened, perhaps significantly, because the two-fold foundation of his reliance on the Śrauta Sūtras is formed by the similarity between ritual and linguistic sutras, and by the scientific adequacy of the latter.

The bulk of the current section will be devoted to a brief study of a type of rule called paribhāṣā in Sanskrit. I will consider the significance of these rules in both grammar and ritual, and look at the significance that Staal seems to attribute to ritual paribhāṣās, as well as putting this discussion in the context of my discussion of ritual in Chapter 3.

4.3.1. Review: Rules As Discussed in Chapter 3. Since the rest of this chapter will introduce several Vedic ritual rules, let me begin by briefly reviewing my earlier treatment of Vedic ritual, which I presented in Chapter 3. In section 3.4.1, I pointed out that Staal's ritual parses are only approximate. They are intended to illustrate the general form of the rules that he thinks structure Vedic ritual, and therefore it is better to consider his ritual rules as abstract models of actual rules. Furthermore, it appears that none of his parses terminate in what I have called "unitary actions", which we would expect to be the referents of terminal characters in a ritual parse. Staal tried to address this problem in response to criticism by Penner (see also section 0.3.5 above), but Penner did not really put his finger on the exact nature of the problem, and Staal erred likewise in allowing Penner to set the
terms of the debate. It is not a problem that Staal presents a simplified model of the phenomena he is considering (this is common practice in the natural sciences), but it is a problem that his parses would be incomplete in form even in the simplified cases that he analyses.

In section 3.4.3, I move from the symbols that seem to name individual actions to those that appear to be category symbols. Because of how Staal presents his argument, symbols that appear to be category symbols do not in fact label categories of action, but rather sequences of discrete actions. When a given unitary action occurs in multiple sequences, Staal's notation is unable to capture this fact. Taking a simple model of the standard Vedic iṣṭi rite (and building on section 3.2.4), I found several examples of rite-types that the sutra tradition itself treats as categories of ritual action; these, presumably, should replace Staal's pseudo-types, those elements to which he had given names like G and d₁.

My conclusion in Chapter 3, very generally, was that Staal's analysis was lacking at several levels. He professed to present actual, simplified analyses when instead he was presenting the forms of rules. And further, it was far from clear that these were the correct forms of rules for Vedic ritual. On the other hand, it did seem that a proper analysis could be derived for the Vedic ritual system, based on the general principles that Staal had laid out. I have suggested that the first step in developing such an analysis would be to derive a "distributional" description; approximations to this already exist for most Vedic rituals. The next step would be to develop a set of rules that would most efficiently generate these sequences. This would require the same sorts of skills used in current linguistics research, and while the analyses of the ancient Vedic scholars might provide inspiration and shortcuts, I am not convinced that Staal is correct in assuming that the Śrauta Sūtras provide the correct analyses in all cases. In any case, further study would prove my intuition to be either well-grounded or baseless.

4.3.2. Staal on Paribhāṣās. Staal has argued that the basic Vedic ritual rule types correspond to phrase structure rules and transformations (I discussed this in Chapter 1). In addition, he shows a recurrent interest in so-called "metarules", paribhāṣās in Sanskrit, but unfortunately he is never fully explicit as to why paribhāṣās are significant from a theoretical point of view. Perhaps the comparison
with the linguistic analysis of the Sanskrit grammatical texts is all that lies behind this interest: if paribhāṣās occur in the latter, then it must be significant that they also occur in the former. But this argument is really just deference to recognized authorities, a means of proof that is not highly prized in the sciences. It is, however, possible that Staal believes that the paribhāṣās represent the general principles that make ritual (singular) into an object of scientific study, i.e., that they capture the significant generalizations that structure the ritual system.

If the latter hypothesis is correct, then it would be worthwhile to look at paribhāṣās in more detail, as they are treated in both the ritual and grammatical texts. The word itself does not seem to be adequately explained etymologically (Chakrabarti 1980, 25) and is not consistently used with a single, clear sense in the sūtra texts themselves; this is perhaps not surprising, since we see a significant evolution, lasting several centuries, between the early ritual sūtras and the developed grammatical theory of Pāṇini. In any case, the word seems often to be synonymous with "nyāya" (Chakrabarti 1980, 26), and might have originally meant "discourse round the text" (Gonda 1977). The translation "metarule" implies that paribhāṣās are rules that govern the application of other rules. This seems to be the sense of the term as it is applied in the grammatical literature, and seems a sensible way of describing at least the bulk of rules traditionally recognized as paribhāṣās in the ritual sūtras. If we think of them as rules governing the application of other rules, then paribhāṣās form a distinct type of rule and clearly an important one for understanding the overall structure of the Vedic ritual system. It is not clear that the ritual paribhāṣās do in fact represent general rules in the sense that Staal seems to believe they do. When we try to compare them to the paribhāṣās found in Pāṇini, it appears likely that their role in the ritual texts can easily be overstated.

The following considerations are very provisional, but they seem sufficient to underwrite my strong reservations as to whether paribhāṣās are significant for understanding ritual structure at a general level. I begin with two examples that Staal (1989a, pp. 355-359) gives, both taken from the Āpastamba Śrauta Sūtra.
The first example considers Āpastamba Śrāuta Sūtra (ĀpŚŚ) 24.1.23-26, which presents, Staal assures us, "a striking parallel to the structure of Pāṇini's grammar" (translations are Staal's, with slight changes in punctuation):

(ĀpŚŚ 24.1.23) "When 'he makes an oblation' is enjoined, 'of ghee' should be understood."

(ĀpŚŚ 24.1.24) "As agent, the Adhvaryu (should be understood)."

(ĀpŚŚ 24.1.25) "As implement, the juhū."

(ĀpŚŚ 24.1.26) "When (the juhū is already) used, with the śruva."

Even with Staal's parenthetical suppletions, these rules might not be terribly clear, so let me briefly explain them. Rule (23) indicates that all oblations (ritual pourings of plant or animal matter) should be with ghee, or clarified butter, unless the text indicates otherwise for some particular rite. Thus, ghee is the "default" offering. Similarly, the adhvaryu is the default officiant in all ritual actions, and the juhū is the default ritual implement. Rule (26) is subsidiary to rule (25): when the juhū is already in use, the default implement for further specifications of ritual action becomes the śruva. We can understand these rules in one of two ways. The first way, which I will consider at some length, supports Staal's interest in paribhāṣās whereas the second, which I can treat much more briefly, does not.

This is the first way of understanding ĀpŚŚ 24.1.23-26: if Staal is correct, then ghee occupies a privileged place among oblations, and similarly for the adhvaryu among officiants, and the juhū among implements. We have seen that many core elements of the ists are oblations: āghāras, prayājas, ājyabhāgas, etc. Generally, in accord with ĀpŚŚ 24.1.23 and other similar rules in the Śrāuta Sūtras, these are offerings of ghee. But, if in some rite or a given variant on one of these rites, a different material is offered in place of ghee, then this can be viewed from a scientific perspective as a transformation of the general pattern of offering, and our formal analysis of the rite in question will indicate this clearly. This assumes, of course, that there are cases in which ghee is not offered. If ghee
were the only option, then a rule making ghee the default option would be uninteresting from the perspective of a science of ritual, though it may be useful for pedagogical purposes. Put differently, a rule must be limited in scope if it expresses a generalization that is significant for the study of ritual. The metarules in Pāṇini’s system work this way: they regulate only some of the other rules of the grammar, not all of them.\textsuperscript{11}

To understand this, we may think about how a formal analysis along the lines Staal proposes would explain, for example, Vedic oblations. Let me therefore propose a tentative ritual analysis, which I will refer to throughout this section. Consider such oblations in the context of the central portion of a Vedic īṣṭi ritual:

\begin{align*}
S & B & A & B & S \\
\end{align*} \quad (2)

Here S indicates a sāmidheni sequence, B represents a sequence of oblations, and A indicates the core of the īṣṭi, the āvāpa. S- and B-sequences frame the central core, but in what follows I will consider only one of these, the initial B-sequence.

Analyzing this B-sequence means breaking it down into its component parts. In the notation that I will adopt, basic types of oblation are indicated by an initial subscript, and variants on those types are indicated by further subscripts. We know from the Śrauta Sūtras that there are three basic types of oblation in such a sequence. Thus, āghāras are denoted by $B_1$, prayājas by $B_2$, and ājyabhāgas by $B_3$.

The two āghāras differ in regards to the god to whom they are offered (the first is to Prajāpati and the second is to Indra), so these are indicated by $B_{11}$ and $B_{12}$, respectively. The sequence āghāras-prayājas-ājyabhāgas can be generated by a recursive, reduplication rule for oblations ($B \rightarrow BB$) combined with further transformational rules that will give us the correct types of oblation in each case, in other words rules that add subscripts. Thus, the element B of (2) can be transformed into the sequence (3) by a combination of these two rules (reduplication and subscript-adding rules):

\begin{align*}
B_1 & B_2 & B_3 \\
\end{align*} \quad (3)
(3) tells us that the oblation sequence is composed of three distinct types of oblation, but it does not tell us the number of each or the internal composition of any of them. To get the correct number of performances of each oblation, we could rely on a rule of the general form $B_{(k)} \rightarrow B_{(k)}B_{(k)}$, which would allow us to preserve the subscripts without having a separate recursive rule for each type of oblation. If the subscripts were optional, this same rule could have been used to generate (3) in the first place.

A rule or rules that generate a second subscript could then be used, resulting in the following sequence (assuming five instances of prayājas and two instances of ājyabhāgās):

$$B_{11} B_{12} B_{21} B_{22} B_{23} B_{24} B_{31} B_{32}$$

(4)

Perhaps $B \rightarrow BB$ and $B_{(k)} \rightarrow B_{(k)}B_{(k)}$ could both be taken as instances of a more general rule, which could generate as many subscripts as necessary to indicate finer and finer distinctions between oblations. In any case, in (4) above the first subscript indicates that an oblation is of the type āghāra, prayāja, or ājyabhāga, and the second subscript indicates the deity to whom material is offered. The initial partitioning of $B$ into subtypes $B_1$, $B_2$, and $B_3$ is underwritten by the analysis found in the Śrauta Sūtras, as is the distinction between subtypes of these subtypes based on the deity to whom sacrifice is offered. But let me change my notation slightly.

Let us write $B_{31}$ instead as $B^g_6$ (i.e., an oblation made with ghee to the god Agni), and $B_{11}$ as $B^p_6$. In other words, alphabetical superscripts could be used to represent the material offered, and alphabetical subscripts the deity to whom offering is made. The analysis would, in general terms, follow the same lines as indicated above, except that the rules would have to be formulated differently to account for the different notation.

Of course, we could eliminate g-superscripts for convenience in our final notation, or only add substance-superscripts if the material used was something other than ghee, but this would be merely a notational variant and not of theoretical significance --- this would be similar to the convention of only

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specifying the base of logarithms when they differ from base 10 --- unless it captured a significant generalization.

Notice also that something of the form $B^p_0$ is not what I have called a unitary action, but rather an action sequence in its own right. The advantage to this notation is that it explicitly provides information necessary for generating the sequence of unitary actions that composes it. Thus, let us assume that $B^p_0$ contains the recitation of a certain number of mantras, in which the name of the god Prajapati is mentioned, and a sequence of the same number of offerings of ghee. Then in our analysis we would replace $B^p_0$ with the following sequence (where $M_p$ indicates a mantra mentioning Prajapati; $F_s$ represents an offering of ghee; and numbers in subscripts indicate the sequential ordering of particular actions, such that the mantra $M_{p_1}$ precedes the mantra $M_{p_2}$)\(^{12}\):

$$M_{p_1} F_{s_1} M_{p_2} F_{s_2} \ldots$$

(5)

The initial subscripts of each $F$ would be taken directly from the superscript of the $B$ from which it derived.

Let this analysis suffice for the present. The basic structure of the analysis should be evident: a series of similar ritual actions (in this case oblations and their accompanying actions, notably the recitation of mantras) can be broken down into smaller and smaller series, until unitary actions are reached, by means of a categorization scheme according to which types of ritual are interrelated in a rule-governed manner. I presented two formalisms for analyzing oblation sequences, with the second being intended to capture important qualities of the oblations, such as the material offered.

I began this analysis in order to explain why Staal seems to think that paribhāṣās like ĀpŚŚ 24.1.23 are important for understanding the ritual analysis provided by the Śrauta Sūtras. Such paribhāṣās are important just in case they provide important generalizations about the structure of Śrauta rituals; this becomes evident in the very structure of the analysis. Although in (5) all the offering-elements of the oblations (the elements called "F") are offerings of ghee, it would be possible to continue the analysis with transformations that would replace selected offerings of ghee with
offerings of, say, milk; it would also be possible to replace these globally in a ritual, of course. We
would represent such a case by changing the g-superscript to a m-superscript, thusly:

\[ \ldots B^g_p \ldots \rightarrow \ldots B^m_p \ldots \]  \hspace{1cm} (6)

The ellipses indicate that other symbols might be needed to account for context. In any case, the key
point is that, according to this analysis, no complex oblation symbol (such as \(B^m_p\)) with a superscript
other than g occurs except when that symbol has been derived at an earlier stage from a symbol with the
superscript g. In other words, a paribhāṣā like ĀpŚŚ 24.1.23 gives us constraints on the possible rules
we might consider in order to describe the Vedic ritual system. It therefore helps guide us to the actual
rules that govern the system, by eliminating some rules that at first might have seemed plausible. For
example, without the paribhāṣā ĀpŚŚ 24.1.23, we might consider generating a complex symbol (\(B^x_p\))
with a variable \(x\) that could then be replaced by a symbol from the set \([g, m, c, a, s]\)\(^16\).

There is, as I mentioned earlier, a second way to understand ĀpŚŚ 24.1.23. This rule might
state nothing more than a convention used to shorten the phrasing of subsequent sūtras that relate to
oblations. Then the analogue to a rule like ĀpŚŚ 24.1.23 would not be a scientific law; rather, it would
be a list of sigla at the beginning of a book. Far from possessing any theoretical significance, such a
rule under this alternate interpretation would be utterly trivial except for those interested in learning
how to read sūtra texts.

Staal assumes the significance of ritual paribhāṣās, but in fact it is impossible to know whether
they are, for the most part, of great theoretical significance or theoretically trivial, until a careful and
"scientific" study is made of the ritual system. Staal assumes the Śrāuta Sūtras are based on such a
study, but it is quite possible that they are in fact "prescientific": their similarity to the paribhāṣās in
Pāṇini’s grammar might indicate more a resemblance in form than in content. The fact that the genre
evolves noticeably between the Baudhāyana Śrāuta Sūtra and the Aṣṭādhyāyī indicates, if anything, that
Staal is quite possibly wrong in his reliance on the accuracy of the analysis given in the Śrāuta Sūtras:
just as the genre did not emerge fully-formed at one instant, so probably did the tools used to study ritual and language take time to develop.

The second example that Staal gives is of a somewhat different kind; he takes this example from a paper by Harold Arnold. Here the ritual parabhāṣā is ĀpŚŚ 24.2.13: “the ritual limbs should be inside, the officiating (priests) outside.” The ritual limbs (aṅga) are the implements used in performing the ritual, so this rule is what we might call a spatial-distributional rule. Up until now, I have used the term "distributional" to refer to sequential ordering in time, but of course rituals operate on multiple dimensions and sequential orderings are found in space when we look at the relative positioning of objects and agents in space at any given moment during a ritual performance. Language operates in only one modality, and time is the single measure of that modality (though space is the measure of written language); but it is only for convenience that, in accordance with the language analogy, we think of rituals as performed along a single, temporal axis. The interpretation of ĀpŚŚ 24.2.13 thus seems quite clear.

Staal compares it to a rule given by the grammarian Kātyāyana, which makes use of the terms antaraṅga (“inner”) and bhaṁgra (“external”). According to Kātyāyana's metarule, a rule classed as bhaṁgra cannot be applied until a rule classed as antaraṅga has been applied. The terminology is certainly similar in the two cases, but otherwise the similarity does not seem terribly close. Both contain the word aṅga (“limb”) and both use the stems antar- and bhaṁ- in the sense of "inside" and "outside". But there is clearly an important difference: "inside" and "outside" refer to the order of application of rules in the linguistic case but not in the ritual case. If the linguistic sūtras constitute the "scientific" standard against which the ritual sūtras are measured, then the latter seem to lack in the comparison. The linguistic rule clearly helps to structure the rule system, and while the ritual rule is, I would argue, almost certainly of theoretical significance, it serves a very different function than the linguistic rule. In fact, Kātyāyana's rule provides a good example of the role of many metarules in the linguistic sūtras. They help determine the scope of rules, and they clarify how different rules interact so
as to determine a single linguistic output for every sentence generation. The "corresponding" ritual rule does no such thing. It would appear that Staal has been mislead by a merely verbal similarity in this case.

4.3.3. Summary. The preceding section covered a lot of material rather quickly, so it is perhaps worthwhile to pause briefly before continuing.

As I point out in this chapter and the next, any scientific approach to ritual will have to find generalizations analogous to laws in the natural sciences. The status of laws and their ontological implications are the subject of active debate in the philosophy of science, as is the relation between the natural and human sciences, so my treatment of ritual generalizations in 4.2 was intentionally informal. Nevertheless, the analogy between ritual generalizations and scientific laws should be clear enough.

Staal argues that rituals can be analyzed in terms of phrase-structure and transformational rules. But it is not clear that just any system constructed of rules provides sufficient meat for scientific study (would we speak of the science of chess or backgammon?), and thus it seems Staal was motivated to look for other ritual rules, of more general application. I have proposed that his interest in paribhāṣās can best be understood in this light. It is not sufficient to think of paribhāṣās as metarules if a science of ritual is our goal. Rather, paribhāṣās must capture broad generalizations that underlie a ritual system. Broad generalizations may take the form of metarules, but they need not.

I have already considered Staal's treatment of paribhāṣās. He presented two examples, which seem to me insufficient to achieve the purposes I have attributed to him. Staal's first example was intended to show that paribhāṣās can express generalizations about a ritual system that help to determine the proper analysis of that system. I argued that we cannot know whether he is right about this until we have considered critically the analysis provided by the ritual sutras, but I allow that Staal might turn out to be correct. Staal's second example was supposed to illustrate a second way that ritual structures are like language structures, but his interpretation of this parallel seems to me to be quite clearly wrong. It therefore seems worthwhile to consider ritual and grammatical paribhāṣās further, in
order to determine whether Staal's intuitions are helpful, even if his application of them seems somewhat weak.

4.3.4. Paribhāṣās in Ritual and Language. Let me look then more generally at ritual paribhāṣās, and see what other evidence we can find to support Staal's position. A good and detailed survey of these paribhāṣās as a whole is provided by Chakrabarti 1980, a revised version of his University of Calcutta dissertation. Chakrabarti is interested in the texts and text-histories of the sūtras, however, and only considers the rituals they describe when this is necessary to explain variations between different sūtras. Since Chakrabarti's book surveys the ritual paribhāṣās comprehensively, I will use it as my guide to the texts, and draw attention to various sūtras and to his interpretations of them only when relevant to my theoretical concerns. Similarly, I will rely on Cardona (1988/1997) for my treatment of the grammatical paribhāṣās.

The ritual sūtras exhibit strong surface similarities, and also strong differences, when compared to the grammatical sūtras19. A key point of similarity is the technical terminology that both use, as has been commented on by a number of scholars, including Staal. I wish to begin, however, by considering a point on which the sūtras seem to differ. The Aṣṭādhyāyi begins with elementary units of language --- sounds and morphemes --- and shows how these combine into larger units of speech. The ritual sūtras, however, are organized into sections that address different key rituals, so in a sense they begin with the largest possible ritual units and only then describe the more elementary parts that constitute these rituals. We would not be too far off, then, to say that the grammatical sūtras follow the method of synthesis while the ritual sūtras follow the method of analysis. Several caveats would be in order, of course, if we were to make such a coarse generalization, for example the ritual paribhāṣās seem to capture synthetic generalizations when we view them as Staal wishes us to do. But the fact remains that the basic structure of the grammatical sūtras differs substantially from that of the earlier ritual texts, and this fact deserves careful attention inasmuch as it might point to substantive differences in the methods of the two genres.
This point comes across most clearly when we look at the phenomenon known as atideśa. In the ritual sūtras, paribhāṣās govern the process of application of details from an archetypal ritual (prakṛti) to an ectypeal ritual (vikṛti). This statement probably deserves some explanation. The ritual sūtras describe a large number of distinct rituals, and so as to avoid having to repeat the same details over and over again (because many of the rituals are largely the same, though they display certain key differences), the texts first present detailed descriptions of a small number of rituals and then describe ways in which other rituals (ectypes) differ from these archetypes. I have already described how we could develop a basic model of sāmīdhēnī offering, and then treat alternate types of sāmīdhēnī offering as simply modifications of a given prototype: this is an example on a much smaller scale of the general procedure that the ritual sūtras follow, with the difference that sāmīdhēnī offerings are rites within a larger ritual, not independent rituals in their own right. If Staal is correct in thinking that the ritual sūtras are scientific texts, then the paribhāṣās express scientific generalizations just as, at a less general level, specific phrase-structure and transformational rules do.

Since the general arguments for and against this view of atideśa have already been given, I will not consider them at length here. Suffice it to say that paribhāṣās should only be seen as expressing scientific generalizations if they capture the actual processes that structure the ritual system, and we cannot take it for granted, without further study, that they do this. It should be noted, however, that the paribhāṣās do express real generalizations; whether these count as scientific generalizations is a theory-laden judgment, but the breadth of some of these generalizations is not in doubt. Take one example: "Only the mantras that are congruous with the acts in archetypes should be modified in the ectypes. If a Mantra is not in conformity with the archetypal act itself in the context of which the Mantra occurs, it should not be modified in the ectypes either" (Chakrabarti (1980, p. 135)). What this means is that some of the mantras (verse quotations from the Vedic saṃhitās) recited in the course of Vedic rituals can be taken as referring to the action of the ritual --- it is these, and only these mantras, that are modified from their archetypal form to suit the context of a given ectype. Mantras that are not taken as
referring to ritual actions remain unmodified. This is an extremely broad generalization as it encompasses a huge number of particular instances and apparently has few if any exceptions.

The process of atideśa seems quite different in the grammatical sūtras. Here "atideśa" refers to the paribhāṣās that "extend to elements operations and properties which would otherwise not pertain to them" (Cardona 1988/1997, 57). For example, rules that govern initial and final sounds would normally not be taken to apply to stems containing only a monosyllabic vowel, so a special rule is necessary to allow such rules to apply to forms like ait ("he went") and ābhyaṁ ("them")22. The metarule applicable here is Aṣṭ. 1.1.21 (ādyantavad ekasmin), which Cardona translates as follows: "an operation takes effect with respect to a single sound (ekasmin) as it would with respect to an initial or final sound (ādyantavat) of a larger unit" (Cardona 1988/1997, 58).

In short, we find in both the ritual and grammatical sūtras cases in which properties are applied to one product (ritual or utterance) that we would have expected, but for the paribhāṣā, to apply to another product but not to that one. But the ritual and grammatical rules seem to project their properties from one product to another in quite different ways.

In the grammatical cases, it would appear that the atideśasūtras always serve to modify the conditions under which another rule applies (or does not apply, as the case may be). Thus, the rule that "a substituend (sthānin) has the same status as its replacement (ādeśaḥ) except in respect of an operation that depends on an original sound (anālvidhau)" (Aṣṭ. 1.1.56) does not apply, in respect to the element preceding the modified element, when a vowel is replaced in a right context (i.e., the modification is conditioned by the element that follows the element that is modified) (Aṣṭ. 1.1.57). And just as Aṣṭ. 1.1.57 stipulates conditions under which Aṣṭ. 1.1.56 does not apply, so the next two rules (Aṣṭ. 1.1.58-9) give conditions under which Aṣṭ. 1.1.57 in turn does not apply.

My impression is that, in general, ritual structures do not impose contextual constraints on elements nearly as much as language structures do (in other words, many ritual elements can occur nearly anywhere in a ritual sequence); and when ritual structures do impose constraints, they tend to be

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fairly absolute (exceptions occur much less often than in natural language). A great deal of empirical research would be necessary to demonstrate this, of course, but if my impression is correct, it has major implications for the language analogy. In any event, in the case of Vedic ritual the term "atideśā" is not used for such modifications of the conditions of application of other rules\(^2\). Rather, it is used as I have already described, namely to identify to relationship between ritual archetype and ectype. I am not sure what the natural language analogy of this relationship would be.

Perhaps the closest phenomenon in the grammar of Sanskrit can be found in the relation between nominal and verbal roots. While many Sanskrit nouns are built on bases that are not used to construct verbal forms (see Aṣṭ. I.2.45), many do derive from verbal roots. The sūtras following Aṣṭ. 3.1.93 give the (so-called kṛt, or "primary") affixes that turn verbal bases into nouns; another set of affixes, beginning with Aṣṭ. 4.1.76, are referred to as taddhita or "secondary" because they can generate additional nouns from other deverbal nominals (Cardona 1988/1997, 192-203, 229-255). In such cases, we could perhaps consider any verbal form as a sort of archetype, and a nominal form built on the same base as an ectype derived from the same verbal base (dhātu). Whereas the offering and the accompanying mantras may undergo substitution when transforming one ritual into another, so the affixes attached to verbal bases may undergo transformation. This is not how the sutras describe this phenomenon, of course, but it is the closest analogy to the phenomenon of ritual atideśā that I can find.

In short, whether we look at similarities of language or similarities in the form or application of rules, it appears that metarules play a significantly (though not entirely) different role in the generation of ritual structures than in the generation of language structures. Another possibility would be that the classical sūtras (grammatical and ritual) are not as scientifically accurate as Staal would have us believe. Perhaps the same phenomenon does in fact occur in the formation of deverbal nominals as in the modification of ritual archetypes. I do not mean to suggest that this is the best way of drawing the parallel between ritual and language (it probably is not), though this may serve as an example of the sort of critique that could be mounted against the analysis given by the ritual (or grammatical) sūtras.
In this case, the similarity in terminology might draw attention away from similarities in the underlying rule systems.

4.3.5. Purpose of the Śrauta Sūtras. Let me conclude this chapter with some broad reflections that might shed further light on Staal’s reliance on the analysis presented in the Śrauta Sūtras.

Staal wishes to show that the Śrauta Sūtras are scientific in their method of describing ritual structure. To be scientific does not mean to be correct in every detail: it has more to do with the way that a theory is developed and supported. I will have more to say about Staal’s understanding of “scientific” in Chapter 5, but for the present let us focus on the purpose of the ritual sutras in order to get a handle on the role that pārībhasās play in the Vedic ritual system.

The Śrauta Sūtras hold that the Vedic samhitas and brahanas prescribe correct ritual action, in the cases where those early texts give ritual prescriptions at all. The ritual sūtras are designed to fill in the gaps, to provide prescriptions not found in the earlier sacred literature and to present all the ritual prescriptions in a systematic fashion. Clearly the sūtras, and all the earlier literature that is still extant, were transmitted largely or entirely by oral means, at least at first. The extent to which the genres themselves were affected by the need for oral transmission is perhaps open to question, but no doubt the method of transmission affected the texts that have been preserved into the present, in one way or another. The sūtras texts were composed by authors working inside the tradition, rather than by objective observers interested in religion as a general category, and must have been composed by ritual practitioners for ritual practitioners. This much I believe we can take for granted.

It is a straightforwardly empirical matter (based on the evidence of the texts) that there were debates among ritualists at the time that the extant sūtras were composed over details of the ritual, as well as over the proper interpretation of the rituals and the grounds for assessing rival practices. Chakrabarti discusses several of these. The purposes of the authors of the ritual sūtras were therefore very different than the purposes of modern scholars of ritual theory, or of modern scientists generally. This by itself does not affect Staal’s view of the ritual sutras as scientific: the descriptive adequacy of
Panini's grammar is not affected by the religious concerns of its author, and the same argument can certainly be made for the ritual śūtras. But the concerns of the authors of our texts should not be ignored any more than they should be treated as the sole basis for our assessment of the value of the analysis of ritual that they contain.

In particular, the fact that different śūtras differ over details of the ritual, and that different schools appear to engage one another in debates, should be a sign for some concern. The śūtras are not purely descriptive accounts of actual performances, nor are they theories about the structure of actual rituals. They are, rather, theories about what the rituals ought to be, theories about what the tradition, partly codified in earlier sacred texts that have survived to the present and partly implicit in the realm of practice, says ought to be done. They therefore are theories of what the rituals ought to be.27

Consider the analogous situation in the case of language. If we think of a language as competence, we realize that the actual utterances of a particular individual might rarely, or never, be entirely well-formed, for example in the case of a stutterer. Yet we would not be inclined to say that linguistics describes the formulas that the stutterer ought to say but doesn't. Rather, we would say that linguistics describes the expressions that the stutterer actually produces, i.e., the output of the language faculty is well-formed, and the vocal output is ill-formed because of a failure of the output device. The problem of anacolouthon is somewhat different. Here the output of the language faculty is ill-formed because of a choice (conscious or unconscious) to abandon a construction before completing it, and begin in media res with a different construction. Perhaps then linguistics describes what ought to have been the output had the original sentence been finished, but this is a very different sense of "ought" than in the case of the ritual śūtras. The ritual-śūtra "ought" is an "ought" in the proper sense: it indicates a value rather than a fact. But the linguistic "ought" is really shorthand for a statement of fact: "if the language device functions properly, then the signal to the output device will be well-formed."

My point is that the Śrauta Śūtras are theories of how rituals ought to be performed, and consequently it seems likely that there will be cases in which they reach judgments about the correct structure of ritual that would differ from the judgments of competent students of the human sciences.
This is why I have argued that modern scholars who wish to develop formal analyses of the Vedic ritual system will have to start with distributional descriptions, though the Śrauta Sūtras will certainly prove valuable at various stages of analysis -- in addition to providing an analysis of ritual structure that modern scholars can build on, the Śrauta Sūtras also provide us with valuable and irreplaceable evidence for the composition of Vedic rituals.

1 This is, of course, Wittgenstein’s arithmetic analogy as applied to language learning. In fact, the infant will encounter words and sentences, and intuit from them the morphology and syntax appropriate to the language to which it is being exposed.
2 Stern (1996, p. 473, note 19) estimates the size of the Nachlass as amounting to 20,000 pages: 12,000 in handwritten manuscript pages, and 8,000 in typed pages. He adds that this might amount to at least 2.5 million words.
3 In other words, I am sceptical of the view presented by Kripke. That view is Wittgensteinian in that it is attributed to a reading of Wittgenstein’s works, and it is sceptical in much the same way that, for example, Hume is sceptical.
4 When I use the phrase “American marriage” in what follows, I do not mean this in any essentializing way. The phrase is simply shorthand for “a type of marriage in America”. I should add that I wrote this section shortly before a major public debate erupted in the U.S. over the definition of marriage; by using this particular example, I am not taking any stand on public or social policy. The marriage example was suggested only by Sperber’s well-known treatment of the topic (see below).
5 If we were studying marriage rituals, we would have to define such a marriage rite in terms of a pair [S, P], such that S is a set of actions and P (rho) is a relation between the marriage rite in question (which by definition conforms to S) and any other rite also conforming to S, in which one of the actors to be married occupies the same role as in the marriage rite under study. Call the rite under study s₁ and any other rite meeting these conditions s₂. s₁ counts as a marriage just in case there is no s₂ that was performed previous to the performance of s₁.
6 Of course, Searle developed the term “constitutive rule” to explain linguistic phenomena, but that the notion is not restricted to language is clear from the fact that he explains the idea with reference to the role that such rules play in the generation of games.
7 I should add in passing that my discussion of the similarity between social generalizations and analytic statements is consistent with Searle’s analysis: “Constitutive rules constitute (and also regulate) an activity the existence of which is logically dependent on the rules” (Searle 1969, 34); “notice that [constitutive rules] are almost tautological in character, for what the ‘rule’ seems to offer is part of a definition of ‘checkmate’ or ‘touchdown’” (Searle 1969, 34); “that such statements can be construed as analytic is a clue to the fact that the rule in question is a constitutive one” (Searle 1969, 34).
8 Here I use “scientific adequacy” as a general term for the criteria that I have already listed: the texts adequately describe the ritual system; the rules they present actually work to structure the ritual or language system; and these rules meet the same sorts of standards required of laws in other special sciences.
9 Beginning perhaps with the Baudhāyana Śrāuta Sūtra (Chakrabarti 1980, p. 49, Staal 1989a, p. 353).
10 “General rules” can be used to mean rules that capture what linguists call “significant generalizations” or simply rules with a wide range of application. Here I intend the former sense, though later in this chapter I will make use of both senses (while always attempting to be explicit about which sense is meant in any given context). Note, then, that paribhāṣās may prove to be metarules but not general rules (in the first sense).
Let me add in passing (I do not want to pursue this hypothesis at length here) that the discussion in Humphrey and Laidlaw 1994 of ashtanars in the later Indic tradition may also prove relevant to an understanding of Vedic metarules (page 119-120). They believe that ashtanars are regulative rules, some of which prohibit events that are unlikely to occur in the usual course of ritual performance. We could generalize their argument and propose tentatively that some of the metarules might either prohibit or allow for the possibility of actions that were never viewed as likely to happen. In such cases, the default action would be the only action that one might expect, and the possible alternatives might be proposed (if we accept Humphrey and Laidlaw's hypothesis) only to emphasize to ritualists that the ritual system is rule-governed, and not as viable options. I mention this possibility only to emphasize that other interpretations of Vedic metarules are possible, especially in cases where it seems that there is no alternative to the default case enjoined by a metarule.

Recall that I defined the symbol B as a sequence of obligations, so (3) does not necessarily indicate only three obligations, even though it contains only three letter symbols.

Ghee offerings to the Samidhs, Tanaupat, Id̄a, Barhis, and Savahakara. Apparently offerings within the sacrifice are treated as gods for purposes of the prayāja; see Kane 1941-1975 (1057), vol. 2, pt. 2, n. 2368.

Ghee offerings to the gods Agni and Soma, respectively.

The numerical subscripts are important because the mantras recited in a given oblation sequence are generally different for each recitation in the sequence. Thus it would not be satisfactory to write $M_k F_{a}, M_k F_{b}, \ldots$, since this would imply that every recitation in the sequence was identical to every other.

Vedic oblation offerings can be of any of five types: ghee, milk, corn (grain, not maize), animal, or soma, see Chakrabarti 1980, 171.

Actually, Staal says that the term refers "primarily" the implements. I do not know what else could be covered by the term, and I think his "primarily" is conditioned by his understanding of "implement", which might not include objects like ritual fires. But I am not absolutely sure why he says "primarily".

Katyāyana was the author of the first extant commentary (vārttika) on the Asṭādhyāyī of Panini. He lived around 250 B.C., and is perhaps the same as the ritualist of the same name. Staal seems to like the possibility that the two authors were the same, as it may be an indication of the similarity in thought processes between ritual and language study.

Although for convenience I will speak of the grammatical sūtras in the plural, what I really have in mind in what follows is the Asṭādhyāyī.

Renou 1941-2, 131 noted that the word "atideśa" was used in both genres, but unfortunately did not consider the implications of its differing usage.

The technical term for such modification of mantras is Ūha.

The monosyllabic stems for these words are i and a, respectively.

I have given one example of such a case already: the jūhū is the default implement, but when a jūhū is already in use, the default implement becomes the śrīva (ĀpSS 24.1.25-26, see section 4.3.2 above).

For example, does the concision of the sūtras make them more amenable to accurate transmission than the more wordy, prose Brahmanas?


We see the same situation elsewhere: for example in the exact sciences, where the theological concerns of Ptolemy or Newton, significant as they are for understanding the evolution of these scholars' theories, do not in any way damage the descripational adequacy of their theories.

I should admit that a similar charge has been leveled against Pāṇini: that the language he describes was never a living dialect, but rather a somewhat artificial form of Sanskrit that in later centuries became prescriptive.

Anachronism, or grammatical inconsistency, is the inadvertent or purposed deviation in the structure of a sentence by which a construction started at the beginning is not followed out consistently" (Smyth 1920/1984 (671)).
The case is somewhat more complicated than I let on here. We can think of Vedic ritual as pragmatic action to achieve some goal, such as greater wealth or heavenly rewards. Vedic ritual could also be viewed as an obligation, actions that certain people ought to do because of their station, or because of the will of the gods. In the latter case we would say that "ought" is a value statement. But these two alternatives are both traditional beliefs about the source or end of ritual action, and thus they fall under the broad category of ritual meaning. If we are considering the ritual faculty as a component of the mind, then judgments of whether a particular performance is well-formed are just judgments of fact, regardless of what interpretation a religious tradition gives to them.
Chapter 5

Staal has written on the history of science in a number of publications, touching in the process on problems of a more philosophical nature. He has demonstrated a good command of the semipopular, secondary literature in the history of science (for example, he cites Neugebauer's short *Exact Sciences in Antiquity*, but not his monumental *History of Ancient Mathematical Astronomy*) and of course he has a much better command of the literature on science in India than in the "Middle East". Two lecture sequences (Staal 1982b and 1993b) were published as short, independent volumes. As with his other work on ritual, many of his arguments are revisited in a number of places, especially the excellent first lecture in Staal 1982b, which was also published separately as Staal 1982a.

In all of these works, Staal's interests are explicit: he wishes to remind his readers that science did not originate just in Greece (as Westerners for a long time believed), or even in Mesopotamia, Greece, and China (as was becoming clear in Western scholarship by the middle of the twentieth century). India also played a role, especially in linguistics: Indian linguistics was the catalyst of modern linguistics research in the West, and reached heights not achieved in the European-American tradition until the twentieth century. Important to such an argument is the distinction between different classes of science, in particular the division between natural, social, and human sciences. It seems likely that much research in the exact sciences in India was based on Mesopotamian models, though as he points out in Staal 1995a certain results were derived from this shared base in early modern India before they were reached in Europe. But any contribution to the comparative history of science needs a theoretical basis, and while he shows a good knowledge of research in the history of science, Staal curiously enough¹ demonstrates no interest in contemporary philosophy of science.

My work in this chapter will therefore be rather simple. Following my approach in previous chapters, I will not attempt to review all of Staal's arguments, but rather select those that are most essential to understanding his theory of ritual, as I have presented it, and consider them in that context. Readers who wish to get a feel for his approach to the history of science would do well to begin with Staal 1993b and 1995a; the former is easy to read and very nicely written; the latter is a careful and
serious work of scholarship. In the second half of this chapter, I will pursue an issue that I have alluded
to a number of times already, namely the psychological interpretation of Staal's ritual theory, building
on the base established in section 5.1. And just as section 5.2 builds on section 5.1, the latter in turn
builds on the arguments I developed in a casual and intuitive way in section 4.2. Section 5.3 connects
Staal's work on ritual syntax with other cognitive studies of religion, especially those of Sperber and
Boyer.

5.1. Staal's Philosophy of Science. In a number of places (Staal 1982a, 1982b, 1989a2), Staal
presents his own list of four features characteristic of science:

(1) empirical adequacy
(2) abstract generalizations
(3) consistency ("to some extent") in description and generalization
(4) the existence of a methodology of argumentation.

He briefly considers a fifth feature, "the pure and theoretical nature of science", only to reject it. This
feature was of course important to the Greek philosophical tradition, and had originally been presented
as a criterion for distinguishing between the application of two basic types of knowledge, which we
might call the pure and the applied. One of the key elements of Staal's philosophy of science, then,
affirms that pure and applied sciences are equally sciences.

In this list, the ability of scientific theories to predict future outcomes is given little attention.
It is included under #2 on the list, though it seems prediction is not a necessary property of a scientific
type: "such generalizations may include predictions" but they need not. Already this tells us a great
deal about Staal's view of the sciences. We might say that for Staal, a science is a formal method for
considering a subject matter. Though he does not consider this example, we could expect that he would
assent that there is (potentially) such a thing as a science of literature, as long as there was a corpus of
texts upon which to base theories, plus generalizations, standards of consistency, and methods used in
the study of literature.3 This follows from his list of features as given above. In fact, it is hard to think
of any field of study that is not amenable to scientific study under these broad, vague standards. Yet I do not know what sort of predictions a science of literature could ever produce. A science of literature would almost necessarily be ex post facto, describing and systematizing our knowledge of extant works of literature without telling us anything about the potentialities of particular genres or styles, or about how or why literature is produced or why it takes the forms it does in particular cases.

It appears that Staal's claim, that India played an important role in the history of science, is in fact a rather hollow and uninteresting claim if it means no more than "India has had people who spent a lot of time thinking about particular topics, and developing consistent views on those topics built on general claims and argued carefully". In other words, it may appear that for Staal "India developed sciences" means little more than "India had intellectuals and schools of thought". The sciences occupy a privileged role in Western thought, and claims that India had science should be able to do better than Staal provides for. But this will require tightening the standards Staal presents, and this in turn means that we might have to reject as scientific some of the sciences that Staal would accept.

Features #2 - 4 have to do just with the formal properties of a scientific system, and #1 seems to impose only minimal constraints on scientific theories. This becomes clear when we realize that Staal seems to accept mathematics as a science. He does not explicitly say, as far as I can recall, that mathematics is in fact a science, but he gives us good reasons for believing that this is the case. Yet according to the received view, mathematics plays a key role in many natural and social sciences without itself being a science. This is because mathematics is founded (largely, at least) on logical grounds, and is in any case entirely prior to empirical considerations. The more mathematics is a science, the less feature #1 has any traction.

In addition to downplaying the role of prediction in the sciences, Staal also does not take into account the role of mechanism in the sciences. Mechanism is in fact a subject of debate in the philosophy of science. Many fields reject the significance of mechanical explanation, as for example most areas of applied psychology or epidemiology do. Thus in recent decades we have seen large increases in the number of diseases, disorders, and medical conditions from which people might suffer -- at the same time, ironically, as Foucault-inspired research has rejected the very notion of pathology as
culturally conditioned and consequently not scientific in any real sense. Many diseases (AIDS, for example) were originally defined in terms of symptoms and only later redefined in biological terms, while other conditions (Attention Deficit Disorder, for example) are unlikely to ever be redefined in terms of underlying physical causes. If we restrict our attention to the exact sciences, we see that problems with traditional mechanical models have resulted in a variety of philosophical theories. After it became clear that light, and even substances at the atomic level, cannot be adequately described in terms of either particle or wave models alone, "instrumentalist" arguments began to appear, holding that theoretical (non-observational) terms do not, strictly speaking, refer at all. Under such a view, mechanical models might prove useful but are no longer necessary in underwriting the adequacy of any scientific theory.

Throughout this study, I am attempting not to commit too firmly to any one philosophy of science, because I think that many questions are still open to debate. But I do fall on the side of those who believe that mechanical models are good to the extent that they can be developed; and further that scientific theories built solely on perceived regularities (whether quantifiable or not) are generally suspect. In section 5.2, therefore, I attempt to develop a minimal mechanical model for a theory of ritual along the lines that Staal proposes.

Close to the problem of mechanism is the related problem of the ontological implications of scientific theories. In section 4.2, I attempted to pump my readers' intuitions about ontology and the similarities and differences between the natural and human sciences. Then my focus was on generalizations, laws, and rules; here it is on theories of science more generally, so I will return to that discussion now with a different emphasis. I introduced a rough-and-ready distinction between contingent, ontological, and governing generalizations in order to make two points. First, it seems important to distinguish those generalizations that tell us about the properties that structure the world and the things in it from generalizations that just happen to obtain in a particular time and place. It turns out that these distinctions are much harder to justify than we might at first think, but it still is an important exercise. We tend to end up with essentialistic terminology when we try to argue that a
particular generalization is of the former type rather than the latter, and essences are philosophically problematic. But again, the goal seems worth the effort, even if we do not have complete success.

It appears that the best generalizations about social life and practices belong to a category all their own. I contrasted social generalizations with analytic truths in roughly the same way that I contrasted ontological generalizations with contingent truths, in order to justify the intuition (which I am not alone in holding) that the facticity of social phenomena is qualitatively different than the facticity of physical phenomena. This is a topic on which Staal has quite definite views, and on which I tend to disagree with him. Staal holds, as I have already said, that there can be sciences built on all sorts of things, perhaps on any subject at all. The degree to which a theory is scientific is measured by the degree to which it is formalized, and has nothing to do with the properties of the thing being studied through the theory. Staal does require that the theory have empirical content, but it is not clear how stringent this requirement actually is for him. All my talk of facticity, however, aims at just the opposite conclusion, namely that some subjects are more appropriate objects of scientific theorizing than others. In particular, I have indicated reasons for thinking that social facts are a different kind of thing than physical facts, and a corollary of this is that they require different methods of study --- in other words, that there is a principled distinction between the social and natural sciences. Presumably the human sciences are a third area all their own, though my Durkheimian orientation tends to prejudice social causes over individual causes, and consequently leaves scant room for the human sciences. Staal disagrees: "the case of ritual demonstrates that existing classifications of the sciences --- and especially the alleged distinction between the sciences and humanities --- lack a serious foundation" (Staal 1982a, 32). But this is just what we would expect, since he holds that every subject has the same potential for scientific study. In short, it seems that a theory can be more or less scientific depending on how well it meets certain standards, but all fields are equally open to scientific study.

Let me also point out that despite his rejection of ordinary language philosophy (see Staal 1995a, 121), Staal sometimes seems quite persuaded by implications he draws from non-technical language. Thus in, e.g., Staal 1982a, 5 and 1995a, 78, Staal looks at the words for science in Sanskrit, French, German, and Dutch to argue against the privilege given to the exact sciences in Anglo-
American philosophy. The argument is that since "Geisteswissenschaften" and "sciences humaine" are acceptable terms in German and French, and similarly neither "śāstra" nor "vedāṅga" is an adequate translation of "science", clearly we should not unilaterally restrict the latter term to the study of physical phenomena. Like many of his arguments, this is perhaps interesting to those already sympathetic to his case, but it is not intended as a persuasive argument in its own right.

In any case, the larger context of this problem is a well-known one in the study of religion. The phenomenologists most notably have argued that the human sciences require a different type of study than the natural sciences. *Explanation* is a function of the latter, but the former engage instead in *interpretation*: the actions of humans qua humans cannot be adequately explained, but to a greater or less extent they can be understood by sympathetic others through a process of setting aside of our usual engagement with the world through a process of "epoché". Phenomenology is one of the chief bases for the contemporary study of religion, and by rejecting both it and Anglo-American philosophy of science Staal is attempting to situate his thought in a rather unique position within the contemporary scene. The opening chapters of Staal 1989 are intended to explain and justify this stance: Staal's position, while a minority position today, is in fact based on a framework within Continental thought that was dominant before the split between Anglo-American and Continental thought began around 1900. Or at any rate, that is a one-sentence summary of the matter. The significant point is that Staal recognizes how his philosophy differs from most of his readers'. My position is similarly but differently divergent from the mainstream.

5.2. Staal and Linguistic Theory. Staal's adoption of a version of the language analogy was not random: trained in mathematical logic, a significant portion of his scholarly energies in the 1960's and early 1970's were devoted to research in generative grammar and the relationship between natural language and formal logic. He therefore understood quite well the debates over method and the proper uses of data within the field of linguistics, and when he turned to ritual he was therefore in a much better position than most people working in the social and human sciences to apply linguistic models to the study of other aspects of social life. In addition to his work in generative linguistics and logic, Staal also devoted significant energy to the study of the Sanskrit grammatical tradition and to Indian
philosophy. This spreading of his energies over a number of loosely related fields has brought him criticism from specialists in some of these areas, as is perhaps to be expected, but it did put him in a position to produce original and sophisticated cross-disciplinary scholarship in a way that few could manage.

We might take as an example the short monograph *Word Order in Sanskrit and Universal Grammar* (Staal 1967) which focuses attention on one particular problem in generative grammar, namely that it was originally developed with English in mind, a language that identifies grammatical relations largely through word order; consequently, generative grammar appears to work less well for languages like Sanskrit (and in fact all early dialects of Indo-European) in which word order is largely unconstrained. This volume did exert some influence on subsequent developments in generative grammar, for example it was heavily cited in Boas 1975, but overall I am inclined to think that it received less attention than it was due. The only review that I have been able to find was written by the eminent Dutch Indologist Jan Gonda. Gonda, choosing to focus on the monograph solely from the perspective of a Sanskritist and not a linguist, consciously and explicitly refuses to read Staal's argument for what it says that it is, for example arguing that "the answers to these questions [relating to the semantic implications of different word orders such as SOV, OSV, and VSO] can be found by a profound study of a sufficiently large collection of Sanskrit texts" (Gonda 1969, 252). The key to this quotation is the word "profound". Gonda adopts the philological view that a reader with a proper appreciation for the Sanskrit language will understand the reasons for a particular word order in context; in other words, he focuses on the specificity of a particular sentence, rather than the general principles that govern the language system as a whole. In fact, Gonda's criticism is quite similar to the phenomenologist's criticism of scientism in the study of religious phenomena, as both argue that an empathic method relying on interpretation rather than explanation is the best way to appreciate the subtleties of meaning found in any unique context. Staal's position, of course, is that even a large corpus like the extant collection of classical Sanskrit is liable to provide an insufficient number of sentences containing the same words in different orders. The latter is, of course, necessary for a proper

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distributional study, and therefore for any general theory of the role of word order in generating underlying phrase-markers in a language.

Gonda concludes his fiercely critical and condescending review with the comment that perhaps generative linguists would be more interested in a monograph that studies free word order in "one of the generally known living languages" (Gonda 1969, 253). Put simply, he feels that few Sanskritists will have any interest in general linguistic theory, and that few linguists will bother to read a monograph on Sanskrit, even if it contributes to a general understanding of linguistic theory. In large measure, Gonda's analysis of the scholarly scene seems to have been accurate, even if his own critique is less than generous. This is an example of a problem that has beset Staal's work in ritual as well: he has come under fire for his limitations on the Indological side, whereas the amount of technical detail in his ritual analyses has inclined few specialists in other traditions to learn enough about Vedic ritual to allow them to seriously engage his arguments.

As with my approach elsewhere, I am more interested here in the problems Staal addresses than I am in assessing Staal's contribution on its own terms. But since Staal has presented the most detailed and serious attempt to date to develop a syntactic theory of ritual, it does profit anyone interested in general ritual theory to understand something of the background to Staal's own theory, especially in cases where he has left the connection implicit in his own writings on ritual. This was borne out in my discussion of computational models in Chapter 3, and it is the case here as well. The current section, then, is a compromise between a systematic evaluation of Staal's work in linguistics and a complete neglect of it. I will focus attention on representative publications that illustrate key issues for understanding his views of ritual theory and the application of scientific methods to the study of social and human facts.

Staal's linguistic researches can be divided into several categories, though most of his publications overlap these boundaries to varying extents. Some are focused primarily on problems in the interpretation of Indian grammarians or philosophers, and hence belong more to the history of Indian thought than to original linguistic theorizing. When he considers linguistic problems primarily on their own account, he works consistently within the transformational-generative model. His work
demonstrates a continuing interest in the problem of how well formal languages (symbolic logic, scientific language, etc.) capture the subtleties of natural language, and this frequently leads him to reflect on semantic theory, and how firm the boundary is between semantic issues and those treated under syntax, pragmatics, etc.

In addition to a number of single-authored publications, Staal was a primary participant in several international linguistics conferences, the proceedings of which we published, see Staal 1969, 1971, and 1974.

5.2.1. The Language of Science and Nominalization. Linguistics plays an important role in Staal's history of science, especially in India where it was the most developed science. But there is another important connection between language and science as well: language is necessary to express scientific theories. While this is a fact well known to philosophy after the linguistic turn, Staal's approach to the topic has been relatively unique: he considers scientific language, even the most abstract and rarefied, as occupying a continuum with everyday natural language. Let us consider briefly the implications of his assertion that "both scientific Sanskrit and Western formal systems can be described as specimens of artificial Indo-European" (Staal 1965c, 173).

Because of limitations of space, my discussion in what follows will necessarily not be able to do justice to the topic, which is in fact quite complex. What I wish to achieve in this subsection is merely to provide some motivation for Staal's claim that artificial language is always just an extension of natural language, formed by extending procedures used in natural language to a degree rarely or never encountered in ordinary uses of natural language.

(4) below, "namely (x) (3y) (3z) [R(x) → T (x,y,z)]", may serve as an example of a "Western formal system". Usually a formulation such as this is described as a statement fully expressed in the notation of formal logic: natural language translations may be given (for example, the sentence "everyone in the room knows two languages" in English), but predicate logic is generally viewed as possessing an artificial language all its own, in which logicians (and philosophers and scientists) are wont to operate. The formal expression seems very different from natural language sentences in many ways: it uses no recognizable words and no punctuation, and literate English-speakers cannot read or
understand it without special training. Part of the problem is that everyday language does not highlight its use of variables or quantification, and everyday speech almost never is used for statements that contain only variables and no names of individuals. But certainly part of the problem lies the notation, which has been abstracted from the notation used for natural languages over a long period of time. Yet Staal argues that we should see "(x) (∃y) (∃z) [R(x) → T (x,y,z)]" and "everyone in the room knows two languages" as extremes within the same continuum.

Perhaps a brief look at what Staal calls "scientific Sanskrit" will help clarify what he has in mind. Staal 1995, 79, defines his use of this term as follows: "I call scientific Sanskrit any Sanskrit used for the expression of scientific statements or truth". He contrasts this with "artificial Sanskrit" ("any artificial language intentionally created to deal with scientific problems . . . based upon, but deviating in some important respect or respects from ordinary, natural Sanskrit", Staal 1995, 78), but he also makes it clear that scientific Sanskrit is often written in artificial Sanskrit. Staal describes the general process of transforming ordinary Sanskrit into artificial Sanskrit in several places. As an example, the following three statements are all synonymous:

\[
\begin{align*}
ghaṭo nīlāḥ & \\
ghaṭāṣya nīlātvam & \\
ghaṭanīlātvam &
\end{align*}
\]

Two phenomena are responsible for turning the two words occurring in these sentences into the forms given in (1) - (3): first, sandhi, or the rules of external combination of sounds, a phenomenon that occurs naturally in ordinary Sanskrit (and other natural languages) and which is indicated by the scripts in which Sanskrit is generally written; second, nominalization, or the generation of a noun construction from another type of linguistic construction; and finally, nominal composition, or the combination of two or more nouns into one, complex noun. Nominalization and nominal composition also occur in ordinary Sanskrit, but their use is extended in scientific Sanskrit to a degree quite unusual in ordinary contexts.
The two words found in (1) - (3) are ghaṭa ("pot") and nila ("blue"). The -o and -aḥ endings in (1) are the result from the application of sandhi rules; the -asya ending in (2) is a normal inflectional ending, indicating possession just like the preposition "of" does in English. Other changes to these words are the result of nominalization or nominal composition.

In (2), the adjective "nila" is transformed into the noun "nilatvam" ("blueness") by the addition of the nominalizing ending -tvam. Both adjective and noun are used to talk about a certain quality (in this case, a color), but if we were to try to explain what the ending -tvam does to the stem nila-, we might say that the form "nilatvam" names that quality. (3) contains the two stems, with a nominalizing ending attached to nila- just as in (2), but the stems are joined into one word: this joining of stems is nominal composition. This means that the inflectional ending -asya is not used, and similarly no rules of external sandhi (which are used at the junctures of separate, consecutive words) are applied.

While (1) - (3) are essentially synonymous, they can be translated in different ways depending on context. The most obvious sense is "the pot is blue", especially if they alone are used to form a sentence. In such a case, we would speak of the two words as forming a "nominal sentence". This means that it is a sentence (it has a predicate as well as a subject), but contains no verb. The alternative would be a "verbal sentence". "Ghaṭo nilaṣṭi" is a verbal sentence synonymous with (1) - (3); it contains the verb "asti" and is formed from "ghaṭo nilaḥ asti" through yet another application of the sandhi rules. (2) and (3) can also be translated "the blueness of the pot", and (1) "the blue pot". In such cases, they are treated as noun phrases, not clauses: in such a case, the word sequence is viewed as lacking a predicate.

The key advantage to (3) is that the fact that the pot is blue (or the bluish quality of the pot) can be talked about in a more efficient way than with (1) or (2). It can be embedded with ease in a larger sentence. Thus, the form ghaṭanilatvat (the ablative form of the noun) can be translated causally "because the pot is blue" or, with the adverbial phrase "ve api" added, concessively as "even though the pot is blue" (Staal 1995, 82). Actually, any of (1) - (3) can be used to get this meaning, but the further
nominalization is extended, the further scientific Sanskrit gets away from ordinary forms and the more artificial it becomes. The more artificial it becomes, in turn, the more precise and unambiguous it becomes (artificial Sanskrit gives up multivalency in exchange for precision), and the easier it becomes to identify the logic of an argument presented. This is the key to Staal's analysis of scientific Sanskrit: as its artificiality increases, it comes more formal and exhibits more of the surface features of formal languages like symbolic logic.

This general phenomenon, namely the use of a natural language in a way rarely if ever encountered in normal discourse for the expression of highly precise ideas, is in fact a normal function served by natural language: this is why it is best to view natural and artificial languages as extremes of a continuum. Staal points to the scientific Latin of Isaac Newton for an example more familiar to historians of Western thought. Thus, for example, Newton's second law of motion was originally formulated as follows: "mutatis motibus proportionalibus esse vi motrici impressae, et fieri secundum lineam rectam qua vis illa imprimitur" (cited in Staal 1995, 75), though it is more familiar in the form "f = ma". The Latin of this sentence would make little sense to a literate Latin speaker/reader who has not studied physics, because, first, it contains highly technical language, and second, it expresses in a very compact way the logic of the sentence through its sparse syntax. Newton's Latin may be thought of as an approximation to the artificial Sanskrit of the scientific śūtra texts.

Let me clarify one point that Staal does not directly address. A formation like "ghaṭanilatvam" is rather like the hybrid predicate "(In the room) (x)"; it contains natural language words, and hence explicitly includes its own interpretation within its "spelling", but it also contains formal features designed to illustrate the formal properties of the idea expressed with those words. Compare "R(x)" to "(In the room) (x)". The former, unlike the latter, contains no English words, and hence can only be interpreted once one is given a translation of the relevant predicate. "(x) (∃y) (∃z) [R(x) → T (x,y,z)]" is just as easily understood by monolingual Polish or Chinese speakers as by English speakers, whereas "(In the room) (x)" requires some knowledge of English. But if we wish to talk about logical form and only need a few constructions with which to provide examples, we can introduce a few vocabulary items from Sanskrit as though they were formal terms, and then we can incorporate our hybrid
predicates into a logical discussion in any language. Consider the two following sets of symbols and interpretations:

(I) Variables \{x, y, z\}
    Operators: \{\ldots \rightarrow \ldots\}
    Interpretation: \ldots B means "is blue" \ldots

(II) Variables: \{ghaṭa, pata\}
     Operators: \{\ldots t^9 \ldots\}
     Interpretation: \ldots nīlātva- means "is blue" \ldots

Statements in (II) will be somewhat longer (contain more characters) than statements in (I), but if the goal is simply to illustrate logical relations, rather than operate with them using an indefinitely long set of possible predicates, then (II) is nearly as efficient as (I), once the symbols are understood. In other words, scientific Sanskrit, to the extent that it sticks more closely to ordinary Sanskrit than the notation of formal logic sticks to European languages, will be correspondingly less artificial. But this does not affect Staal's overall point, namely that the same processes can be seen as operating in both Indian and Western artificial languages.

I should point out that Staal holds that "nominalization increases the descriptive power of language" (Staal 1965, 157). He does not support this claim, in my view, and it is probably better to simply say that nominalization makes for increased efficiency in expression. This should be sufficient to support the argument he makes in Staal 1995, to the effect that scientific advances often require certain advances in the ability to formalize complex expressions. Brilliant thinkers like Newton or Mādhava (Staal 1995, 119) might have been able to overcome the practical limitations that restrict most of us in conceptualizing abstract notions, but for the sciences to advance it is necessary that a means of expressing abstractions be easily available to the mass of educated people. Other advances, for example
in information technology, may also be important as well, but the ability to create and use artificial languages is, as a practical matter, necessary even if not sufficient.

In any case, nominalizations can effectively serve the purpose of generating an efficient artificial language based on a corresponding natural language (and hence allowing for ease of translation between the two). This is true whether the result is a nominal construction like "ghaṭanilatvam" or "R(x)".

Caveat. Though I present Staal as arguing that nominalization serves to transform adjectives denoting qualities into predicates that can range over variables, he has made it clear in Staal 1973b that this is not quite right. If we were to follow the classification given in Ganeri 2001’s outline history of the reception of Indian philosophy in the West, I have described Staal as though he subscribed to the "formalist interpretation of Indian logic", whereas in fact he was instrumental in pointing scholars towards the more sophisticated "property-location interpretation". Ganeri 2001, 20, concludes his review of Western scholarship on Indian logic as follows:

It is clear that a logic developed along these lines has no easy reduction to syllogistic or subject-predicate forms. It will have to develop its own techniques for the treatment of quantification, and the domain in which it has an application will not coincide with other logical theories, for it will be better suited for formalizing and solving some problems, and less for others. The task for a modern interpreter of Indian logic is to understand the distinctive problematic within which Indian logic developed, and to evaluate the theory within that context.

Staal, as I read him, agrees with Ganeri's methodological prescription up to a point. The choice that all comparative historians of thought must make is between an appreciation of each culture within its contingent context or the creation of an external model, which may be useful for comparison but to some extent will be unfair to all of the objects compared. Ganeri seems to support an emphasis on particular historical situations, whereas Staal tends more towards comparing cultures on the basis of some common standards. This does not mean that he is unfamiliar with the alternative, or does not take it seriously. The same problem occurs also in the comparative study of ritual, and Staal's approach to comparative logic and comparative science seems broadly similar to his approach to comparative ritual: emphasize similarities by creating descriptive models without losing sight of culturally-specific
differences. For the present, I will settle for this overly-simple description of a complex problem, and move on.

5.2.2. Compositionality in Language. I have argued previously that the key to understanding how Staal’s Meaninglessness Thesis and Syntax Thesis fit together is to be found in the assumption that ritual meaning, if it exists, must be generated compositionally. It is unfortunate that Staal has published relatively little on the problem of linguistic compositionality, since any detailed argumentation along these lines might shed light on his ritual theory. The one place where he makes more than passing reference to compositionality is in his review (Staal 1965) of Katz and Postal’s An Integrated Theory of Linguistic Descriptions. I will begin with a brief context for Staal’s comments in this review.

In language, we have strong intuitions about semantic data: about the meaning of individual words, sentences, and word sequences both shorter and longer than sentences. These intuitions constitute a wealth of data that can be mined by linguists, and it has always been the case that our intuitions about meaning far exceed the capacity of linguistic theories to account for the available data. Consequently, research in semantics has always lagged behind advances in phonology and syntax. In its difficulty in explaining meaning relations linguistics is somewhat like ritual theory, but unlike with language our intuitions regarding ritual meaning are confined mostly to the largest units of ritual action, or sometimes the smallest. In any case, the case for compositionality seems much harder to make for ritual than for language, even though it is not secure even in the latter case.

Thus, linguistic meaning at first blush seems compositional. If we replace one word with another, we almost always will see a corresponding change in meaning. A similar change occurs if we shift a word within a sentence, for example an adverb closer to or farther from the verb, or move a verb or direct object to the front of its sentence. But ironically the subtleties so readily apparent in linguistic meaning also present a problem for arguments in favor of compositionality, especially from the perspective of generative linguistics. Let us take a simple model of language of the type presumed in Staal’s treatment of ritual syntax, according to which component parts (the most important words and the functional relations between them) are combined into sentences by means of phrase structure rules,
and the result is then turned into a finished utterance by means of transformations (and phonological rules). Linguists realized shortly after this model was developed that a trade-off must be made between compositionality and the ability to explain the relationship between similar utterances.

Consider a now-traditional example, the active-passive transformation as applied to the sentence "everyone in the room knows two languages" (see Chomsky 1957, 100-101; Katz and Postal 1964, 72-74; Staal 1965, 145-146). It is not immediately clear that the passive version of this sentence, "two languages are known by everyone in the room", has the same meaning as its active counterpart. Chomsky argued that in the active version, there is no implication that everyone knows the same two languages, but this implication does follow from the passive version. If we translate these two meanings into a suitably constructed formal language, the distinction is quite clear:

\[
(x) (\exists y) (\exists z) \ [R(x) \rightarrow T(x,y,z)] \tag{4}
\]

\[
(\exists y) (\exists z) (x) \ [R(x) \rightarrow T(x,y,z)] \tag{5}
\]

But it is less clear that these meanings should be assigned to the two versions of the natural language sentence. The problem is partly that intuitions of native speakers may differ, and partly it is a function of the degree of semantic detail we wish to attribute to the sentences as a function of their syntax. We can argue that sentence meaning in all its subtlety is dependent not only on its syntactic and lexical properties, but also (for example) its pragmatic properties. In this case, context is relevant in determining whether active and passive correspond to meanings (4) and/or (5). Thus compositionality may be saved by being enervated --- or, depending on how we look at it, expanded by accounting also for pragmatic properties. We can also argue that the meanings of the active and passive differ, but that they are not transformations of the same sentence. In this case we might say that syntactic theory becomes enervated.

Since Staal 1965 is a review, it gives us only a limited idea of how Staal comes down on the problems relating to compositionality. As for the example I have cited, Staal does state that he agrees with the further argument of Katz and Postal (Staal 1965, 146) that the active version has the meaning
attributed by Chomsky to the passive version, thanks to a consideration of sentence embedding by means of relative clauses. He does allow, however, that the argument is ad hoc (he is swayed by his judgment of how the principle of simplicity should be applied to the theory, but he recognizes that this will not persuade everyone) and that critics are also on solid ground in rejecting it. This analysis is consistent with his earlier comments on compositionality, see Staal 1965, 137-138.

Once we know how someone comes down on compositionality in language, then we are well on our way to understanding how they will come down on ritual compositionality, assuming of course that the formal structure of ritual may securely be treated as syntactic. Unfortunately, I have not found a clear statement of exactly where Staal comes down on compositionality in either area, much less a definitive argument that will solve the problem once and for all in either area. A sophisticated treatment of ritual meaning is certainly needed, and not only to help us understand arguments in favor of ritual compositionality.

5.2.3. Meaning in Language. Let me therefore move to a consideration of Staal's study of meaning. I will focus on only two examples from his linguistic writings, chosen to emphasize both Staal's linguistic research and also problems in linguistic semantics that might be useful in considering the related problem of ritual meaning.

First, I will look at Staal 1973a, which provides an interesting approach to lexical meaning. This paper turns out to be rather disappointing, since Staal does not reach a definitive conclusion, but his discussion of the problem proves interesting in several respects. Let me begin with an example. Staal considers a type of semantic relation that cannot be accounted for in compositional terms.

Consider the sentence:

If John's birth preceded his mother's wedding, then her wedding followed his birth.  

Clearly the logic of the sentence hinges on the fact that the meanings of "precede" and "follow" are opposed to one another in a very particular fashion. If one thing follows another, then it is preceded by that thing. But a linguistic analysis of this sentence will generate the two verbs in entirely separate steps, and consequently will not be able to account for the relationship between them. The problem is
that the relation is not a logical one, but rather is purely semantic. It is possible, of course, to explain such phenomena by developing a separate theory of lexical meaning, but it is not obvious how the resulting semantic rules could be combined with syntactic and phonological rules to provide a unified linguistic description (or generation procedure) for the sentence.

However the problem is to be resolved, at some point one has to face the problem of lexical insertion, in other words how to get the particular words into the formal schema that the syntactic component produces. At this point Staal shifts his example to the insertion of nouns. Even a brief study of the distributional properties of nouns will show that they easily sort into classes based on their preferred environment. Mass and count nouns, for example, require different treatment as far as how they form plurals and take articles; some adjectives may only be used with nouns denoting animate subjects, or with certain genders; and so on. A common way of treating these phenomena is illustrated in (7) below (from Staal 1973a, 850). Phrase structure rules may be written so as to yield not just grammatical categories, but also additional subcategories that correspond to the groupings found in natural languages. These subcategories are indicated by information in square brackets (note that this particular example does not include any adjectives, but if it did, they would correspond to their respective nouns in subcategorical features).

![Diagram](attachment:image.png)

(7)
In order to adequately deal with the distributional structures found in noun phrases, quite a bit of subcategorization is required. Staal points to an argument in Chomsky 1965 to the effect that not all the relevant subcategorization information needs to be explicitly stated in the representation of the final phrase structures. For example, all units marked "+Human" are necessarily also "+Animate", so the former implies the latter, which therefore need not be explicitly given. But this is a breed of the same problem as we saw with verbs, Staal argues. The rule necessary to state the relationship between +Human and +Animate does not belong to any of the rule types otherwise given in the grammar: it is not a phrase structure rule, or a transformational rule. It is, he argues, a semantic rule, or what in philosophical circles is called a "meaning postulate".

A properly developed theory of language should include rules that govern all phenomena encountered in the language, and in principle syntactic and semantic rules should seamlessly fit together to generate the sentences of the language. But the syntax-semantics interface has always presented problems, as this example shows. Staal argues that it is an analytic truth that the entries of a lexicon are of the form:

\[ P(w) \text{ is } \ldots \]  

where \( P \) means "the meaning of". It is not clear to me exactly how Staal intends to reconcile this with the subsequent account he gives of lexical structure as given in Chomsky 1965. According to the latter view, a lexicon is an unordered list of items, each of which specifies the phonological properties of a word and its category and subcategory properties, for example:

\[ (Jack, (N, [-Common], [+Human])) \]  

Such a structure is clearly necessary in order to govern the application of lexical insertion rules, for example to allow "Jack" to occupy the first slot in the tree structure (7), but not the second. And it is not inconsistent with Staal's view of what it means to be a lexicon.

The entire entry (9) could replace the "w" of (8), for example. Then the lexicon would be a mapping of a semantic property onto a complex of phonological and semantic properties, rather than
simply a mapping onto a phonological sequence alone. While this might be odd, it is not clear that it would present a problem. However, Staal goes on to make another suggestion that might serve as an improvement over the traditional view. Rather than having the semantic component of a grammar be composed of a lexicon structured so as to allow for lexical insertion, perhaps it could be composed instead by an onomastikon, or a "conceptual dictionary" (Staal 1965, 859; see esp. n. 4). This would be an ordered list, in contrast to an unordered lexicon, in which words are grouped into semantic categories. Such a system would provide additional semantic information not contained in lexical entries themselves, implicitly in the system, and might allow the function of meaning postulates to be served without the need for a separate rule type to govern the relevant semantic relations. Staal does not, unfortunately, pursue this question at greater length, but it does bring his theory closer, in terms of the structure of his theory, to the cognitive theories that will be considered in section 5.3. It is in this regard that I find his proposal most interesting. It also serves to illustrate some of the approaches that might prove useful for the analysis of ritual meaning, were he not to argue so stringently for the meaninglessness of ritual.

Second, consider Staal 1974, 34 (in this case, Staal was a participant in a conference, and only part of the publication contains his words). Bar-Hillel has commented on the oft-quoted definition of linguistics as the study of the relationship between sound and meaning, adding that a more accurate characterization would be the relation between sound-in-context and meaning. Staal proposes that this be taken further, to the relation between sound-in-context and meaning-in-context. He then says:

There is some kind of feeling like the following: Sounds are clear. Meanings are mysterious. I do not think that this is the fact at all. I think this was the basic motivation behind structural linguistics, it is still extremely strong in all later forms of linguistics we are now dealing with. But there is absolutely no justification for that kind of feeling because it is a certain development of science or the fact that linguists were impressed by physics, which they misinterpreted for in fact physics had more theories and more abstraction than almost any other science maybe at the time and may be even now.

This is an extemporaneous comment, and certainly not as well phrased as Staal's writings normally are, but I have quoted the passage anyway because it is very interesting. Normally meaning is considered the most problematic part of linguistics: this is exactly what I indicated earlier, when I said
that it has lagged behind both phonology and syntax. But certainly this is partly a function of how we
divide linguistics into subparts, so if we had divided it differently at the outset of the development of
modern linguistics, semantics would seem better understood and syntax, perhaps, would seem more
mysterious in order to compensate. Our analysis of speech into a distinct sequence of sounds is far
from theoretically simple, and in fact it relies in part on a pretheoretical judgment about meaning: to
take an obvious example, we know that palatals and nasals are different sound types because "cat" and
"mat", etc., have different meanings; not all such judgments are so easily made. Such pretheoretical
judgments allow linguistics to get off the ground, and account for the generation of scientific-looking
formalisms. But the later result that meaning seems mysterious may be traced (at least in part) to the
fact that the initial judgments that got the process started were necessarily not principled ones, and thus
the laxness with which the procedure began eventually results in problems at a later stage.

Linguistics aims at scientific accuracy, on the assumption that this is the hallmark of all
sciences. But, Staal adds, not even physics, the exemplar of science, is free from pretheoretical
assumptions. And just as twentieth century physics has ended up with seeming paradoxes for which
there is no easy solution (in quantum theory, for example), so a similar phenomenon occurs in
linguistics. Staal does not conclude what many do today, however, namely that we can have no
confidence in the objective validity of any of the sciences. As section 5.1 made clear, he believes in the
promise of scientific knowledge, so long as we recognize that it suffers from problems that are endemic
to language, thanks to its expression in formalized language. But this brings us full-circle back to the
problems with which we began, and gives us Staal's version of the hermeneutic circle.

Conclusion: It is clear that Staal thought a great deal about linguistic meaning: this is well
attested in his publication record. It is also clear that he understood some of the problems that beset the
student of semantics, and the problems with providing a coherent account of the syntax-semantics
interface. In this light, his theory of ritual meaninglessness seems somewhat odd, as though it was an
attempt to avoid the problems that are unavoidable in the study of language precisely because they are
so challenging.
5.3. The Cognitive Science of Ritual. Staal’s theory of ritual is important, I would argue, in part because it pursues the language analogy further, and more convincingly, than perhaps any other theory of ritual. But despite this, Staal does not take the language analogy as far as he could. Having considered in section 5.1 Staal’s history and philosophy of science in the context of his theory of ritual, it is now reasonable to ask what a scientific theory of ritual would look like if it took the language analogy as seriously as it possibly could. This is the task of section 5.3.

5.3.1. Ritual Syntax As Psychology. Chapters 1 and 3 considered the formal analysis of ritual systems along the model provided by transformational-generative linguistics. But little has been said, except in passing, about the implementation of these properties in actual ritual agents. Now I will look at the way current linguistic theory views the implementation of linguistic rules, and then consider whether a similar story can be told with equal plausibility about ritual rules. For convenience I will loosely follow in this section the second chapter of Chomsky 1986, which is still the locus classicus for the problem at hand: all quotations in the current section (5.3.1) are from this book.

We can approach a theory of ritual (or a theory of anything, for that matter) in two ways. We can assume that there is something called "ritual", and we can inquire into the nature of that thing; or we can make a list of things that could plausibly be called "ritual", and select one to be the focus of our theory. In the latter case, we would treat "ritual" as a technical term, and we probably would develop other technical terms for the various things that we had rejected as candidates for the name "ritual". The former method has been popular with a wide variety of thinkers over the centuries: this is the method pursued by Socrates and Plato, for example, and more recently by the ordinary language philosophers. The latter method is the one commonly adopted in scientific circles, and it is the one that will be followed here.

Chomsky identifies two major approaches to language, and coins the terms I-language (internalized language) and E-language (externalized language) to denote the objects of study of these two approaches. The traditional approaches to linguistics --- such as those pursued by Saussure, Whitney, Bloomfield, and Sapir --- took a given language to be the set of all attested or potential utterances in a particular dialect\(^{11}\), and this is what Chomsky refers to as E-language, the language
which exists externally to language speakers. If we say it is external to the speaker, one could reasonably adopt Chomsky's spatial metaphor and ask where E-languages are. The answer is not so simple. If Chomsky was a Durkheimian along roughly the lines that I have sketched previously, he could say that E-language supervenes on individual utterances, or something along these lines. But he is too much of a physicalist to take this approach, and he rejects it as (in my words, not his) metaphysical mumbo jumbo. In Chomsky's words, "languages in this sense are not real-world objects but are artificial, somewhat arbitrary, and perhaps not very interesting constructs" (26).

If a Durkheimian approach is not acceptable, a Platonic approach fares no better (33); this can best be understood by comparing a natural language like English to a "language" like arithmetic. The facts of arithmetic are true, we might argue, independent of anyone knowing them, or in a world with no objects with which one could count. In this sense arithmetic might be true in some "Platonic heaven" over and above the phenomenal world. But it makes no sense to speak of a natural language that never was and never will be spoken by anyone. Certainly we could generate a system of rules for language generation --- we could in fact generate an infinite number of such systems, in principle --- but it is not clear how we could be justified in describing these as natural languages, if they stayed up in the Platonic heaven. To continue the spatial metaphor, natural languages have to be somewhere where we can get to them. The obvious place for this is in the mind-brains of individual speakers of the language. But in this case, it makes little sense to define "language" in terms of the technical term "E-language". After all, to do this would be to open up a whole series of questions that would not be easy to answer, among them the following: how does it come about that everyone has the same E-language in their mind-brain (the "learning problem")? how do we know that they do12 (the "epistemology problem")? under what conditions would we be warranted in saying, on the basis of finite evidence, that a Martian, with a silicon brain and radically different cognitive architecture than humans, was speaking English (the "identity theory problem"; we could also ask this about computers running speech-generation programs)? This is where I-language comes in.

We could alternatively define "language" as "I-language", namely some part of the mind-brain of the speaker. This solves the metaphysical problems of identifying language with E-language,
because we know exactly where I-language is: it's "in the head". We don't have identity theory problems, because the Martian and the computer can't have English as an I-language if their brains can't get in the right physical states, and they can have English as an I-language if they can. As for the epistemology problem, we know from the outset what the conditions are for a mind-brain to be in a certain state, and it becomes an empirical problem whether we can measure that state sufficiently to determine whether two people have the same I-language. The epistemology problem is only a problem when we are combining a formal description of a language with a physical description of the individual: once we remove the former, the problem becomes trivial.

Perhaps "trivial" is a bit of an overstatement. Even if we identify language with internal mental processes, we still need a way of relating those processes to the utterances that give I-language external form, and a means of relating both of these to the rule-systems developed by linguistics. The relationship between utterances and underlying rules of linguistics is a problem that has been long understood, and to the extent that it has not yet been fully solved, it poses no particular problems for a psychological approach to linguistics. As for the question of how to bring internal mental states into the picture, in practice we can get away with a lot of handwaving and the promise of future advances in neuroscience. We know the general structure of neuronal connections in the brain, and the processing that such an architecture requires seems pretty clearly to be "connectionist" rather than "symbolic": in other words, any given representation (like the lexical entry for the word "dog", or the rules governing the word order within English prepositional phrases) cannot be localized to a particular set of neurons, but rather is in some sense imminent in a much larger number of neurons, along with a large group of nominally distinct representations that are also localized in the same larger group of neurons. How a set of linear rules (rules that are written for sequential processing) is processed in this sort of highly distributed network is not clear, though it does seem that this is a hardware problem, as it were, and not one that is particularly important for our understanding of the unique features of linguistic or ritual thought.

The E- and I-language distinction is important, in other words, because it allows us to get away from a focus on language as a set of actual utterances and allows us to think of it in terms of the set of
all possible utterances that a language can generate, or the device that has such a set as its output. There
is of course a distinction between a language device and the complete set of its output, but as with many
other things I will ignore this distinction for the present. But despite my handwaving, my
characterization of the problem should be sufficient to emphasize one major point: there is a vital
distinction between conceiving of linguistics as the study of "a system of linguistic forms or events"
(19) and conceiving it as "some element of the mind of the person who knows the language" (22) in
question. The latter can be characterized in terms of its output, but to do so is not to identify the mental
mechanism with the output that it produces, it is merely to describe it in a certain way.

Combining this story with other claims that I will not defend here, Chomsky concludes that all
humans have a genetically-derived initial mind-brain ground state $S_0$, which is then modified by
experience (interaction with other language-users, and perhaps natural developmental processes) until
the mind-brain reaches a relative steady-state $S_5$, at which point the person can speak idiomatic English
(or whatever language the person has learned). According to Chomsky, all languages have a common
base (universal grammar, or UG), and the grammars of all natural languages are derived from this UG
by selecting certain values for a finite number of variable properties (think of customizing a webpage
by selecting options from a series of pull-down menus). In any case, the existence of UG and of
language universals generally is entirely distinct from the problem of how "language" is to be defined.
The key is that for Chomsky, and for most linguists subsequently, language is seen as an element of
psychology: the grammar of a language is a state of the mind-brain, and the language is in a real sense
physically contained within the individual. The grammars described in books, as formal systems
constrained by rules, are useful in describing language, but they are not identical to language unless the
rules that constitute such languages are physically instantiated in the mind-brain.

If we want to develop a theory of ritual in strict conformity with the language analogy, we
should apply the same arguments to rituals and ritual rules. In this case, rituals are not the things that
people do, but rather the representations within their mind-brains that result in ritual behavior, ceteris
paribus. It makes sense in this context to distinguish between ritual competence and ritual performance,
as Lawson and McCauley 1990 does, and it makes sense to individuate ritual action in terms of actual
performances of rituals (a subject on which I am writing elsewhere). Just as the distinction between E-language and I-language solves a number of problems in linguistics by a simple clarification in technical vocabulary (and a consequent tightening of the boundaries between different disciplines), so does a similar distinction work in the study of ritual.

5.3.2. Sperber's Cognitive Theory. If we do decide to treat ritual as an element of psychology, in keeping with the language analogy, perhaps we should also consider it in the light of the other sciences of the mind, which currently are conveniently packaged under the rubric of cognitive science. In fact, some of the most exciting research in recent religious studies has applied insights from the various cognitive sciences to the study of religious rituals. Though this research has only just begun to come together as a distinct subfield over the past decade, its roots can be traced back over a quarter of a century. The most important work in this area is undoubtedly Boyer 1994, to which I will devote significant space in what follows. Boyer is, in my opinion, the only scholar to develop a general theory of the cognitive science of religion, and ritual plays a significant role in his treatment of religion --- as it does also in Lawson and McCauley 1990, a book more restricted in scope than Boyer's but still quite interesting in its own right. The most recent, significant contributions to the field are McCauley and Lawson 2002 and Atran 2002. But as in Chapter 2, I will introduce my discussion of Boyer 1994 with a brief review of Dan Sperber's work.

A cognitive approach to ritual, then, will rely on methods and results from a number of different sciences in order to provide the most complete description of ritual taken as the capacity ("competence", as it is also known) to conceive of ritual actions within their proper ritual system. The relevant sciences would include natural sciences, history of religions, ethnology, theology, etc. --- in other words, "science" in the sense of "Wissenschaft". Roughly, the natural sciences would provide the means for describing the substratum of ritual thought, and the social and human sciences (i.e., all the other fields) would allow for the description of the facts as they are perceived by human agents. This need not entail intertheoretic reduction, since a cognitive science of ritual does not require, in principle, a fully-developed, unified scientific theory according to which statements containing predicates from one science can be reformulated in the language of another science, with no loss of content. It might
turn out that some processes can best be described phenomenologically, using tools from the history of religions or theology, whereas others can best be described in terms of underlying physical mechanisms, or perhaps computational mechanisms that are only imminent in the physical organization of the mind-brain.

A result of a cognitive approach to ritual, then, is that the number of topics available for study increases roughly as the number of tools at our disposal increases. This is because every additional science will bring its own list of problems, its own ontology, its own techniques, etc. And while sometimes it will be clear that two sciences are providing roughly equivalent descriptions of roughly equivalent phenomena, other times new sciences will bring with them the ability to consider problems that were simply not considered before. In cases where different sciences address the same or similar problem in different terms, it will be the case that we will have some choice in how we will describe a phenomenon, and thus our technical vocabulary will contain a number of near-synonyms.

One example is the relationship between beliefs and representations. In general, the human sciences will use "belief" in cases where the natural sciences will use "representation"; the social sciences may use either. It is not correct to say that these terms are synonymous, but it is fair to say that they are roughly coextensive: the difference is more nearly one of sense rather than reference. "Belief" in this context does not imply commitment to the truth or reality of an idea as it generally does in common parlance, but just that an idea is being considered or reflected on. "Belief", in short, may be used to refer to propositional attitudes generally, without specifying the particular attitude that is taken to the proposition. It can also be used of attitudes towards content that is not propositional in character or form, for example a name or an object. "Representation" can be used to refer to beliefs so understood when the context places more emphasis on the relation of the agent to the belief than on the content of the belief. It can also be used to refer to external objects that bear properties often associated with mental representations, for example statues or paintings. In fact, both words are highly multivalent, and the decision to use one or the other is contextual, and difficult in cases where they are largely synonymous. "Representation" as I use it does not imply, as it typically does in continental philosophy, a covert reference to a self for whom the representation presents something.
Before continuing, let me briefly review the argument of Sperber 1975. Reacting against traditional theories of social practices (especially that of Levi-Strauss) that treated symbolism as a code for the expression of hidden meanings, Sperber argued that symbolism is a particular type of system of representation. He proposed a tentative cognitive architecture that included a symbolic module, the purpose of which was to assign a provisional interpretation of sensory data that could not be adequately dealt with by the other main processing unit of the mind-brain, the "rational device". While the details of this mental architecture are probably not satisfactory\(^\text{17}\), Sperber's theory stands as the first serious attempt to apply methods and insights from the contemporary sciences of mind to traditional problems in the social sciences\(^\text{18,19}\). This theory is important because it does not treat symbolism as an abstract theory focused on systematizing our knowledge of the products of symbolic thought. Rather, it explains symbolism by positing a mental mechanism that accounts for not only the meanings traditionally associated with symbols, but also the variations in symbolic exegesis given by one individual over time, and by different individuals who possess different experiences. Sperber does for cultural symbolism what Chomsky does for language: he treats it as an I-system, and treats the symbolic process as an element of human psychology rather than as an abstract system existing in some "Platonic heaven" (see section 5.3.1 for my treatment of Chomsky).

Sperber usefully applies methods from other scientific fields to the study of social forms in his arguments for an epidemiology of representations. While this argument is presented in a number of places, notably Sperber 1990 and 1996, as with his theory of symbolism Sperber does not significantly develop his theory beyond what he achieves in its initial formulation.

**5.3.3. Boyer's Cognitive Theory.** The next major step beyond the contribution of Sperber is made by Boyer 1994. Boyer's approach assumes the mind is a product of macroevolution, designed for a certain type of environment and to the extent required by evolution optimized to this environment. He does not spend time hypothesizing about the environment responsible for the mental adaptations that account for human culture, but he does devote significant attention to research in cognitive psychology (his own area of field research) in a search for evidence regarding the mental processing of social forms. In particular, he focuses on four general classes (or "repertoires") of religious representations,
namely the representations of: religious ontologies, causal judgments, social categories, and ritual actions. He does not argue that these repertoires account for all religious ideas, nor does he argue that there are principled boundaries separating these into four distinct classes. Rather, he treats these classes of religious representations because he has useful things to say about them, and he finds this division into classes a useful one. While the theory is certainly not comprehensive, I have described it as a general theory because Boyer has accounted for a wider variety of religious representations than anyone else working in the cognitive science of religion.

In particular, Boyer treats ritual representations (the fourth repertoire) as a part of a larger system. This contrasts with the approach of, e.g., Lawson and McCauley, Staal, and Whitehouse, whose cognitive theories focus on particular aspects of ritual representation. Boyer also goes beyond Sperber in giving special treatment to different types of religious representation, rather than lumping them all together. My main goal in this section is to relate the ritual theories given by Staal and Boyer, but in order to do so I will have to summarize Boyer's theory. I addressed Boyer's treatment of meaning briefly in section 2.2.2, but in order to clarify Boyer's view of ritual, I will first have to consider his theory as a whole in some detail. Before discussing each of the four repertoires, I should say a few words about Boyer's approach, in particular his technical use of the word "natural".

Boyer does not seek to account for the subtleties of abstract theological doctrines, but rather for the intuitions people have before they learn complex theology. These intuitions are a product of evolutionary history, and can directly account for many widespread beliefs and practices among small-scale societies. These intuitions do not constitute absolute limits on our ability to conceive of the divine, however, and perhaps are less helpful in explaining conceptions of the divine prevalent among literate religious practitioners in complex societies. However, even if our inborn religious intuitions do not limit our ability to form religious representations, they often do continue to exert their power even on people who have undergone detailed theological study and who therefore ought to "know better" than to rely on their natural intuitions. In short, Boyer is interested in the natural endowment people have specifically for conceiving of religious topics, but he does not pretend to present a cognitive account for all the variety of types of religious thought.
First Repertoire. Ontology is the philosophical study of what exists, and in anthropology it is common to talk of ontologies in the plural to describe indigenous theories about what types of things exist. A religious ontology, therefore, is the set of beliefs a person has about what beings exist in the divine or supernatural world, with the caveats that many of these beliefs may be subconscious and that "belief" means "mental representation" and need not be propositional in form. Such ontologies constitute the first of Boyer's four repertoires. Boyer states his key thesis regarding religious ontologies quite concisely:

If religious assumptions can be seen to be constrained by intuitive ontologies, then this may account for the recurrence of certain particular features in the "supernatural furniture" imagined in many different human groups. (Boyer 1994, 91-92)

Boyer begins his examination of religious ontology with a summary of his own fieldwork among the Fang people of Cameroon, focusing on their beliefs in "ghosts". While I will not summarize the ethnographic details he discusses, I will point to three conclusions he draws from this case study.

First, representations of religious agents are founded on a set of natural "background" notions that are also utilized in conceiving of profane agents. A listing of Fang beliefs about ghosts leads one quickly to a hypothesis about the nature and source of this background knowledge. Some dead people become ghosts, and while they lose many of the physical properties they possessed in life, they tend to maintain the psychological states of humans. Thus,

> Ghosts are also described as intangible, in the vague sense that they can go through physical obstacles. Moreover, they are described as able to move extremely fast, though no one ever supposes that they could be in two places at once. (Boyer 1994, 93)

Clearly ghosts are thought of as humans in many regards, for example they have the same range of psychological states as well as some physical states in common with humans (they cannot be in two places at once). If we were to generalize from this example, we would say that religious objects and agents are thought of in mostly the same terms as mundane objects and agents, but with certain differences. In Boyer's terminology, "supernatural furniture" is represented in the mind in much the same way as "natural ontologies", with only a finite and small set of differences. In short, religious
representations depend partly on assumptions that people make "spontaneously", either because certain mental patterns are preprogrammed in the mind-brain, or else through normal developmental processes (much like naive notions of physics develop in infants, according to principles that are now well-understood). Partly they also depend on cultural forms of transmission, with the key proviso that representations transmitted specifically by cultural means form a small set as compared to the total set of representations that result from spontaneous and cultural methods in combination.

When we look at the ways in which cultural representations are formed, we see that we must deal with a version of the "learning problem" that we encountered earlier in the discussion of language acquisition: direct experience is not sufficient to allow Fang to acquire a common set of beliefs through induction alone, so there must be cognitive constraints that help guide Fang to particular notions about ghosts (and other religious or supernatural beings). These constraints determine the background knowledge shared by most Fang.

This view of natural and supernatural ontologies\textsuperscript{22} sets the model for the rest of Boyer's approach to cognition. Human evolution has bred in us certain cognitive constraints, that incline us to see the world in certain ways. The idea is certainly not new: Kant argued well over two centuries ago that our very perception of space and time is conditioned by the way our minds are structured, not just by objective facts about the world. Similarly, simplistic models of culture were developed in the nineteenth and twentieth centuries, according to which all societies were constrained by universal cultural laws, which tended to result in similar cultural products around the world. Boyer's view of religion is not dissimilar to such approaches, but it does have the advantage of being a bit more subtle. He does not merely look to surface similarities to identify cultural universals; but rather seeks to identify cultural constraints, which will only result in similar cultural products under similar environmental conditions. This is just the problem of "generative" versus "selective" models of culture, see Boyer 1994, 9-12: a generative model posits an underlying mechanism sufficient to result in a given product, whereas a selective model only gives the same output when a necessary but insufficient mechanism is combined with the proper type of environmental input.
Second, non-transmitted assumptions do not merely serve as a background for the more strictly counter-intuitive assumptions that underlie religious representations, they also "impose strong constraints" (Boyer 1994, 100) on those representations. To understand this we need to backtrack slightly. A recurrent theme in Boyer's work, which he takes from Sperber, is the similarity between individual learning processes and systems of cultural knowledge. The same factors that make a belief memorable to the individual tend to make it easily transmitted from generation to generation, for reasons that should be obvious. In some respects, generalizations about society tend to be explicable in terms of generalizations about people in aggregate, and the issue at hand is a perfect example of this fact. The more memorable a belief is to any random individual, the more people will tend to acquire it, and the more easily they will acquire it. Consequently, that belief will be distributed widely in a population in proportion to its memorableness.

"Memorableness", however, is a somewhat ambiguous term. Some beliefs are widespread because the human mind is constructed so as to encourage their acquisition, for example the belief that an inanimate object at rest will remain at rest when not observed, ceteris paribus --- this constitutes the naive analogue to Newton's first law of motion. Such beliefs do not need to be "remembered", because they are simply taken for granted, or assumed to be true. Memorable beliefs are those that are somewhat surprising, and hence cannot be simply assumed. Yet, Boyer argues, beliefs dependent on many, highly counterintuitive assumptions are unlikely to be remembered because of the memory load they require, and consequently they will tend not to be widely distributed among a population. The result is a hypothesis, empirical in nature and hence available for confirmation and extension, that most cultural representations will balance a majority of intuitive features, which need not be explicitly taught to be widely held, with a minority of counter-intuitive beliefs, which add salience and hence memorability to the core of intuitive beliefs. Much of the best work in the cognitive science of religion since 1994, in fact, has taken the form of attempts to demonstrate this hypothesis through fieldwork. Justin Barrett is particularly to be singled out; see Barrett 2002 for a summary of his research. See also McCauley and Lawson 2002 for an up-to-date discussion of these and related topics.
Third, as a corollary to his second conclusion, Boyer argues that the combination of intuitive assumptions and counterintuitive violations of those assumptions imposes constraints on the range of inferences that people might derive from particular instances of cultural knowledge. If a group of informants was asked to select from a list of possible reasons those reasons that might incline ancestral spirits to visit harm on the living, the informants would probably agree to a significant degree. After all, naive psychology is widely shared in any population, and if the spirits are viewed, as among the Fang, as psychologically similar to the living, then people are likely to agree on the reactions of ancestral spirits to different circumstances. However, if we were to describe to our informants a being that possessed a large number of highly counterintuitive features, gradually as the number of such features increased our informants would have an increasingly difficult time identifying likely inferences that could be drawn from claims about such a being. Thus, not only would representations of a being with many counterintuitive features be unlikely to be disseminated widely within a population, but if it were widely distributed (perhaps due to a major educational drive) most people would have trouble drawing inferences from their conceptions of that being. We might call this sort of trouble the "Sunday school problem".

Second repertoire. The next repertoire that Boyer considers is that of causal judgments. Note that he is not directly interested in naive theories of causation, in contrast to the first repertoire where he considered naive theories of biology and psychology. Rather, he is interested in the particular judgments made as to the cause of particular events. Again, the Fang provide his chief exemplum, but his theory is intended to be true of other societies as well, in proper anthropological fashion. The initial example he gives is the Fang belief that particular "cases of illness are caused by witches flying about on banana leaves and throwing poisoned darts at their victims" (Boyer 1994, 125). In other words, it is widely believed among the Fang that particular instances of disease can be explained by application of a particular explanatory framework having to do with witches. If we generalize the details away, the framework might be acceptable to a wider base of observers, for example if we say that "witchcraft", however it is defined, is responsible for illness. But the particular judgment that includes roles for banana leaves and poisoned darts might only be acceptable to Fang observers. Boyer considers such
explanatory models and asks what we can learn about the shared mental apparatus common to most humans.

He begins by considering the various sorts of explanation that have been given in the past for such judgments. I will mention only the key element of his argument. Boyer argues on the basis of previous research in cognitive psychology that people tend to think that natural kinds are individuated primarily by their causal powers, and not by observable features alone. If this is the case, then it should be possible to explain culturally-specific causal judgments on the basis of cultural differences in ontologies, and this is precisely what Boyer does. In the Fang example, witches are widely recognized by their banana leaves and poisoned darts, much like they are known by their broomsticks and pointed hats in the West. It is widely recognized that witches can cause disease, and consequently witches are frequently assumed as the cause of illnesses. Such an argument is not deductive; rather, it is abductive. Abductive arguments, while they do not follow logically from premises, are vital to the development of science, and just as they are useful in the sciences so they are useful in daily life.

Boyer concludes that religious causal assumptions, like ontological assumptions, are of two classes: those "derived from intuitive understandings of large ontological domains" (153), and those that violate such intuitive assumptions. And just as in the case of ontologies, intuitive understandings of causation impose constraints on the range of possible counterintuitive assumptions. Therefore the key to understanding the variety of causal explanations resorted to in different cultures lies in our understanding of the class of intuitive understandings of causation. In fact, though he treats this as a separate repertoire, his analysis of causal reasoning follows directly from the third conclusion in his analysis of the first repertoire.

Thus, the key to understanding Boyer's treatment of causation is to understand his treatment of natural kinds. The former follows from the latter with very little argument needed. The situation is far different, however, in the case of Boyer's third repertoire, namely social categories.

Third repertoire. Boyer's first repertoire accounted for notions of supernatural, religious agents and his second repertoire followed from his analysis of the first. The third repertoire covers the categories of mortal, religious agents, for example PRIEST or DIVINER. Most often, these are
assumed to be socially-recognized classes, and each society has a set of criteria that determine class
inclusion. For example, a priest might have to belong to a certain bloodline, or undergo particular
instruction or initiation procedures. The historical and ethnographic literature is certainly full of
examples of the kind, and Boyer does not argue that there is a systemic error in traditional scholarship.
Rather, he is open to the possibility that people represent to themselves the criteria for class-inclusion in
a way different from whatever they might say they believe in response to explicit questions as to what
makes someone a priest or diviner. In particular, Boyer argues that representations of social categories
may include essentialistic assumptions much like natural kind representations do: social categories, in
short, tend to be naturalized, and this accounts for the unsystematic regularities that can be observed
across cultures, in accord with a selectional model of cognition.

This is perhaps most clear in the case of those small-scale societies that do not have the means
to provide detailed theological curriculum or expensive rites of initiation. Such societies tend to
recognize classes, for example SHAMAN, that rely more on personal attributes that may not be directly
observable. But his model applies equally well to categories within larger, historically attested
traditions that present explicit criteria for class inclusion. Parishoners are liable to complain that a
priest is not very priestly, without implying that his investiture was somehow defectively performed.
Social classes tend to be conceived of in such a way that certain properties (often ones that are not
directly perceptible) tend to be attributed to members of those classes, and clear violations of these
assumptions are liable to cause great consternation within a social group.

Boyer does not argue that essentialism plays as significant a role in social categories as it does
in biological categories. In fact, after considering empirical research regarding the cognition of two
sorts of social categories by children, those of familial relationship and racial identity, he concludes that
"social cognition constitutes a conceptual motley in which diverse sets of categories are acquired and
represented in very different ways" (Boyer 1994, pp. 176-7). This is true of religious social categories
as well, so his argument is not that the actions of human religious agents are understood only in
essentialistic terms, but rather that essentialistic reasoning can come into play in such cases. As with
biological kinds, inferences based on inductive generalization rely on pre-inductive categories, if they are to be drawn at all.

An important conclusion is that non-supernatural, social classes are not the object of a particular cognitive domain. Sometimes natural kind assumptions are marshalled in reasoning about religious agents, and sometimes other assumptions or inductive generalizations are relied on instead. And just as social cognition is less dependent on a particular cognitive domain than biological categories, so is ritual cognition less dependent still. With this I turn to the fourth and final repertoire.

5.3.4. Boyer and Staal on Ritual Representation. Boyer's treatment of ritual is best understood in the context of his larger cognitive theory, and this is why I have spent so much time on the first three repertoires. Ritual is cognized more like social classes and less like ontologies or causal relations, in two regards: first, it is not the object of a particular domain of knowledge, and second, essentialistic reasoning plays only a small part in ritual cognition. Ritual does, though, present unique problems (which are familiar to everyone working in ritual studies), and Boyer's treatment of ritual is by far the most tentative of the four domains.

Boyer structures his discussion of ritual around three key questions (19726):

(1) Which assumptions are activated in the representation and performance of ritual sequences?

(2) How are they connected to the representation of the ritual sequence?

(3) Do certain recurrent features of ritual sequences impose constraints on the range of assumptions activated, and on the way they are activated?

While I will not follow this structure exactly in my review of his argument, it is worthwhile to note at the outset the kinds issues that he takes to be most important. As with the other repertoires, Boyer shows an interest in the background assumptions underlying ritual cognition, and he is interested in the constraints that govern the activation of these assumptions. In addition, he focuses attention on the sequencing of ritual actions, a point that will become important shortly when we compare Boyer's
theory to Staal's. Before looking at these particular issues, however, I wish to begin with Boyer's take on the problem of meaning, which I discussed already in Chapter 2.

He critiques the method commonly followed by anthropologists and historians, who attribute beliefs to cultural actors, and more generally to the larger cultures to which individuals belong. According to the traditional approach, the meaning of a ritual is determined by certain of the beliefs entertained regarding that ritual. Boyer argues instead that "the question of the religious 'meaning' or significance of rituals can be reformulated as the question of the type of connections established by the participants between their representation of the ritual sequence on the one hand, and other types of religious assumptions (notably ontologies, causal assumptions, social categories) on the other" (211). Since the representations of other religious assumptions have been dealt with earlier in his book, the present problem is to give an adequate account of the representation of ritual sequences.

As I pointed out in Chapter 2, it seems that the representations of ritual sequences for Boyer are not quite analogous to the syntactic descriptions of ritual that Staal gives, because the former also include mappings onto the ritual meanings implicit in whatever background conditions are activated. In any case, the representations of action sequences are clearly distinguished from the mappings of these sequences onto background assumptions, so it is perhaps worthwhile to look at these in more detail.

Boyer seems to alternate between two ways of thinking of ritual action sequences. He begins by considering them as a species of action sequences more generally. Basing his argument on the cognitive science analysis of actions in terms of scripts, he argues that action sequences have "intentional structure". Take as an example an action like GOING TO THE DOCTOR. We tend to think of such actions as being composed of prototypical subparts, like SIGNING IN AT THE FRONT DESK, READING MAGAZINES, WAITING IN THE LITTLE ROOM, MEETING WITH THE DOCTOR, and PAYING THE BILL. Each of these subparts, along with the action as a whole, is individuated in terms of the end it serves. How we pay the bill might vary from instance to instance, for example last time perhaps I paid by cash, but this time I use a check because I do not have enough cash with me. But we tend to remember past visits to the doctor in terms of this prototypical script, and we remember particular instances from particular visits most often when they involved departures from

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our mental script, for example the time I couldn't read any magazines because the person next to me kept talking to me. And the sequence as a whole is identified in terms of its main component: it is called GOING TO THE DOCTOR rather than VISITING A WAITING ROOM because the main purpose is the subpart in which I am actually MEETING WITH THE DOCTOR. In short, each part, large or small, has a purpose, and this purpose not only gives a name to the part, it actually is the fact that gives the part its identity as a distinct action in its own right (individuates it).

Ritual actions, however, do not work this way. It is because ritual actions are not individuated in terms of the ends they serve that the parts of rituals are more restrictively governed than pragmatic actions. If we do not see the end that a particular action serves, we only know we are doing it correctly if we focus on correct execution, on means rather than on end. Boyer recognizes this, as does Staal. Boyer concludes that rituals are underspecified in their intentional properties. Unfortunately, he phrases this slightly differently, in a way that I think may be somewhat misleading. Boyer says that rituals have "underspecified intentional structure", which implies that ritual actions are structured, like pragmatic action, in intentional terms. I think it is better to say that they are underspecified in their intentional properties, but fully specified in their distributional structures, since this captures the idea that ritual representations are largely the representation of action sequences.

In any case, they are also underspecified in terms of their relevant background conditions. Here Boyer's approach is the same as Sperber's. Because the meaning of a ritual, such as it is, can be identified just with the background conditions that are activated by the ritual, and since as non-pragmatic actions rituals do not have a clear intentional structure, there is no determine set of background conditions associated with any given ritual. Different people will associate different background conditions with different rituals, depending on their previous cultural experiences.

Such, then, is Boyer's treatment of ritual. Inasmuch as it build on his treatment of the other three repertoires, it goes a long way towards fleshing out the notion of ritual meaning. However, while the sequential structure of rituals (what I call their "distributional" structure) is the only aspect of ritual representations that is not underspecified, Boyer says almost nothing about it. He focuses attention on intentional structure, which ritual actions do not have (or at least have to a much lesser extent than
pragmatic actions do), and on background conditions, which are not associated with rituals in any fixed
or "necessary" way. But he says almost nothing about the sequencing of ritual actions.

Staal's theory has just the opposite focus. While Staal and Boyer agree to a great extent in
their analysis of ritual meaning (they both argue that there is no strict relation between one ritual
sequence and one meaning), they describe ritual meaning in very different terms, Staal going so far as
to reject any talk of ritual meaning. He does, however, have a great deal to say about the structure of
ritual actions. Perhaps the best way to approach ritual, at least from a cognitive perspective, is to
combine the strengths of both theories: the result would provide an account of both ritual structure and
ritual meaning. It seems that even Boyer does not recognize the potential of such a combination, but I
think it holds great promise. I think that Staal's theory, if pursued along its natural course as a theory of
ritual cognition, would lead us precisely to this point.

1 Curious because he views himself as primarily a philosopher: "I might be described as a philosopher
who has long been exposed to India", Staal 1982b, v.
2 When I make short quotations from this section of these works in the paragraphs that follow this list, I
will not provide exact page references to each of these works.
3 There are of course a variety of fields that study literature, notably comparative literature and literary
criticism. There are also fields that are relevant to literary study, though not restricted to works of
literature, such as linguistics and philology. The latter fields merit more serious consideration as
sciences than the former do.
4 Both trends have gone on at the same time because they have taken hold of different segments of the
academy. Clinical medicine has been under pressure from interests as diverse as Big Pharma and
patients' advocacy groups to determine clinical definitions of disorders in order to facilitate the
financing of treatment regimens, whereas Foucault-inspired critiques of the very foundations of such
trends have been limited mostly to the humanities and social sciences, and to segments of the history
and philosophy of science.
5 For a good discussion of mechanism in the study of AIDS, see Steel 2002.
6 On this point, see for example Chomsky (1986, p. 27): "Linguistics will be incorporated within the
natural sciences insofar as mechanisms are discovered that have the properties revealed in these more
abstract studies [of the functional properties of l-languages] ..."
7 Sometimes for didactic purposes one will see a formulation in mixed formal and natural language, for
example "(x) (3y) (3z) [(In the room) (x) \rightarrow (Knows the language) (x,y,z)]".
8 Staal emphasizes that these formal features, for example case endings to express different types of
relationship, are derived from usage in the ordinary form of the language, but also abstracted from the
range of possible interpretations for each case to select the most logically appropriate meanings only.
9 Of course, "because" and its synonyms in other languages are typically used in a wide variety of ways,
and even in philosophical English "because" may only rarely be translated as \rightarrow. But the above
formulations should be sufficient for the current purposes, which are only intended to illustrate the
issues at hand.
This contrast with the European tradition partly results from the fact that I am comparing ancient Indian thought to modern European thought. If we compare the Indian scholars to, for example, Aristotle, we will find the same mix of natural and artificial expression in both.

"Dialect" is a poor word to use here. I would like to say "took a given language to be the set of all extent or possible utterances in a particular language", but that would be an ambiguous formulation. The problem, of course, is that "language" should not be defined in terms of "language". We have, in an ironic twist, a paucity of words for use in defining "language". In any case, I am trying here to introduce as few technical terms as necessary, to limit our need for definitions, so I will for the moment hold off introducing terms like "grammar" that might ameliorate the problem.

This is a version of the argument that Chomsky attributes to Quine (20, 30).

This is a radical simplification of a complex problem, since it glosses over the question of whether an I-language can be defined solely in terms of physical predicates, or must make use of functional predicates. But since I am restricting myself to Chomsky's discussion, I will ignore this problem for the present.

In other words, treat ritual as an element of the mind and the study of ritual as an element of the field of psychology.

Let me note a few other ways where my terminology differs from that most often found in discussions of the cognitive sciences; I trust the reasons for these departures will be obvious without further discussion. First, I will use "cognitive sciences" so as to incorporate also the fields generally known as "cognitive neurosciences". Second, I will count fields like ethnology and theology in the same manner as linguistics is typically listed as a cognitive science. Finally, I tend to think of philosophy more as a metatheory for talking about the specific sciences than as a separate component of the cognitive sciences.

This is again in contrast to a reductionist approach to ritual, since in the latter the goal is (more or less) to find a means of describing the most phenomena with the fewest means.

Unfortunately, while the general cognitive approach begun by Sperber has sprouted in the last decade, few scholars have attempted to seriously critique this particular theory of mental architecture; Toren 1983 is one of the few exceptions.

Earlier attempts at modeling cognitive architecture had, of course, been made previously by scholars such as Aristotle, Descartes, and Freud.

While I emphasize the role of a modern cognitive architecture in Sperber's theory, Boyer (1994, 58) emphasizes the similarly important role of memory search mechanisms.

These are slight variations on the key terms he himself uses.

In the style of argument pursued here, "belief" and "representation" are essentially synonymous. The former is typically used in contexts relating to the content of beliefs or representation, the latter in contexts relating to their role and place in the mental apparatus. As a general rule, "belief" occurs in social science contexts and "representation" in natural science contexts.

To review: "natural" and "supernatural" are not antonyms in Boyer's terminology. Rather, supernatural conceptions are based on both more basic conceptions founded on natural presumptions, and on strategic violations of natural presumptions (on this last conjunct, see the second conclusion immediately below).

This example is a variation on those given by Boyer.

This is a belief that, he proposes, would not be accepted at face value by most Westerners, and which would equally well be rejected by most Chinese, Amazonian, or Melanesian observers.

This conclusion that most people reach through natural developmental processes is also argued for on principled grounds by prominent philosophers, including Sydney Shoemaker and Peter Geach (see 145, n. 11) in order to distinguish between genuine and "Cambridge" properties.

All page references in this section are to Boyer 1994.
Conclusion

This dissertation has covered a wide range of topics, all relating in one way or another to the details of Staal's theory and the implications and prospects for future research in the general area of syntactic approaches to ritual. I will not summarize all of the these topics. Instead, I will identify only the chief topics that I have addressed, and I will consider the implications of my work for future research.

My analysis of Staal's theory of ritual as being composed of three distinct but interrelated theses and my analysis of Staal's treatment of meaning are probably the most accessible parts of the dissertation, and I hope that future works on ritual theory will take these analyses into account when they discuss Staal's theory. The same should apply as well to my more general treatment of ritual meaning, especially in Chapter 2, though in this I have attempted to follow the analysis presented by Pascal Boyer. If Boyer has not been entirely successful in convincing scholars of religious ritual to be more careful in their "mad" attempts to attribute meanings willy-nilly to ritual actions, then perhaps I can add my voice to this call for precision in meaning attribution.

My position, of course, is that the most important element of Staal's theory is the syntax thesis, and I feel that my discussion of this part of the theory is the best part of the dissertation. In Chapters 1 and 3, I illustrate some of the ways in which Staal's presentation of this thesis is misleading. I point out that when he professes to offer an actual rule within the Vedic system, Staal often presents instead the general form of a rule. Furthermore, he does not succeed in demonstrating that the Vedic system is structured by phrase structure rules and transformations: rather, he illustrates what a proper demonstration would look like, and he gives his readers some reasons to think that a more detailed study, along the lines he indicates, would be worth undertaking. Unfortunately, he makes it appear that he has in fact proven his case, and thereby dissuaded other South Asianists from building on his work at the same time that he has convinced the rest of the Religious Studies community, through the carelessness of his writing style and his unwillingness to follow his 1979 articles with substantially more detailed studies, that his theory does not deserve to be taken seriously. Since the theory is both
good and important, and his own contributions (while being insufficient) constitute an excellent start, it is particularly unfortunate that his approach has resulted in the marginalization of the syntactic study of ritual.

The fact that Staal is very careless in explaining what he is doing should not be a crucial problem once it is recognized. Dr. Mitch Marcus has pointed out to me that this type of presentation is a common practice among linguists: they present partial analyses or rule types without pointing out that they are doing so. But professional linguists write for other linguists, and their readers generally fill in the blanks, often without even noticing that they are doing so. Staal is in the position of a linguist writing for people in Religious Studies, however, and few of his readers have a sufficient grasp of syntactic theory to fill in the blanks and read Staal’s works as they were meant to be read. Since I emphasize in my dissertation how much Staal’s contributions to ritual theory follow Chomsky’s contributions to the study of language, I should also point out that the same carelessness of exposition is found in both. I suspect that both scholars are sometimes having trouble slowing down for the rest of us, because they both are brilliant and consequently perhaps a little impatient. But a good theory needs a good reception in order to influence its field positively. Chomsky has had many able interpreters, who in some cases have even helped move theory along by pointing to problems with the original formulation of issues, and I hope that, as the first serious interpreter of Staal’s theory, I will similarly be the first of many and in the process help to advance the theory itself, in addition to clarifying it.

My own attempts to present a formal analysis of parts of the Vedic system are similar to Staal’s. I have not undertaken a thorough study of all the evidence available, but rather looked for particular segments of the tradition that serve to illustrate the points I wished to make. My hope is that my presentation is at least more clear as to what I am doing: when I present the form of a rule, I make that clear, and when I simplify the structure of a rite in order to emphasize one part of its structure, I say so. I have pointed to aspects of the structure of Vedic ritual that Staal did not, and it is up to Vedic specialists to assess the value of my analyses of these parts of the tradition. Even if they should prove to be unsupported by a careful analysis of the rituals themselves, I believe they are successful in illustrating the points I wished to make by introducing them. I also have considered various arguments
that Staal made with regard to the larger South Asian tradition, for example his consideration of metarules or his view of the significance of the sattras. Even if my formal analyses of the rituals themselves should prove unsatisfactory, I believe these other interpretations of the South Asian tradition are likely to be accurate. But I am eager for specialists to consider these problems in greater detail, and correct any mistakes I may have made.

In retrospect, I think it is best to describe the syntax thesis as a hypothesis. Staal has not proven his case, but he has made a valuable contribution by identifying an important topic for further research, and he has gone a good way towards showing the rest of us how to proceed next. Throughout the dissertation I have periodically encouraged others to present what I call "distributional" studies of ritual, and despite one small section where I attempt to clarify what I mean by this term, I have not considered this topic at length anywhere. This has to do with the structure of my dissertation: by focusing on Staal's theory, I have not left myself much room for systematic theorizing. Future publications will fill this gap. But let me say a few words here, to tide readers over until I can provide a more comprehensive treatment.

I do not believe that we can rely on the analysis of the Vedic ritualists as being a fully adequate, scientific analysis of the formal structure of Vedic ritual. Earlier scholars have collated the evidence presented by these ancient ritualists (in particular I should point to Caland, Dandekar, Dumont, Heesterman, Hillebrandt, Kane, Kashikar, Schwab, and Staal himself in *Agni*), and it seems to me that, in his actions, Staal is trying to continue this tradition of writing modern paddhatis rather than taking the next step, which is his explicit goal, of developing a truly scientific analysis of Vedic ritual. Clearly Staal sees no contradiction, because he thinks the Vedic ritual manuals are constructed on strictly scientific grounds, but I have presented what I hope is a strong case that this assumption of his should be revisited rather than taken for granted. It seems to me he is simply wrong here.

This means that the first step in the process of developing a satisfactory grammar of Vedic ritual is what I have called a distributional study of the tradition. We need to generate a listing of all the rituals within the Vedic tradition, and a sequential listing of all the actions that constitute each ritual. According to the linguistic analogy, these listings would collectively constitute the corpus on which
subsequent analysis would be based. This step could, perhaps, be skipped, and theories about the
proper grammar of the ritual could be proposed as Staal and I have done, namely in an unsystematic
fashion. But my intuition is that it is better at this point to do the initial spade work to generate
complete distributional descriptions, and therefore this is my recommendation. Vedic specialists should
be able to do this, building on ancient and modern paddhatis, fairly efficiently and quickly if there is
agreement that this is a necessary next step. In any case, the same kind of distributional studies should
also be made of other ritual cultures.

After this spade work is completed, I do not know whether the next step should be
distributional analysis in the strict sense of the term (i.e., making use of IC analysis). While the
distributional descriptions are being generated, more theoretically-inclined scholars of ritual can mine
the extensive work that has been done already in linguistics and propose tentative analytical procedures
and formal models that are liable to be most successfully transferred from language to ritual study.

In addition to my study of the syntax theory itself (both Staal's version of it and my views on
the future of such theories), I also have considered periodically throughout my dissertation the impact
that such work ought to have on the cognitive study of ritual. The end of Chapter 5 addresses this
problem directly. This is important because it puts the syntax theory in a larger context, and it also
advances the cognitive study of religion in an important way, by opening up a new area of inquiry in a
field that is, in my opinion, currently beginning to stagnate (perhaps "mature" would be a less
intemperate word) even though it is still in its infancy. Important work has been done in the cognitive
study of ritual, but the most recent publications are revisiting topics that already have been well covered
rather than attempting to find new topics and new methods.

There is, of course, the good possibility that other researchers are, like me, exploring other
areas and will be publishing on them in the next few years. I can only judge by the publications now
out and the research I know is being conducted. I should also add that my comments about "stagnation"
should not be viewed as applying to work in the cognitive neuroscience of religion, but only to the
cognitive science of religion, which at present mostly means the cognitive psychology of religion. I am
currently beginning work on a more comprehensive overview of the state of research in the cognitive study of religion, and hope to have it finished in the near future.

It became clear as soon as I began researching this project that most of the descriptions of rituals that we have, across the spectrum of cultures, are not documented in a way that allows us to produce a proper syntactic study. A distributional description of the Vedic ritual system should be possible, because we have such full documentation in the ancient sūtra texts, and while it would probably not be possible to do this for most ancient ritual traditions, there is no reason why ethnographers shouldn't be able to collect the data necessary to produce distributional descriptions of the rituals they witness. Fieldworkers are also needed to help us understand the extent that practicing ritualists can judge the well-formedness of ritual performances, and this can only be learned through the study of living traditions.
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