ANCIENT LOGIC
AND ITS MODERN INTERPRETATIONS

SYNTHESE HISTORICAL LIBRARY

TEXTS AND STUDIES IN THE HISTORY OF

LOGIC AND PHILOSOPHY

VOLUME 9

ANCIENT LOGIC AND ITS MODERN INTERPRETATIONS

PROCEEDINGS OF THE BUFFALO SYMPOSIUM
ON MODERNIST INTERPRETATIONS OF ANCIENT LOGIC,
21 AND 22 APRIL, 1972

Edited by

JOHN CORCORAN
State University of New York at Buffalo

D. REIDEL PUBLISHING COMPANY
DORDRECHT-HOLLAND / BOSTON-U.S.A.

Library of Congress Catalog Card Number 73–88589

ISBN-13: 978-94-010-2132-6 e-ISBN-13: 978-94-010-2130-2
DOI: 10.1007/978-94-010-2130-2

Published by D. Reidel Publishing Company,
P.O. Box 17, Dordrecht-Holland

Sold and distributed in the U.S.A., Canada, and Mexico
by D. Reidel Publishing Company, Inc.
306 Dartmouth Street, Boston,
Mass. 02116, U.S.A.

Softcover reprint of the hardcover 1st edition 1974

to Lynn
with love

TABLE OF CONTENTS

PART FIVE

Final Session of the Symposium

PREFACE

During the last half century there has been revolutionary progress in logic and in logic-related areas such as linguistics. Historical knowledge of the origins of these subjects has also increased significantly. Thus, it would seem that the problem of determining the extent to which ancient logical and linguistic theories admit of accurate interpretation in modern terms is now ripe for investigation.

The purpose of the symposium was to gather logicians, philosophers, linguists, mathematicians and philologists to present research results bearing on the above problem with emphasis on logic. Presentations and discussions at the symposium focused themselves into five areas: ancient semantics, modern research in ancient logic, Aristotle's logic, Stoic logic, and directions for future research in ancient logic and logic-related areas.

Seven of the papers which appear below were originally presented at the symposium. In every case, discussion at the symposium led to revisions, in some cases to extensive revisions. The editor suggested still further revisions, but in every case the author was the final judge of the work that appears under his name.

In addition to the seven presented papers, there are four other items included here. Two of them are papers which originated in discussions following presentations. Zirin's contribution is based on comments he made following Kretzmann's presentation. My 'Remarks on Stoic Deduction' is based on the discussion which followed Gould's paper. A third item contains remarks that I prepared in advance and read at the opening of the panel discussion which was held at the end of the symposium. The panel discussion was tape-recorded and the transcript proved of sufficient quality to merit inclusion in these proceedings with a minimum of editing.

Funds for the symposium were provided by a grant to the Philosophy Department of the State University of New York at Buffalo from the University's Institutional Funds Committee. Departments of Mathematics, Classics and Linguistics cooperated in the planning and in the symposium itself. Professors Richard Vesley (Mathematics), Ronald Zirin (Classics), Madeleine Mathiot (Linguistics) and Wolfgang Wölck (Linguistics) deserve thanks, as do the following Professors of Philosophy: William Parry, John Kearns, and John Glanville. Special thanks goes to Professor Peter Hare who conceived of the idea for the symposium, aided in obtaining funds for it, and gave help in many other ways as well.

David Levin, Terry Nutter, Keith Ickes, William Yoder, Susan Wood, Sulé Elkatip, and Alan Soble, all students in Philosophy, aided in various ways. Levin was especially conscientious and generous with his time.

JOHN CORCORAN

Buffalo, N.Y., November 1972

PART ONE

ANCIENT SEMANTICS

NORMAN KRETZMANN

ARISTOTLE ON SPOKEN SOUND SIGNIFICANT BY CONVENTION

A few sentences near the beginning of *De interpretatione* (16a3–8) constitute the most influential text in the history of semantics. The text is highly compressed, and many translations, including the Latin translation in which it had its greatest influence, have obscured at least one interesting feature of it. In this paper I develop an interpretation that depends on taking seriously some details that have been neglected in the countless discussions of this text.

The sentence with which *De interpretatione* begins, and which immediately precedes the text I want to examine, provides (as Ackrill remarks[1]) the program for Chapters 2–6.

> ... we must settle what a name is [Chapter 2] and what a verb is [Chapter 3], and then what a negation [Chapters 5 and 6], an affirmation [Chapters 5 and 6], a statement [Chapters 4 and 5] and a sentence [Chapters 4 and 5] are. (16a1–2)[2]

But Aristotle says "*First* we must settle what a name is ...", and that is what he does in Chapter 2. The remainder of Chapter 1, then, may be thought of as preparatory to the main business of those chapters. And since their main business is to establish definitions, it is only natural to preface them with a discussion of the defining terms. At the beginning of Chapter 2, for instance, Aristotle defines 'name' in these terms: 'spoken sound', 'significant by convention', 'time', and 'parts significant in separation'. These terms continue to serve as defining terms beyond Chapter 2, and the remainder of Chapter 1 (16a3–18) is devoted to clarifying them. The special task of the text I am primarily concerned with is the clarification of the proximate genus for the definitions in Chapters 2–6: "spoken sound significant by convention".[3]

I

Ἔστι μὲν οὖν τὰ ἐν τῇ φωνῇ τῶν ἐν τῇ ψυχῇ παθημάτων σύμβολα,	Now spoken sounds are symbols of affections in the soul, and writ-

J. Corcoran (ed.), Ancient Logic and Its Modern Interpretations, 3–21. *All Rights Reserved*

καὶ τὰ γραφόμενα τῶν ἐν τῇ φωνῇ. καὶ ὥσπερ οὐδὲ γράμματα πᾶσι τὰ αὐτά, οὐδὲ φωναὶ αἱ αὐταί· ὧν μέντοι ταῦτα σημεῖα πρώτως,[4] ταὐτὰ πᾶσι παθήματα τῆς ψυχῆς, καὶ ὧν ταῦτα ὁμοιώμτα πράγματα ἤδη ταὐτά.

(16a3–8)

ten marks symbols of spoken sounds. And just as written marks are not the same for all men, neither are spoken sounds. But what these are in the first place signs of – affections of the soul – are the same for all; and what these affections are likenesses of – actual things – are also the same.

Ignoring the claims about sameness or difference to begin with, we can pick out four elements and three relations. (I am going to use 'mental impression' only because it is handier than Ackrill's 'affection in [or of] the soul'.)

Elements	*Relations*
actual thing	is a likeness of
mental impression	is a sign of
spoken sound	is a symbol of
written mark	

Aristotle makes four claims in which these elements and relations are combined.

(1) Written marks are symbols of spoken sounds.
(2) Spoken sounds are symbols of mental impressions.
(3) Spoken sounds are (in the first place) signs of mental impressions.
(4) Mental impressions are likenesses of actual things.

I shall begin by pointing out some obvious features of these claims. (In the long history of this text even what is obvious has often been overlooked.)

There is nothing explicit in these four claims relating spoken sounds or written marks to actual things, nor is there any apparent implicit claim about such a relationship.[5] When we are told that spoken sounds are symbols and signs of mental impressions and that mental impressions are likenesses of actual things, we are given no license to infer anything at all about a relationship between spoken sounds and actual things. Yet this is just what commentators on this text have regularly done, usually remarking (as if it were obvious) that Aristotle maintains that words stand directly for thoughts and indirectly for things. I think they have been led to do so because they have approached this text in the belief that it contains Aristotle's general theory of meaning. But, as we shall see, this text makes better sense and fits its context better if it is interpreted as playing a more modest role. If it contains no claim at all, explicit or implicit, about a relationship of spoken sounds to actual things, then it is not even a sketch of a general theory of meaning.

In claims (2) and (3) a spoken sound is said to be a symbol and to be a sign, to bear two apparently different relations to a mental impression. Boethius, however, translated both 'σύμβολα' and 'σημεῖα' as '*notae*', thereby hiding this difference from the view of Western philosophers for seven centuries or more, the centuries during which his translation of *De interpretatione* was one of the few books which every philosopher discussed.[6] I am going to proceed on the hypothesis that this terminological difference reflects a real difference Aristotle recognized.

Claim (1) uses one of the two relational terms of claims (2) and (3) in a context in which we can provide a definite, clear interpretation for it, one that must have been evident to Aristotle as well. In one of the relations a spoken sound bears to a mental impression, a spoken sound is to a mental impression as a written mark is to a spoken sound; and we know how a written mark is to a spoken sound. Consider the written mark 'ἄ-ν-θ-ρ-ω-π-ο-ς'. It is, following claim (1), a "symbol" of the spoken sound of the Greek word for man. Now that mark is not a name of that sound or a likeness of it. Nor is that mark a symbolic representation of that sound as the owl is of Athena. It is neither a symptom nor a nonsymptomatic index of that sound on the basis of a regular natural association of occurrence.[7] (It occurs on this page, for instance, in the absence of any associated occurrence of the spoken sound.) To be a symbol, then, is not the same as to be a name, or a likeness, or a symbolic representation, or an index. For x to be a symbol of y is for x to be a notation for y, to be a rule-governed embodiment of y in a medium different from that in which y occurs. Thus the Roman alphabet and the dots and dashes of Morse code are two notations, or symbolizations, for spoken English. The symbolization of spoken sounds in written marks is independent of any semantic role the sounds may be assigned. We write 'w-o-m-a-n' as the symbol of a spoken sound which happens to be the sound of an English word, but we can equally well write 'n-a-m-o-w', the symbol of a sound with no semantic role. Aristotle in claim (1) was of course concerned only with phonograms, but the ideograms of a sign-language are also rule-governed embodiments in another medium, with the interesting difference that the original medium is not vocal but mental.

These observations about the objectively assessable claim (1) can be used in interpreting (2): Spoken sounds are symbols of mental impressions. It should now be clear that this is not a claim that spoken sounds are names, or likenesses, or symbolic representations, or indices of mental impressions. They are to mental impressions as written marks are to them; that is, they are rule-governed embodiments of mental impressions in another medium, "ideophones". One way in which a spoken sound plays a semantic role is in symbolizing a mental impression. When Smith asks Jones 'What's a pentacle?' and Jones says 'A five-pointed star', he may be described (at least sometimes) as rendering audible what was only mental, just as Smith, if he then writes down what Jones said, renders visible what was only audible. Spoken sounds, those that constitute words, are rule-governed embodiments of mental impressions in a vocal medium just as written marks, those that constitute pronounceable sets, are rule-governed embodiments of spoken sounds in a visual medium.

The symbol-relation as described so far is symmetric. As written marks are rule-governed embodiments of spoken sounds in another medium, so spoken sounds are rule-governed embodiments of written marks in another medium; and the same applies to the symbol-relation as it obtains between spoken sounds and mental impressions. But written marks are devised as symbols of spoken sounds and not vice versa. I will take account of this asymmetry by distinguishing encoding and decoding symbolization. If Smith writes down what Jones has said and Robinson reads aloud what Smith has written, Smith encodes and Robinson decodes. Claims (1) and (2) could, then, be revised and supplemented in this way:

(1′) Written marks are encoding symbols of spoken sounds.
(1″) Spoken sounds are decoding symbols of written marks.
(2′) Spoken sounds are encoding symbols of mental impressions.
(2″) Mental impressions are decoding symbols of spoken sounds.

The symbol-relation is clearly irreflexive, because of the stipulation of a different medium if for no other reason. What about transitivity? What, if anything, can be inferred from claims (1) and (2) regarding a symbol-relation between written marks and mental impressions? If the relation is considered in the broad sense, without the encoding/decoding distinction, it is both irreflexive and symmetric and so cannot be transitive. If the encoding or the decoding relation as described so far is considered separately, it may appear to be transitive. But the rules governing the encoding of mental impressions in spoken sounds are obviously different from the rules governing the encoding of spoken sounds in written marks. For that reason we could infer from (1′) and (2′) only that written marks encode mental impressions indirectly, or at one remove. The fact that these characteristics of the symbol-relation are what we should expect is some confirmation for this interpretation.

From the immediately accessible claim (1) I have derived an interpretation of claim (2). Now I want to look at claim (3) in the light of this interpretation of (2). There are two obvious questions of interpretation, even of translation. (A) Are the words 'signs' (σημεῖα) and 'symbols' (σύμβολα) synonymous here? Bonitz says they are,[8] and many translators have evidently been so sure of it that they have not bothered to give their readers a chance to raise the question. (B) Is 'in the first place' (πρώτως) connected (i) with the primacy of the sign-relation over the symbol-relation or (ii) with the primacy of any semantic relation of spoken sounds to mental impressions over any semantic relation they may bear to actual things? Most interpreters have adopted the second of these alternatives.[9] And since an affirmative answer to question (A) precludes recognition of the first alternative, most have adopted (Bii) with no sense of having rejected a competing interpretation.

I have already adopted a negative answer to (A) as a working hypothesis. This is not the point at which to decide how well the hypothesis works, but I can offer some explanation and support for it. Aristotle's general verb for semantic relations is 'σημαίνειν', on a par with our verbs 'mean' and 'signify', and he sometimes uses the closely related noun 'σημεῖον' for general purposes too,[10] somewhat as we sometimes use 'sign'. But the juxtaposition of 'σημεῖον' with 'σύμβολον' in these few lines suggests a stricter interpretation, one borne out by the facts of the language. Elsewhere in Aristotle and in other authors before and after him the words 'σημεῖον' and 'σύμβολον' differ in being associated broadly with natural and with artificial indications, respectively. A medical symptom may be considered the paradigm of a σημεῖον, and an identity token (especially one of two irregular broken halves of a potsherd or a seal on a document) may be considered the paradigm of a σύμβολον. This natural/artificial division is the philological basis of my hypothesis.

If 'σημεῖον' is interpreted as 'symptom', then claim (3) may be rewritten in this way:

(3′) Spoken sounds are (in the first place) effects indicative of their concurrent causes, mental impressions.

I am going to adopt this reading of claim (3) in the further development of my interpretation of this text.[11] As (3′) suggests, the symptom-relation is logically prior to the symbol-relation between spoken sounds and impressions in the mind of the speaker. (That is my reading of 'in the first place'.) A parrot may produce spoken sounds of which impressions in your mind may be the (decoding) symbols, but because they are not symptoms of the occurrence of such mental impressions in the parrot they are not produced by the parrot as (encoding) symbols.

Written marks are symptoms neither of spoken sounds nor of mental impressions although, as we have seen, they are symbols of spoken sounds and perhaps indirectly also of mental impressions. They are not symptoms of spoken sounds because they are regularly produced in the absence of spoken sounds; and they are not symptoms of mental impressions because they persist past the time of their production as spoken sounds do not.

Claim (4) presents difficulties of another sort. Ackrill complains about its vagueness.

What precisely are 'affections in the soul'? Later they are called thoughts.[12] Do they include sense-impressions? Are they, or do they involve, images? Aristotle probably calls them likenesses of things because he is thinking of images and it is natural to think of the (visual) image of a cat as a picture or likeness of a cat. But the inadequacy of this as an account or explanation of thought is notorious.[13]

One respect in which it is notoriously inadequate is its failure to make sense of the notion of true and false thoughts – most obviously of the notion of a true or false existential thought, such as the thought that there is a goat-stag. And since this is the very example of thought which Arisstotle uses in the latter half of Chapter 1, where he speaks of νόημα ἐν τῇ ψυχῇ rather than of παθήματα ἐν τῇ ψυχῇ, there are good *prima facie* grounds in the context of this text for distinguishing between thoughts and affections in the soul (which I have been calling mental impressions). In the best-known passages elsewhere in which Aristotle speaks of affections of the soul (πάθη more often than παθήματα) he is typically speaking of emotions and personality traits – e.g., shame, irascibility, anger, gentleness fear, pity, courage, joy, loving, and hating.[14] There is no reason to suppose that Aristotle or anyone else would describe such affections of the soul as likenesses of actual things. But in the first chapter of *De anima* Aristotle includes "sensing generally" (ὅλως αἰσθάνεσθαι) among affections of the soul, and that must be where the likenesses come in. It seems clear to me that claim (4) is concerned not with thoughts or with emotions and personality traits but with sense-impressions and perhaps with mental images generally, including those of imagination and memory.

In *De anima*, Book III, Chapter 8 (432a7–14), Aristotle apparently claims that no mental activity can occur without (ἄνευ) mental images. I am not sure whether this means that mental images are a necessary concomitant of all mental activity or merely a necessary precondition of all mental activity. But in either case Aristotle clearly distinguishes in those passages between the images on the one hand, and thoughts, mental acts, and other mental entities on the other. Claim (4) is obviously inadequate as "an account or explanation of thought", but the reference to *De anima* in 16a8–9 is evidence that Aristotle is not alluding to mental entities or mental acts in general here: "These matters have been discussed in the work on the soul and do not belong to the present subject".

Taking 16a3–8 seriously requires us to begin, at least, by interpreting claim (4) in such a way as to give it a chance of being true, and that means considering it as applying only to mental images of actual things. As I have tried to show, there is strong support in Aristotle for such an interpretation. Once the narrow scope of claim (4) has been revealed, it is harder to suppose that this text was intended as a general theory of meaning. The difficulty is enhanced by the fact that Chapters 2–6 address themselves to detailed questions regarding meaning and come up with answers that bear no clear resemblance to the account in 16a3–8. But if it is not Aristotle's theory of meaning, as it has almost always been taken to be, what is it?

II

If the text is read as a unit, the emphasis falls on the claims I have so far left out of consideration, the claims regarding the interpersonal sameness and difference of the four elements:[15] actual things, mental impressions, spoken sounds, and written marks. There are four such claims.

(5) Written marks are not the same for all.
(6) Spoken sounds are not the same for all.
(7) Mental impressions are the same for all.
(8) Actual things are the same for all.

These claims, I believe, constitute the grounds for Aristotle's subsequent claims (at 16a19, 16a27, and 17a2) that one or another linguistic entity is significant "by convention" (κατὰ συνθήκην).[16] The point of 16a3–8 is the presentation not of a general theory of meaning but of grounds for the claim that linguistic signification is conventional, a claim that still needed to be made, or at least made unambiguously, in the generation after Plato.[17] From the strength of the conventionalist claims and the breadth of their application[18] we might have expected Aristotle to have provided a full-fledged argument in their support, but the only support we are given for them is in 16a3–8. Such semantic theory as is in that text is there, I think, only to the extent to which it contributes to the support for conventionalism.

An example will fairly illustrate the interpersonal samenesses and differences and Aristotle's intentions in this text generally. Two experimental subjects, Smith and Schmidt, are seated in a darkened room facing a screen. Smith knows only English and Schmidt knows only German. On the screen is projected this shape: △ . Each subject is then told to close his eyes and is asked whether he perceives a likeness of the actual thing he saw, and each says that he does. Each is then asked to draw what he perceived in his mind's eye, and each produces a drawing that looks just like this: △ . Each is then asked to write down under his drawing what that thing is. Smith writes 't-r-i-a-n-g-l-e' and Schmidt writes 'D-r-e-i-e-c-k'. Each is then asked to read what he has written. Smith says 'triangle' and Schmidt says 'Dreieck'.

Of course the example is thin and artificial as an instance of linguistic meaning, but if I am right about Aristotle's intentions in 16a3–8 he has no need of anything richer or more realistic. As I intend to show, something as skimpy as this will serve to elucidate "significant by convention".

All the interpersonal samenesses and differences in claims (5)–(8) are illustrated in my example. Only those concerning the actual thing and the mental impressions need even a word of explanation. The actual thing – the projected figure – is numerically the same for Smith and for Schmidt. The mental impression Smith has of it is numerically distinct from Schmidt's mental impression of the figure, but the two impressions are interpersonally the same in that they have a single Aristotelian form (or are two tokens of a single type). This can be confirmed, if not proved, in the drawings made by the two subjects.

Ackrill suggests that what I am calling claim (7) is meant to follow from the considerations I expressed in (4) and (8): "different people (or peoples) confront the same things and situations, and have the same impressions of them and thoughts about them (*likeness is a natural relation*)".[19] In my article on the history of semantics I make such a suggestion even more explicitly: "The mental modifications arising from that confrontation are *likenesses* (ὁμοιώματα) of the things, and they are thus the same for all men too".[20] If Aristotle intended to argue as I there suggested he did, his argument would clearly be unacceptable. Consider this original –  – and these two likenesses:   . Of course neither of the likenesses is perfect, but imperfection is a regular characteristic of likenesses, more obviously of mental images than of some other sorts. Nevertheless, although the two are not even decent likenesses of each other, much less the same, each of them *is* a likeness of the original.

This line of criticism, suggested in different ways by Ackrill's account and mine, can be directed against Aristotle effectively only if we suppose that he is out to make general claims here regarding mental impressions. If we adopt instead the hypothesis that his purpose is not to do psychology or epistemology but rather to provide grounds for the conventionalism he is going to proclaim, then we can see that all he needs here is an illustration – a single case like my imaginary experiment.[21] Claim (7) does not follow from claims (4) and (8); it is not true in general that if A′ and A″ are two likenesses of A, then A′ and A″ are alike. But of course there are cases, even when A′ and A″ are mental impressions (as in my experiment), in which A′ and A″ *are* alike while the symbolizing spoken sounds and written marks are *not* alike, and that is all Aristotle needs here. That is, I am claiming, all he needs here is a single instance in which claims (5), (6), (7), and (8) are true together.

Before examining my claim further I want to consider claims (5) and (6). In Aristotle's words the two claims are put this way: "just as written marks are not the same for all men, neither are spoken sounds". The 'just as' (ὥσπερ) suggests that what is intended in (6) is brought out more clearly in (5). What is clearer about written marks than about spoken sounds, even in the context of a single language and especially from an unsophisticated point of view, is the conventionality of their relation to what they immediately symbolize. Thomas Aquinas puts this clearly and correctly in his commentary on this passage:

No one has ever raised any question about this as regards letters. It is not only that the principle of their signifying is by imposition, but also that the formation of them is a production of art. Spoken sounds, on the other hand, are formed naturally, and so some men have raised the question as to whether they signify naturally.[22]

It is of course easy to illustrate the lack of universal sameness in written marks, as in the case of 't-r-i-a-n-g-l-e' and 'D-r-e-i-e-c-k'. But the illustration is more to the point if we choose cases in which different marks symbolize one and the same spoken sound – e.g., 'ἡ' in Greek and 'h-a-y' in English – and cases in which one and the same written mark symbolizes different sounds – e.g., 'P-H': 'ray' in Greek and 'f' in English.

III

If I am right in my view that in 16a3–8 Aristotle is providing the grounds for his attribution of conventional signification to linguistic entities, then why does he approach the topic obliquely by way of considering interpersonal sameness and difference rather than pointing out the various principles governing natural and conventional signification? I think there are three reasons for the oblique approach.

In the first place, if he did simply point out the principles governing signification – resemblance, causal connection, regular association, custom, agreement, imposition – he would not be providing any *grounds* for his subsequent claim that spoken sounds (and written marks) are significant by convention in their capacity as symbols. Saying that they are significant by custom, agreement, or imposition is just a fancier way of saying that they are significant by convention.

In the second place, Aristotle is stating his conventionalism against the background of Plato's *Cratylus*. The fact that he has the *Cratylus* in mind in these opening chapters of *De Interpretatione* is indicated by his statement of conventionalism in 17a1–2: "Every sentence is significant (not as a tool but, as we said, by convention)". The phrase 'not as a tool' (οὐχ ὡς ὄργανον δέ) alludes to nothing in *De Interpretatione* and makes sense only as a reference to the doctrine of the *Cratylus*.[23] Moreover, at the beginning of the *Cratylus* the criterion of linguistic naturalness is sameness for all men,[24] and one of the important problems of the dialogue is the difficulty of determining exactly what semantic element Plato thinks is the same for all men, regardless of linguistic differences among them. Plato was concerned to distinguish between natural and conventional *correctness* of names, while Aristotle is concerned with conventional *signification*. But interpersonal sameness and difference are criteria of naturalness and conventionality generally, and Aristotle's claims (5)–(8) regarding comparatively commonplace semantic elements are clear on points that Plato left mysterious.

In the third place, and most important, considerations of sameness and difference do constitute criteria for distinguishing between natural and non-natural signs, and for present purposes we can simply identify non-natural and conventional signs. Temporarily ignoring Aristotle's own use of these notions, I can offer this definition (and complementary definition).

> A natural sign is a sign the correct interpretation(s) of which is (are) necessarily the same for all men.
> (A non-natural sign is a sign the correct interpretation(s) of which is (are) not necessarily the same for all men.)

A few explanatory remarks. I say 'correct' because, of course, there is no assignable limit to incorrect interpretations. An eclipse is not and has never been a natural sign of God's displeasure, no matter what anyone may think or have thought about it. I leave open the possibility of more than one correct interpretation to cover cases of correctness at more than one level of interpretation. A red sunset is correctly interpreted as a sign of good weather the next day and also, on another level, as a sign of considerable dust in the atmosphere. I say 'necessarily' because it could happen that all men adopted a single convention – e.g., 'Mayday' as a signal of distress – and that adoption would certify a single interpretation as correct for all men. But no such decision has any efficacy in establishing the correct interpretation of a natural sign.

Although the definition and its complement make no reference to the principles of signification, they do distinguish effectively between natural and non-natural signification. How closely do they match Aristotle's remarks? The most striking dissimilarity may seem to be the shift from Aristotle's consideration of sameness and difference of signs to a consideration of sameness and difference of interpretations. My illustrations of sameness and difference with regard to written marks help to show that this dissimilarity is only apparent. When Aristotle says that "written marks are not the same for all men" he may mean to remind us that one and the same spoken sound – 'ray' – is symbolized in Greek letters as 'P-H' and in English letters as 'R-A-Y'; and that is a difference of signs. But he may also be taken to mean that the written mark 'P-H' is not the same for all men in that Greek speakers read it as 'ray' and English speakers as 'f'; and that is a difference of interpretations. (Analogous illustrations can be devised of sameness and difference of spoken sounds as symbols of mental impressions.) As for 'correct', which appears in my formulation but not in Aristotle's, it surely is to be understood in his for just the reasons I gave for including it in mine.

The one real difference between what Aristotle says and what I say may be his omission of 'necessarily'; but, given his purposes, I do not think that omission is in any way damaging. As I see it, all he really wants or needs to do here is to establish on the basis of considerations of sameness and difference that spoken sounds and written marks are non-natural, or conventional, signs. Observing that they are not *in fact* the same for all men does that very well; *a fortiori* they are not *necessarily* the same.

From the standpoint of my interpretation of this text the most misleading feature of it is claim (8): Actual things are the same for all. It is innocuous in itself, and it does not get in the way of my interpretation, but it can work together with the reference to *De anima* to give the first half of Chapter 1 the look of a summary statement of the foundations of knowledge and communication, and it is that look which has deceived so many.

IV

In Chapter 1 Aristotle supplies some content for the notion of a spoken sound significant by convention, a notion he first makes use of in Chapter 2.

Ὄνομα μὲν οὖν ἐστὶ φωνὴ σημαντικὴ κατὰ συνθήκην... τὸ δὲ κατὰ συνθήκην, ὅτι φύσει τῶν 'ονομάτων οὐδέν ἐστιν, αλλ' ὅταν γένηται σύμβολον· ἐπεὶ δηλοῦσί γὲ τι καὶ οἱ ἀγράμματοι ψόφοι, οἷον θηρίων, ὧν οὐδέν ἐστιν ὄνομα. (16a19; 26–29)	A *name* is a spoken sound significant by convention... I say 'by convention' because no name is a name naturally but only when it has become a symbol. Even inarticulate noises (of beasts, for instance) do indeed reveal something, yet none of them is a name.

How are these passages to be read in the light of my interpretation?

A name is said to be a spoken sound and not also a written mark because a written mark is simply an encoding symbol for a spoken sound, which is, in turn, (at least sometimes)[25] an encoding symbol for a mental impression. But writing, like speech, is a linguistic medium, as mind is not; and so the primary *linguistic* element is the spoken sound.[26]

In his note on the phrase 'spoken sound significant by convention' Ackrill says

> The linguistic items he wishes to consider are marked off from sounds not spoken, from spoken sounds that are not significant, and from spoken sounds that are natural signs,[27]

which seems clearly right. But if my interpretation is correct, there is something misleading about the way in which the third category of excluded entities is described, since on my interpretation conventionally significant spoken sounds are (at least sometimes) primarily natural signs – σημεῖα, symptoms – of mental impressions. My description of the third category would have to be not 'spoken sounds that are natural signs' but 'spoken sounds *considered as* natural signs' – i.e., all those that are significant *only* as natural signs and those that are also significant by convention considered *in their role* as natural signs.

I want to try to clarify this point before going on. If in ordinary circumstances Smith asks Jones 'What's in the bottle?' and Jones, after examining the contents of the bottle, says 'Water', the sound Jones utters is considered mainly (and perhaps exclusively) in its role as a conventional sign. But Smith may be Jones's doctor. He may know that there is water in the bottle but be interested in determining the nature and extent of brain damage in Jones. In this case when Jones says 'Water' the sound he utters is considered mainly (but not exclusively) in its role as a natural sign, as a symptom of his just then forming the mental impression of water or managing to come up with the spoken sound which symbolizes that impression. In this case it would be equally valuable to the questioner if the respondent uttered a nonsense-syllable or a completely inappropriate word. His attention in this case is directed not to the respondent's message but to the respondent; he wants information not about what the respondent has information about, but about the respondent.

In 16a26–28 Aristotle explains his use of the phrase 'by convention' (κατὰ συνθήκην): "because no name is a name naturally but only when it has become a symbol". Of course a *name* does not *become* a symbol, but a spoken sound (or a name considered simply as a spoken sound) may be said to do so. The point is that no name *considered as a name* exists by nature; a name comes into existence only when a spoken sound becomes a symbol. The notion that a spoken sound *becomes* a symbol is well suited to the view that it is *primarily* a symptom. A spoken sound becomes a symbol by acquiring the same sort of relation to a mental impression as a written mark bears to a spoken sound – rule-governed embodiment in another medium. And it acquires that relation, it seems, by being *used* in certain ways – that is, to call attention to, refer to, *name* the actual things of which the symbolized impression is a likeness. The relation of the spoken sound 'water' to the actual stuff is that of *name to bearer,* which is of course distinct from that of symbol to symbolized (or of symptom to symptomized). But the establishment of the symbolizing relation between the spoken sound and the impression is a necessary condition of the establishment of the name-to-bearer relation. Necessary, but not sufficient; for 'goat-stag' satisfies the necessary condition (in virtue of which it might, somewhat misleadingly, be called a *name*), but in the absence of any actual thing of which the goat-stag image can be the likeness, the establishment of the name-to-bearer relation is impossible.

As Ackrill remarks,[28] the first sentence of the passage in which Aristotle explains his use of 'by convention' is meant to be supported by the second sentence: "Even inarticulate noises (of beasts, for instance) do indeed reveal something, yet none of them is a name". But the support, I think, is in the form of elucidation rather than argument. All *spoken sounds* are symptoms of some state of the speaker, or reveal (δηλοῦσι) something about him. *Inarticulate noises* (ἀγράμματοι ψόφοι) are those for which there is no rule-governed embodiment in another medium; they are unwritable (ἀγράμματοι).[29]

Why is no inarticulate but symptomatic (or revelatory) noise a name? Not simply because it is unwritable. Smith and Jones could agree to play a silly game: "From now on we'll never use the word 'water' but will cough whenever it would be appropriate to use the word". This would count as symbolization, although at least to begin with it would be symbolization not of an impression but of the spoken sound 'water', the encoding medium being inarticulate noise. But if Smith and Jones continued to play their game, the new convention might become so deeply ingrained in them that they would no longer have to "translate"; and if that could happen, why couldn't their coughing become a name? Names do require establishing, and it would be extremely difficult to establish these various coughing noises as a name. But the crucial consideration is that such establishment could take place only within the context of an already established language. The amorphous, unruly character of inarticulate noises would make it impossible to establish the conventions if inarticulate noises were all we had to work with. And, after all, what Aristotle says is not that none of them *can be* a name but that none of them *is* a name. And the reason they are not names is that they are intractable to the demands of convention.[30]

Ackrill criticizes the sentence I am discussing,

> Aristotle only weakens the force of his remark by mentioning *inarticulate* noises, that is, such as do not consist of clearly distinguishable sounds which could be represented in writing. For someone could suggest that what prevents such noises from counting as names is not that they are natural rather than conventional signs, but precisely because they are inarticulate.[31]

I have been trying to show that what prevents them from counting as names is that they are not conventional signs, and that they are not conventional signs "precisely because they are inarticulate".

If I am right about Aristotle's account of conventional signification, then one important feature of it is that it includes one kind of natural signification in an essential capacity. To complicate things further, the semantic element that has the natural signification is a linguistic entity and thus a standard example of a conventional sign. Of course linguistic entities, like anything else, may sometimes occur as natural signs, but Aristotle's account presents their occurring in this capacity as one aspect of their regular occurrence as conventional signs. This combination of what seem to be complementary opposite types of signification strikes me as one of the strengths in the Aristotelian account of conventional signification. Language is not a sign-system *sui generis*, it is just the most complex, most flexible, richest combination of modes of signification; and the more artifical modes are, Aristotle reminds us, constructed on the basis of the less artificial.[32]

Cornell University

NOTES

[1] In the notes to his translation (J. L. Ackrill, *Aristotle's Categories and De Interpretatione*, Clarendon Press, Oxford, 1963; reprinted with corrections, 1966), p. 113.

[2] I am using Ackrill's translation, the only one in English that shows an understanding of the text.

[3] Cf. Ackrill, *op. cit.*, Notes, p. 115: "'A spoken sound significant by convention' gives the genus under which fall not only names but also verbs (Chapter 3) and phrases and sentences (Chapter 4)".

[4] Bekker has 'πρώτως'. Minio-Paluello (*Aristotelis Categoriae et Liber De Interpretatione*, Clarendon Press, Oxford, 1949; reprinted with corrections, 1956) has 'πρώτων' although his sources have either 'πρώτως' or 'πρῶτον'. Evidently he thinks that the two readings are best accounted for by an original that has the omega of the one and the nu of the other. Ackrill's translation is based on Minio-Paluello's text, but he translates this passage as if it contained 'πρώτως' rather than 'πρώτων' with no indication that he has adopted a variant. The Italian translation of Ezio Riondato (in his *La teoria Aristotelica dell'enunciazione*; Editrice Antenore, Padova, 1957) is the only one I know that follows Minio-Paluello's text at this point: "mentre le affezioni dell'anima, di cui questi sono segni come dei (termini) primi (a cui essi si riportano)..." (p. 131). Since the manuscript testimony is overwhelmingly in favor of the adverbial form here, the only reason for adopting the adjectival form to be found in Minio-Paluello's edition is that the adverb makes no sense. Since it seems to me to make good sense, and better sense than the adjective, I follow Bekker's edition (and Ackrill's translation).

[5] The only conceivable textual basis for a claim of this kind is the phrase 'in the first place' (πρώτως) at 16a6, but it supports no doctrine that makes sense. I shall discuss this phrase later.

[6] The ninth-century Arabic translation of Isḥāq ibn Ḥunayn (ed. by A. Badawi; Cairo, 1948) renders both 'σύμβολα' and 'σημεία' as the active participle '*dāll*' in the phrase '*dāllun 'alā*' – 'is indicative of', 'refers to', or 'is an indication of'. Although Isḥāq knew Greek, he translated from the Syriac. (I am grateful to Professor Alfred Ivry for

this information.) William of Moerbeke's Latin translation of 1268 has '*symbola*' and '*signa*' (*Ammonius: Commentaire sur le Peri Hermeneias d'Aristote. Traduction de Guillaume de Moerbeke* (ed. by G. Verbeke) [*Corpus Latinum Commentariorum in Aristotelem Graecorum* II; Louvain and Paris, 1961], p. 32; cf. Verbeke's note on 'σύμβολα', p. LXXXIX; cf. also J. Isaac, *Le Peri Hermeneias en occident de Boèce à Saint Thomas*, J. Vrin, Paris, 1953, p. 160). But Moerbeke's correct translation had no discernible influence. Even Thomas Aquinas, for whom the translation of Ammonius was made (incorporating the new translation of Aristotle), follows Boethius's translation in his commentary on this passage. (Jean T. Oesterle has thereby been misled into writing, in a note on this passage, "The Greek word σύμβολον means 'token' and the Latin word *nota* used by William of Moerbeke is an exact translation of this" [in her *Aristotle: On Interpretation. Commentary by St. Thomas and Cajetan*, Marquette Univ. Press, Milwaukee, 1962; p. 23].) Later medieval commentaries I have seen all follow the Boethius translation of this passage. With the sole exception of J. L. Ackrill English translators of Aristotle have done no better than Boethius. H. P. Cook in the Loeb Aristotle has 'symbols or signs' for the first occurrence of 'σύμβολα', 'signs' for the second, and 'primarily signs' for 'σημεία πρώτως'; E. M. Edghill in the Oxford Aristotle has 'symbols' (for both) and 'directly symbolize'; J. T. Oesterle (*op. cit.*) has 'signs' (for both) and 'first signs'.

7 I am using 'index' as the generic term for an effect as indicative of its cause. A symptom is an effect indicative of a concurrent cause – e.g., a fever taken as indicative of an infection. A nonsymptomatic index is an effect indicative of a cause no longer current – e.g., a scar taken as indicative of a wound.

8 H. Bonitz, *Index Aristotelicus*, Königliche Preussische Akademie der Wissenschaften, Berlin, 1870; art. 'σύμβολον', Part 3. This is also the view of, for example, H. Steinthal (*Geschichte der Sprachwissenschaft bei den Griechen und Römern*, Berlin 1890, 2nd ed., p. 186) and K. Oehler (*Die Lehre vom noetischen und dianoetischen Denken bei Platon und Aristoteles*, München 1962, p. 149). Steinthal's view was developed in opposition to the distinction drawn between the two terms by T. Waitz in his edition *Aristotelis Organon graece* (Leipzig 1844–46). Recent writers who have distinguished the meanings of 'σημεία' and 'σύμβολα' in this passage include P. Aubenque (*Le problème de l'être chez Aristote*, Paris 1962, pp. 106–112) and R. Brandt (*Die aristotelische Urteilslehre*, Marburg 1965, pp. 33–35). (Professor Gabriel Nuchelmans kindly called my attention to Brandt's book and thereby to much of the information contained in this note.)

9 As far as I know, I am the only exception. See my article 'History of Semantics', *Encyclopedia of Philosophy* (Macmillan & Free Press, New York, 1967; Vol. 7, pp. 358–406), p. 362. There may well be others among the Greek commentators, who did not have to rely on Boethius's translation. Ammonius, however, takes 'σημεῖον' and 'σύμβολον' to be two names for artificial representations. See *Ammonii in libro De interpretatione* (ed. by Busse), Berlin 1897, p. 20, lines 1–12. (I owe this observation to Professor Gabriel Nuchelmans.)

10 For example, in *De interpretatione*, Chapter 3, 16b10, where he says of a verb that "it is always a sign (σημεῖον) of what holds, that is, holds of a subject"; and 16b23: "not even 'to be' or 'not to be' is a sign of the actual thing (nor if you say simply 'that which is')...".

11 From here on I will use 'sign' as a generic term and 'symptom' as the term specifically corresponding to what I take to be Aristotle's use of 'σημεῖον' here. It is worth noting that the references to Chapter 1 in Chapter 14 of *De interpretatione* contain passages that seem to reflect the distinction I am drawing between symptom ("spoken sounds

follow (ἀκολουθεῖ) things in the mind" – 23a32) and symbol ("spoken affirmations and negations are symbols of things in the soul" – 24b1).

[12] 16a9–11: "Just as some thoughts (νόημα) in the soul are neither true nor false while some are necessarily one or the other, so also with spoken sounds." The context and the association with spoken sounds certainly suggest that these "thoughts" are to be identified with the "affections in [or of] the soul" mentioned in 16a3 and 6–7. But there are considerations against such an identification too, some of which I will bring out. In any case it is enough for my purposes to show, as I shall try to do, that Aristotle does not need a general claim about thoughts in 16a3–8.

[13] *Op. cit.*, Notes, p. 113.

[14] See *Categories* Chapter 8 and *De Anima*, Book I, Chapter 1.

[15] The sort of interpersonal sameness and difference that is important to Aristotle here is plainly not just individually interpersonal but intercommunal or interlinguistic.

[16] The commentary of Giulio Pacio (recommended by Ackrill, *op. cit.*, p. 156) views these claims in this way and makes some sensible remarks about them (Julius Pacius, *In Porphyrii Isagogen et Aristotelis Organum commentarius*, Frankfurt 1597; reproduced photographically, Georg Olms, Hildesheim, 1966, p. 61).

[17] On Plato's views on the contributions of nature and convention to language see my article 'Plato on the Correctness of Names', *American Philosophical Quarterly* **8** (1971), 126–138.

[18] 16a26–29: "I say 'by convention' because no name is a name naturally but only when it has become a symbol". 17a1–2: "Every sentence is significant (not as a tool but, as we said, by convention)".

[19] *Op. cit.*, Notes, p. 113; italics added. Ackrill's main aim in this passage is to contrast the naturalness of likeness with the conventionality of the symbol-relation.

[20] *Encyclopedia of Philosophy* **7**, p. 362.

[21] I am not maintaining that Aristotle's intentions are plainly disclosed in the language of 16a3–8. On the contrary, I think that it is hard to tell from that text what he intends there and that it is only by reading back into it what we can learn about his purposes in Chapters 2–6 that we can see what must be going on here.

[22] *In libros Peri hermeneias expositio*, Liber I, Lectio II, 8 (Leonine ed., Vol. I, p. 13): "Sed hoc quidem apud nullos unquam dubitatum fuit quantum ad litteras: quarum non solum ratio significandi est ex impositione, sed etiam ipsarum formatio fit per artem. Voces autem naturaliter formantur; unde et apud quosdam dubitatum fuit, utrum naturaliter significent".

[23] See *Cratylus* 385E–390A, especially 387D and 388A, and my article, 'Plato on the Correctness of Names', pp. 128–129. In his second commentary on *De interpretatione* Boethius expressly linked 17a1–2 to the *Cratylus* and developed the connection between semantic naturalism and the tools doctrine (ed. by Meiser, Vol. 2, pp. 93–94).

[24] *Cratylus* 383B.

[25] *Physics*, Book II, Chapter 1 (193a7) is interesting in this connection: "a man born blind may form syllogisms concerning colors, but such a man must be arguing about names without having any corresponding thoughts" (νοεῖν δὲ μηδέν). I think it is significant that the blind man is said to be able to form syllogisms concerning colors – e.g., 'Whatever is white is colored, and Socrates is white; so Socrates is colored'. It is in their occurrence as syllogistic terms that color-words can most clearly be detached from the sort of mental imagery they might be thought to be associated with in descriptive statements.

[26] This seemingly commonplace view may have been developed, like other views in

these opening chapters, in conscious opposition to the *Cratylus*, in which Plato recognizes a trans-linguistic name "naturally fitted for each thing" (389D–390A). Elsewhere, where he may not have had the *Cratylus* in mind, Aristotle speaks casually of discourse in the mind (*Posterior Analytics* Book I, Chapter 10, 76b24). And Boethius reports that "the Peripatetics" developed a doctrine of three discourses: written, spoken, and mental (in his second commentary on *De interpretatione*, ed. by Meiser, Vol. 2, pp. 29, 30, 36, and 42). On this doctrine see Gabriel Nuchelmans, *Theories of the Proposition: Ancient and Medieval Conceptions of the Bearers of Truth and Falsity* (Mouton, The Hague 1973), Chapter 8, Section 1.3.

[27] *Op. cit.*, Notes, p. 115.

[28] *Op. cit.*, Notes, p. 117.

[29] Like Plato (*Philebus* 18C), Aristotle sometimes uses the word 'letters' (γράμματα) to refer to units of spoken sound rather than to written marks: "Spoken language is made up of letters. If the tongue were not as it is and the lips were not flexible, most of the letters could not be pronounced; for some are impacts of the tongue, others closings of the lips" (*Parts of Animals*, Book II, Chapter 16, 660a3–7). The inarticulate, unwritable (literally, unlettered) noises are probably most precisely described as those that cannot be analyzed into these standard units of spoken sound. (I owe this observation to Professor Ronald Zirin, who states it more fully elsewhere in this volume.)

[30] The demands of convention are more stringent for names than for larger units of communication. Language had to begin with inarticulate noises (recognized as, for example, cries for help) playing communicative roles like those now played by certain sentences, but it could not have begun with names.

[31] *Op. cit.*, Notes, p. 117.

[32] I am very grateful to Sally Ginet, Gabriel Nuchelmans, Eleonore Stump, Nicholas Sturgeon, the members of the Cornell Ancient Philosophy Discussion Club, and the participants in the Symposium on Ancient Logic at the State University of New York at Buffalo for their criticisms of earlier versions of this paper.

RONALD ZIRIN

INARTICULATE NOISES

Aristotle's definition of a name (noun?) as 'a sound significant by convention' (*De Interpretatione*, Chapter 2) is interestingly discussed in Professor Kretzmann's paper in this volume. The definition is followed by an elucidating reference to 'inarticulate noises (of beasts, for instance)' which, though they reveal (δηλοῦσιν) something, are not names. The Greek word ἀγράμματοι which is here translated as 'inarticulate' needs further discussion. The metaphor that intelligible speech is 'articulate', i.e. 'provided with joints' occurs in Aristotle in *Historia Animalium*, 4.9:

> διάλεκτος δ' ἡ τῆς φωνῆς ἐστι τῇ γλώττῃ *διάρθρωσις*.
> Speech is the *articulation* of the voice by the tongue.

The word διάρθρωσις is based on ἄρθρον 'joint', a word which is also used by Plato (?) in a definition of the syllable, cf. *Definitiones*, 414 D:

> Συλλαβὴ ἀνθρωπίνης φωνῆς ἄρθρον ἐγγράμματον.
> The syllable is a 'joint' of human voice consisting of letters.

The metaphor of articulation, however, is not apt in translating ἀγράμματος which refers not to syllables but to the letters of which they consist. The plain meaning of ἀγράμματος is 'not having letters' either in the sense 'not consisting of letters' or in the sense 'not knowing letters, illiterate'.[1]

The term, therefore, does not mean inarticulate in the literal sense, and I do not think that it means 'unwritable'. First of all, the sounds of animals are writable. Greek used onomatopoetic written representations of the sounds of animals (comparable to 'meow' and 'bow-wow') in precisely the way English does. But more important, the word γράμμα which literally means 'letter' is often employed by Aristotle in an extended sense. For example, in *De Partibus Animalium* (660a) there is a discussion of the function of the lips and tongue in pronunciation which clearly uses the term γράμμα in the sense of 'minimal unit of speech-sound':

J. Corcoran (ed.), Ancient Logic and Its Modern Interpretations, 23–25.

> ὁ μὲν λόγος ὁ διὰ τῆς φωνῆς ἐκ τῶν *γραμμάτων* σύγκειται, τῆς δὲ γλώττης μὴ τοιαύτης οὔσης μηδὲ τῶν χειλῶν ὑγρῶν οὐκ ἂν ἦν φθέγγεσθαι τὰ πλεῖστα τῶν γραμμάτων τὰ μὲν γὰρ τῆς γλώττης εἰσὶ προσβολαί, τὰ δὲ συμβολὰι τῶν χειλῶν.
>
> For vocal language is composed of *letters*. If the tongue were not such as it is, and if the lips were not pliant, it would not be possible to pronounce most of the letters; for some of them are applications of the tongue and some closings of the lips.

The word γράμμα, then, may be used in reference to language, as the equivalent of στοιχεῖον 'minimal unit (of speech-sound)'[2] and the word ἀγράμματος could be used to mean 'not resolvable into discrete units of speech-sound'. The phrase ἀγράμματοι ψόφοι, then, refers to noises which are not analyzable into discrete units of speech-sound, noises which do not consist of phonemes.

The phrase οἷον θηρίων 'of beasts, for example', provides one example of ἀγράμματοι ψόφοι. A bit more detail about the sounds of animals is given in *Historia Animalium*, 488a 33:

> καὶ τὰ [ζῷα] ψοφητικά, τὰ δὲ ἄφωνα, τὰ δὲ ἀγράμματα.
>
> Some [creatures] emit noise, some are voiceless, some letterless...

At a later date, in [Pseudo-] Aristotle, *Problems* (895a) 'letterless' speech is imputed to both beasts and young children:

> ὁμοίως δὲ οἵ τε παῖδες καὶ τὰ θηρία δηλοῦσιν: οὐ γάρ πω οὐδε τὰ παιδία φθέγγονται τὰ γράμματα.
>
> Children and beasts express themselves in the same way, for children do not yet utter letters.

In conclusion, the term γράμμα was used to refer to minimal units of speech-sound. Hence the terms ἀγράμματος and ἐγγράμματος when applied to vocalization should be taken to mean 'not resolvable into discrete units of speech-sound' and 'resolvable into discrete units of speech-sound' respectively. It follows that the characteristic of human language that Aristotle refers to in the passage under discussion is that the sound of human speech is resolvable into phonemes.

State University of New York at Buffalo

NOTES

[1] The opposite of ἀγράμματος is ἐγγράμματος, which is used in the definitions of language (λόγος) given in Plato (?) *Definitiones* 414 D: λόγος φωνὴ ἐγγράμματος..., "language is voice consisting of (resolvable into) letters...".

[2] This is the term which Plato generally uses to refer to speech sounds in the *Cratylus*, and is also used in this sense by Aristotle in the *Poetics*.

NEWTON GARVER

NOTES FOR A LINGUISTIC READING OF THE *CATEGORIES*

1. If Aristotle's *Categories* provide a classification of things and not of sayings, as is traditionally insisted, the things classified are at any rate 'things that can be said'. It is interesting, therefore, to inquire whether the *Categories* may be regarded as containing, in rudimentary form, results that might be more appropriately and more completely presented in terms of current methods of linguistic analysis, applied to a level of language or discourse that linguists usually ignore.

2. Both the name 'categories', which signifies predications or sayings, and the position of the work at the beginning of the *Organon*, which deals with matters of logic and language, reinforce the temptation to interpret the *Categories* linguistically. Although neither the title nor the position of the work in the corpus is directly due to Aristotle, they do show that the inclination to treat the *Categories* as at least partially linguistic goe sback to the very earliest tradition of Aristotelian scholarship.

3. The determination that the categories can be given a linguistic interpretation – even the conclusion that they are linguistic, Ackrill[1] and Benveniste[2] notwithstanding – would not suffice to show that they are not also (in some sense) metaphysical, nor that they are not universal.

4. The most useful linguistic method to employ in this inquiry is distinctive feature analysis,[3] which has been used in several kinds of linguistic analysis. Passages in the *Categories* can be interpreted as employing a related method, if not an early version of the method itself.

5. This method is based on a complex presupposition: that nothing is linguistically significant (or real) unless it contrasts with something else, that what it contrasts with is an alternative possibility within a systematic array of possibilities, and that the possible alternatives are determined by binary (sometimes ternary, positive/negative/neutral; or at any rate

J. Corcoran (ed.), Ancient Logic and Its Modern Interpretations, 27–32.

finitary) alternation along a finite number of dimensions, called features.

6. It is unlikely that all types of phenomena admit of a fruitful distinctive feature analysis. The method does not, for example, seem fruitfully applicable either to mechanics or to formal logic. Admitting of a distinctive feature analysis may be a distinctive feature of some types of linguistic phenomena.

7. In phonology there are, theoretically, a finite number of articulatory and acoustic dimensions along which spoken sound can vary. In the phonemic analysis of a given language, each phonological dimension is either relevant or irrelevant for the identification of given phonemes, and the relevant dimensions, or features, are either positive or negative. Phonemes can then be regarded as bundles (that is, simultaneous collocations) of distinctive features. The English phoneme /*p*/, for example, can be described as the simultaneous presence of one set of phonetic features (the positive ones) and absence of another set (the negative ones), with the remaining phonetic features (*e.g.* aspiration) being nondistinctive or irrelevant.

8. In semantic theory lexical meanings can analogously, though somewhat more precariously, be regarded as bundles of abstract semantic markers.[4]

9. Aristotle does not define the categories, but he is careful to say what is distinctive about each. Some features, such as whether something in the category can be said to be more or less so, are specified either positively or negatively for each category.

10. Katz[5] has suggested that Aristotle's categories can be interpreted as abstract semantic markers which (a) are entailed by other semantic markers and (b) do not themselves entail other semantic markers. Even leaving aside epistemological questions that arise about the entailments, Katz' suggestion is implausible. His account does not fit what Aristotle listed as categories, it gives no place to the features that Aristotle singled out as distinctive, and it presupposes a full-blown logical apparatus instead of providing a basis for it.

11. Aristotle's categories are not semantic categories.

12. Aristotle's categories are derived from predication: they are the kinds or species of the values of the variables in the form *X is predicated of some a*. This is not to say that every member of each category can be predicated of something, but only that it must be distinctively involved in such predication and that it is what it is because of this distinct sort of involvement. A 'this', for example, cannot be predicated of anything, but it may be the subject of a predication, either as a substance or as something inhering in a substance.

13. Predication, or making truth-claims, is a genus of speech acts (language-games). Aristotle assumes it can be distinguished from other sorts, such as inferring, praying, commanding, imploring, promising, reciting poetry, and so on. Viewed linguistically, therefore, Aristotle's *Categories* form a small subsection in the general theory of speech acts.

14. It is certain that predication is more basic than some other sorts of language acts (such as inferring, which clearly presupposes predication), and there are considerations from generative grammars and from common sense which suggest that it may be the most basic sort of speech act. This suggestion is to be regarded as contentious;[6] but even if it were to be granted, its significance would depend on predication having been recognized or identified initially as one kind of speech act among many.

15. Speech acts are distinguished, one kind from another, by two sorts of criteria, the circumstances in which they are appropriate and the sort of questions and comments that can be made in response to them.[7] The features that Aristotle cites to distinguish the categories belong mainly to the second group.

16. Ackrill points out (p. 79) that "one way in which he [Aristotle] reached categorical classification was by observing that different types of answer are appropriate to different questions". This is true, and useful for seeing the overall design of the *Categories*. But the distinctive features that Aristotle cites are based on the reverse insight, that different questions are appropriate to different sorts of predication.

17. Some examples: (a) 'Substance, it seems, does not admit of a more and a less' (3b33). Suppose *X* is predicated of some *a* (someone says, '*a* is *X*'); It goes hand-in-hand with *X* being in the category of substance that no question can be raised whether *a* is more *X* than *b* or less *X* than *a* was yesterday. If the question can be raised, the predicate must belong to some other category, where this feature is positive or neutral rather than negative. If someone says, '*a* is more a man than *b*', the presence of the word 'more' shows the predication to be qualitative rather than substantial, even though 'man' normally signifies a substance. (b) A substantial predication involves not only *predicating* *X* of *a* but also *saying* *X* of *a*. The latter (but not the former) carries with it a commitment to predicate the definition of *X* of *a*; that is, both the genus of *X* and the differentia of *X* are also implicitly predicated of *a*, when *X* is *said* of *a*. This obviously shapes the subsequent discourse possibilities: for example, I can attack a substantial predication by contending that the definition of the predicate does not apply to the subject; but I could not attack a quantitative predication in this manner.

18. Each feature governs a specific range of possible discourse: they are *discourse features*. When a feature is positive, a certain set of responses (questions, challenges, comments, etc.) is open or permitted to predications in that category. When a feature is negative, another set of responses is open or permitted.

19. From this point of view, therefore, categories are (or are equivalent to) distinct clusters of discourse possibilities.

20. This account has been sketchy and programmatic, and is not intended to establish a definitive reading of the *Categories*.

21. One advantage of such a linguistic reading is that it brings the discussion of categories into a field of active scholarly research. It thereby makes possible a rational and potentially useful criticism of Aristotle's work. Within his category of substance, for example, discourse features can certainly be found to distinguish substances in the modern sense (gold, coal, mud, water, etc.) both from individuals and from natural kinds (species and genera) – perhaps making use of the distiction between mass nouns and count nouns.[8]

22. There are nonetheless serious reservations to be kept in mind. Although predication is a universal speech act, and probably necessarily so, it is not at all clear that the discourse features which distinguish the categories are universal; nor is it clear what the import would be of their not being universal. Another ground for caution is that discourse features seem to belong to the domain of rhetoric whereas the categories have always seemed to belong to the domain of logic. A third concern is that the theory of speech acts (which has the potential for revitalizing rhetoric in the way that the theory of quantification revitalized logic), within which this reading of the *Categories* is to be developed, is itself in a primitive state, and its precise relation to other branches of linguistics remains uncertain.

23. These issues must be kept in mind as further research is done on this linguistic reading of the *Categories*. The reading proposed must be taken as tentative and exploratory. In the long run it may prove to shape our understanding of the theory of speech acts and the science of rhetoric as well as our understanding of Aristotle.

State University of New York of Buffalo

NOTES

[1] J. L. Ackrill, *Aristotle's 'Categories' and 'De Interpretatione'*, Clarendon Press, Oxford, 1963, p. 71. I have used Ackrill's translation. His notes, to which I refer here, are both helpful and stimulating.
[2] E. Benveniste, *Problems in General Linguistics*, Univ. of Miami Press, Coral Gables; 1971, Chapter 6.
[3] This method of analysis is due to Roman Jakobson more than to anyone else. See R. Jakobson, C. G. M. Fant, and M. Halle, *Preliminaries to Speech Analysis*, MIT Press, Cambridge, Mass., 1952; N. Chomsky and M. Halle, *Sound Pattern of English*, Harper and Row, New York, 1968; and Fred W. Householder, *Linguistic Speculations* Cambridge Univ. Press, London, 1971. Most recent linguistic textbooks have a discussion of features.
[4] The best presentation of semantic theory from this perspective is J. J. Katz, *Philosophy of Language*, Harper and Row, New York, 1966.
[5] *Op. cit.*, pp. 224–239.
[6] I take it to have been contested, for example, by Malinowski, with his emphasis on phatic communion, in the appendix to Ogden and Richards, *The Meaning of Meaning*, 10th ed. Routledge and Kegan Paul, London, 1949; by Husserl, with his insistence on the primacy of prepredicative judgment in *Formal and Transcendental Logic*, Martinus Nijhoff, The Hague, 1969; by Wittgenstein in the early sections of *Philosophical Investi-*

gations, Blackwell, Oxford, 1953; and by Derrida in *Speech and Phenomena*, Northwestern Univ. Press, Evanston, 1973.

[7] See J. L. Austin, *How to Do Things with Words*, Harvard Univ. Press, Cambridge, Mass., 1962; J. R. Searle, *Speech Acts*, Cambridge Univ. Press, Cambridge, 1969; and L. Wittgenstein, *Philosophical Investigations*, Basil Blackwell, Oxford, 1953, esp. pp. 1–25, 304.

[8] This sort of development was suggested to me by John Corcoran, to whom I am also indebted for suggestions incorporated at several places.

PART TWO

MODERN RESEARCH IN ANCIENT LOGIC

IAN MUELLER

GREEK MATHEMATICS AND GREEK LOGIC

1. INTRODUCTION

By 'logic' I mean 'the analysis of argument or proof in terms of form'. The two main examples of Greek logic are, then, Aristotle's syllogistic developed in the first twenty-two chapters of the *Prior Analytics* and Stoic propositional logic as reconstructed in the twentieth century. The topic I shall consider in this paper is the relation between Greek logic in this sense and Greek mathematics. I have resolved the topic into two questions: (1) To what extent do the principles of Greek logic derive from the forms of proof characteristic of Greek mathematics? and (2) To what extent do the Greek mathematicians show an awareness of Greek logic?

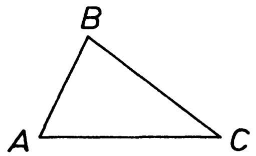
Before answering these questions it is necessary to clear up two preliminaries. The first is chronological. The *Prior Analytics* probably predates any surviving Greek mathematical text. There is, therefore, no possibility of checking Aristotle's syllogistic against the actual mathematics which he knew. On the other hand, there is no reason to suppose that the mathematics which he knew differs in any essential way, at least with respect to proof techniques, from the mathematics which has come down to us.

The major works of Greek mathematics date from the third century B.C. For determining the role of logic in Greek mathematics it seems sufficient to consider only Euclid's *Elements*. It is the closest thing to a foundational work in the subject. The surviving works of the other great mathematicians of the period, Archimedes and Apollonius, are more advanced and therefore more compressed in their proofs. The absence of signs of the influence of logic in them is not surprising. The evidence is too obscure to assign a date to the development of Stoic propositional logic, but I shall take as a date the *floruit* of its major creator, Chrysippus (280–207). Doing so means denying any influence of Stoic logic on the *Elements* and, tacitly, on Greek mathematics in general. I hope that the over-all plausibility of my reconstruction in this paper will provide a

J. Corcoran (ed.), Ancient Logic and Its Modern Interpretations, 35–70.

sufficient justification for the denial. But now I wish to discuss, as the second preliminary, a question relevant to the issue: How does one decide whether a given mathematical argument or work is influenced by a given logic?

In *Elements* I,19 Euclid proves that, given two unequal angles of a triangle, the side opposite the greater angle is greater than the side opposite the lesser. He proceeds as follows:[1]



(1) Let *ABC* be a triangle having the angle *ABC* greater than the angle *BCA*; I say that the side *AC* is also greater than the side *AB*. (2) For, if not, *AC* is either equal to *AB* or less. Now *AC* is not equal to *AB*; (3) for then the angle *ABC* would also have been equal to the angle *ACB*; (4) but it is not; therefore (5) *AC* is not equal to *AB*. Neither is *AC* less than *AB*; (6) for then the angle *ABC* would also have been less than the angle *ACB*; (7) but it is not; therefore (8) *AC* is not less than *AB*. And it was proved that it is not equal either. Therefore (9) *AC* is greater than *AB*. Therefore in any triangle the greater angle is subtended by the greater side. Q.E.D.

Much of the argument here can be analyzed in terms of Chrysippus's *anapodeiktoi logoi*. Thus (5) follows from (3) (an instance of a previously proved proposition, I.5) and (4) (a 'trivial consequence' of (1)) by the second *anapodeiktos*. And (8) is related similarly to (6) and (7). If (2) is taken as an expression of trichotomy, then (9) follows from (2), (5), and (8) by two applications of the fifth *anapodeiktos*.[2]

There are many other cases in the *Elements* which could be analyzed similarly. But since reasoning in accordance with the rules of a logic does not in itself imply knowledge of the logic, the possibility of analyzing a Euclidean proof in terms of Stoic propositional logic does not justify attributing to Euclid a knowledge of Stoic logic. Justification of such an attribution requires, at the very least, clear terminological parallels. However, there are none.

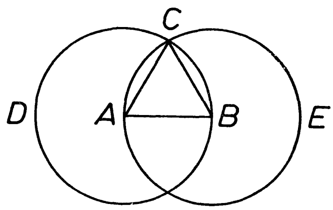
The paper which follows has three main sections. In the first I discuss the character of Euclidean reasoning and its relation to Aristotle's syllogistic. In the second I consider the passages in the *Prior Analytics* in which Aristotle refers to mathematics; my purpose here is to determine whether reflection on mathematics influenced his formulation of syllogistic. In both sections my conclusions are mainly negative. Euclid shows no awareness of syllogistic or even of the basic idea of logic, that validity of an argument depends on its form. And Aristotle's references to mathematics seem to be either supportive of general points about deductive reasoning or, when they relate specifically to syllogistic, false because based on syllogistic itself rather than on an independent analysis of mathematical proof.

In the third main section of the paper I consider the influence of mathematics on Stoic logic. As far as Chrysippean propositional logic is concerned, my conclusions are again negative. However, it is clear that at some time logicians, probably Stoic, began to consider mathematical proof on its own terms. Although they never developed what I would call a logic to cover mathematical proof, they at least realized the difference between it and the logical rules formulated in antiquity. Much of the third section is devoted to an attempt to reconstruct in outline the history of logical reflections on mathematics in the last two centuries B.C. In conclusion I recapitulate briefly my conclusions about the relation between Greek mathematics and logic.

2. EUCLID'S *Elements* AND LOGIC

One still reads that Euclid's logic is Aristotelian syllogistic.[3] But one need only try to carry out a single proof in the *Elements* by means of categorical syllogisms to see that this claim is false. If Euclid has any logic at all, it is some variant of the first order predicate calculus. In order to bring out the specific character of Euclidean reasoning, I reproduce the first proposition of the *Elements* together with an indication of the customary Greek divisions of a proposition.[4]

protasis	On a given finite straight line to construct an equilateral triangle.
ekthesis	Let *AB* be the given finite straight line.



diorismos	Thus it is required to construct an equilateral triangle on the straight line *AB*.
kataskeuē	With center *A* and distance *AB* let the circle *BCD* be described; again, with center *B* and distance *BA* let the circle *ACE* be described; and from the point *C*, in which the circles cut one another, to the points *A*, *B* let the straight lines *CA*, *CB* be joined.
apodeixis	Now, since the point *A* is the center of the circle *CDB*, *AC* is equal to *AB*. Again, since the point *B* is the center of the circle *CAE*, *BC* is equal to *BA*. But *CA* was also proved equal to *AB*; therefore each of the straight lines *CA*, *CB* is equal to *AB*. And things which are equal to the same thing are also equal to one another; therefore *CA* is also equal to *CB*. Therefore the three straight lines *CA*, *AB*, *BC* are equal to one another.
sumperasma	Therefore the triangle *ABC* is equilateral; and it has been constructed on the given finite straight line *AB*. *Quod erat faciendum*.

In modern terms all of this proposition except the *protasis* and *diorismos* would be considered proof. But, as the terminology suggests, only the *apodeixis* was considered proof by the Greeks. I shall here analyze proposition 1 primarily in terms of Gentzen's system of natural deduction for the predicate calculus.[5] This analysis presupposes a somewhat artificial reformulation of portions of the text. For example, the *protasis* is not an assertion at all and hence can not be proved in the strict sense. I shall discuss the character of the *protasis* briefly below. Here I shall take it as a general statement: On any straight line an equilateral triangle can be constructed.

The *ekthesis* is, then, a particular assumption ('*AB* is a straight line') from which a conclusion ('An equilateral triangle can be constructed on *AB*') will be derived. In the *kataskeuē* the drawing of the two circles and of the lines *CA* and *CB* is justified by the postulates 1 and 3:

> Let it be postulated to draw a straight line from any point to any point;
> and to describe a circle with any center and distance.

I know of no logic which accounts for this inference in its Euclidean formulation. One 'postulates' that a certain action is permissible and 'infers' the doing of it, i.e., does it. An obvious analogue of the procedure here is provided by the relation between rules of inference and a deduction. Rules of inference permit certain moves described in a general way, e.g., the inferring of a formula of the form $A \vee B$ from a formula of the form A. And in a deduction one may in fact carry out such a move, e.g., write '$(P \& Q) \vee R$' after writing '$P \& Q$'. The carrying out of a deductive step on the basis of a rule of inference is certainly not itself an inference. For neither the rule nor the step is a statement capable of truth and falsehood. And if the analogy is correct, Euclid's constructions are not inferences from his constructional postulates; they are actions done in accord with them.

There is a further correspondence between constructions and inferences which lends support to the analogy. If one wants to study inference with mathematical precision, one treats deductions as fixed objects, sequences of formulas satisfying conditions specified on the basis of the rules of inference. In other words, when inference is studied mathematically, acts of inference are dropped from consideration and replaced by objects which could have been created by a series of inferences but for which the question of creation is irrelevant; objects satisfying the conditions are simply assumed to exist. The analogy with geometry should be clear. In the modern formulation of Euclid's geometry[6] there are no constructions of straight lines or circles. The axioms are stated in such a way as to guarantee the existence of these objects. Rather than construct the circle with center *A* and distance *AB*, the modern geometer simply derives the theorem asserting the existence of such a circle.

The analogy proposed here is easily extended to explain the character of the *protasis* of proposition 1. The Greeks called proposition 1 a problem, construction to be carried out, and opposed problems to theorems, assertions to be proved.[7] The analogy suggests that proposition 1 be likened to a short-cut rule of inference justified by showing that application of it is tantamount to a series of applications of the original rules. And, of course, Euclid does use the construction of an equilateral triangle on a given line directly in subsequent proofs (e.g., in I,2).

The *apodeixis* is on the surface very simple, very easy to understand, but logically it is fairly complex. The inferences to the equality of AC with AB and of BC with BA are based on definitions 15 and 16 of book I:

> A circle is a plane figure contained by one line such that all the straight lines falling upon it from one point among those lying within the figure are equal to one another;
> and the point is called the center of the circle.

It is clear Euclid is making some kind of deductive argument at the beginning of the *apodeixis*. But it is not at all clear that he thinks of it as a formal argument, an argument based on formal logical laws. In modern notation the definition of 'circle' may be represented as follows:

(1) x is a circle $\leftrightarrow$ (i) x is a plane figure &
(ii) $(E!y)$ [y is a line containing x &
(iii) $(E!z)$ (z is a point within x &
(u) (v) (u is a straight line from z to y &
v is a straight line from z to $y \rightarrow u$ equals v))].

From (1) and 'CDB is a circle' one can infer the *definiens* of (1) with 'CDB' substituted for 'x'. Such an inference could be referred to Aristotle's syllogistic if one were willing to allow singular terms in syllogisms[8] and to treat the complex term corresponding to the *definiens* as a term in a categorical proposition. But doing these two things will not suffice to recover the whole argument. As a next step we need to apply a propositional rule, &-elimination, to get

(2) $(E!y)$[y is a line containing CDB &
$(E!z)$ (z is a point within CDB &
(u) (v) (u is a straight line from z to y &
v is a straight line from z to $y \rightarrow u$ equals v))].

Reconstructing the next piece of Euclid's argument seems to be impossible. For in proposition 1 Euclid makes no reference to the distinction between the circle and its circumference, a distinction which is expressed in the definition of circle. I shall pass over the difficulty here by dropping cause (ii) and identifying the circle with its circumference. As a result we hlave

(3) $(E!z)$ (z is a point within CDB &
$(u)\,(v)$ (u is a straight line from z to CDB &
v is a straight line from z to $CDB \rightarrow$
u equals v)).

We wish to infer from (3) and 'A is the center of CDB'

(4) A is a point within CDB &
$(u)\,(v)$ (u is a straight line from A to CDB &
v is a straight line from A to $CDB \rightarrow$
u equals v).

Obviously the definition of 'center' is being invoked for this step, and the move is logically sound. However, the apparatus involved in justifying the step goes beyond any Greek logical theory known. Since Euclid seems to treat his geometric definitions as concrete specifications of intuitive objects rather than as abstract characterizations,[9] he would probably not recognize that any step of inference at all is involved here.

From (4) by &-elimination we obtain that any two straight lines from A to CDB are equal. The inference from this assertion and 'AB and AC are straight lines from A to CDB' to 'AB equals AC' is an example of the most common form of explicit inference in the *Elements*. The form recurs in the *apodeixis* of I,1 when Euclid establishes the equality of CA and CB using the first common notion, 'Things equal to the same thing are also equal to one another'. In modern notation this argument runs

(5) $(u)(v)(w)$(u equals w & v equals $w \rightarrow u$ equals v);
(6) CA equals AB;
(7) CB equals AB;
(8) therefore CA equals CB.

In later antiquity this argument became the paradigm of a mathematical argument.[10] The Peripatetics, intent upon defending Aristotle, claimed that the argument is really a categorical syllogism:

(A) Things equal to the same thing are also equal to one another;
CA and *CB* are things equal to the same thing;
therefore *CA* and *CB* are equal to each other.[11]

What is the minor term of this 'syllogism'? Presumably '*CA* and *CB*', i.e., the pair (*CA*, *CB*). The modern analysis, according to which the the minor premiss and the conclusion each assert that a certain relation holds between two subjects *CA* and *CB*, seems more natural than one according to which the premiss and the conclusion each assert a property of a pair taken as a single thing. But so long as the inference from (5), (6) and (7) to (8) is treated in isolation, there is no way to refute the Peripatetic analysis. Yet the context of the inference makes clear why the Peripatetics were wrong. The following represent plausible renderings of the proofs of (6) and (7) as categorical syllogisms:

(B) Straight lines from *A* to *CDB* are equal to each other;
CA and *AB* are straight lines from *A* to *CDB*;
therefore *CA* and *AB* are equal to each other.

(C) Straight lines from *B* to *ACE* are equal to each other;
CB and *AB* are straight lines from *B* to *ACE*;
therefore *CB* and *AB* are equal to each other.

The minor premiss of (A) is presumably to be inferred directly from the conclusions of (B) and (C). Clearly it cannot be inferred by a categorical syllogism since such a syllogism will require five terms, '*CA* and *AB*', '*CB* and *AB*', '*CA* and *CB*', 'equal to each other', and 'equal to the same thing'. Thus although (A), (B), and (C) can be construed as categorical syllogisms, they cannot be combined to yield a syllogistic reconstruction of Euclid's *apodeixis*. For it depends on the relations among the three straight lines and not on properties of them taken as pairs.

In ancient logic the *sumperasma* is the conclusion inferred from the premisses of an argument. In the *Elements*, however, the *sumperasma* is not so much a result of inference as a summing up of what has been established. This summarizing character is made clearer in the case of theorems for which the *sumperasma* consists of the word 'therefore', followed by a repetition of the *protasis*, followed by 'Q.E.D.' (See the proof of I,19 quoted above.) From the modern point of view the *apodeixis* ends with a particular conclusion reached from particular assumptions;

tacit in the *sumperasma* are steps of conditionalization to get rid of the assumptions and of quantifier introduction or generalization. Throughout antiquity, indeed down into the nineteenth century, the latter step was not seen as a matter of logic.[12] The inference was brought into the domain of logic only with the invention of the quantifier and the discovery of the rules governing it.

I have analyzed *Elements* I,1 in order to show that Euclid's tacit logic is at least the first order predicate calculus, nothing less. His logic may even be more than that, since representing his reasoning in the first order predicate calculus would seem to require reformulations foreign to the spirit of the *Elements*. I hope I have also sufficiently emphasized that in antiquity only the *apodeixis* would have been thought of as possibly subject to logical rules, and it is often a very small portion of a Euclidean proposition. I would now like to argue that Euclid does not show an awareness of one of the most basic ideas of logic, logical form. Characteristically logicians make clear the importance of form for determining the validity of an argument by obvious artificial devices. When Aristotle writes, "If *A* is predicated of all *B* and *B* of all *C*, necessarily *A* is predicated of all *C*", he uses the letters '*A*', '*B*', '*C*' to indicate the truth of the assertion (or correctness of the inference), no matter what terms are put in their place. The Stoics make a similar claim when they call "If the first then the second; but the first; therefore the second" valid: any substitution of sentences for ordinal number words produces a correct inference.

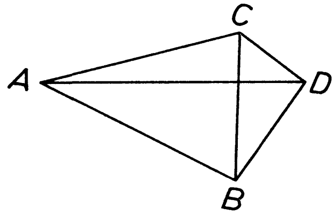
Of course, artificial indications of form are not likely to occur in applications of logic, but a series of correct deductive arguments cannot be said to show a sense of logic unless it shows a sense of form. But Greek mathematics does not show this sense. In it one finds parallel proofs of separate cases which could be treated simultaneously with only slight generalization. In the *Elements* there are separate proofs of properties of tangent and cutting circles when only the points of contact are relevant.[13] Better known in Euclid's separate treatment of one and the other numbers[14] and of square and cube numbers when all that is relevant is one number's being multiplied by itself some number of times.[15] Similar examples can be found in Archimedes and Apollonius. The usual explanation of this proliferation of cases invokes the concreteness of Greek mathematics. What is insufficiently stressed is how a sense of derivation according to logical rules, had it existed, would have undercut this concreteness. Greek geometers obviously trusted their geometric intuition much more strongly than any set of logical principles with which they may have been familiar.

The proof of I,19 presented above is logically very elementary. One has a set of alternatives all but one of which imply an absurdity, and so one infers the remaining alternative. A person with a sense of logic probably would not bother to carry out such a proof with Euclid's detail even once. But he certainly would not repeat the same proof with different subject matter several times. Euclid repeats the proof exactly in deriving I,25 from I,4 and 24, and V,10 from V,8 and 7. Another example is perhaps even more surprising. Euclid repeatedly moves from a proof of a proposition of the form $(x)(Fx \rightarrow Gx)$ to an explicit proof of $(x)(-Gx \rightarrow -Fx)$: assume $-Ga$ and Fa; then, since all F are G, Ga, contradicting $-Ga$. I have noticed five cases in which such an argument is carried out and two others in which the stylized argument is avoided.[16]

One of the main themes of nineteenth-century mathematics was the demand for complete axiomatization, and one of the main charges levelled against Euclid was his failure to make explicit all of the assumptions on which his proofs relied – in particular, assumptions about continuity or betweenness.[17] The absence from the *Elements* of first principles covering these assumptions is another indication of the intuitive character of their work, but it does not seem to me to throw light on the question whether Euclid *wished* to axiomatize his subject completely. I do not know what Euclid would have said if challenged to establish the existence of the point C in which the two circles of the proof of proposition 1 cut each other. But I do believe that he intended to make explicit in the postulates of book I all *geometric* assumptions to be used *in book I*. I stress 'in book I' because there is no reason to suppose that Euclid intended his postulates to suffice for the whole of the *Elements*, since they do not in fact suffice, since they are stated within book I, and since the *Elements* include the theory of ratios, arithmetic, and solid geometry. I stress 'geometric' because Euclid's proofs depend on other more general assumptions, some of which are stated in the common notions but most of which are not.

Discussion of the common notions is complicated by the issue of interpolation. I shall here simply state my view that only the first three are due to Euclid.[18] At the end of the paper I shall suggest why the other common notions were added. In any case even the most extensive list of common notions in the manuscripts is inadequate to cover all of Euclid's inferences. I illustrate this point by reproducing in outline a segment of the *apodeixis* (a *reductio*) of I,7.

(i) angle ACD equals angle ADC;
(ii) therefore angle ADC is greater than angle DCB;
(iii) therefore angle CDB is 'much' greater than angle DCB.



In this argument, (i) is properly derived from earlier assumptions. (ii) would seem to be derived from (i) plus

(iv) angle ACD is greater than angle DCB,
and the general principle
(v) $(u)(v)(w)$ (u equals v & v is greater than $w \rightarrow u$ is greater than w).

(iv) may be justified by reference to the common notion numbered 8 by Heiberg,[19] which asserts that the whole is greater than the part; more probably it is simply a truth made obvious by the diagram. The principle (v) is nowhere stated explicitly by Euclid, although it would seem to be neither more nor less obvious than the first common notion. Approximately the same thing can be said about the inference to (iii), which follows from (ii) plus

(vi) angle CDB is greater than angle ADC,

and the principle of transitivity for 'greater than', again a principle equally as obvious as the first common notion. I mention these tacit principles to show that the deductive gaps in the *Elements* occur at a much more rudimentary level than the level of continuity or betweenness. But more important, this example, which could be buttressed with many others, seems to me to shift the burden of proof to those who claim that

Euclid intended to produce a complete axiomatization of even book I.

I have so far concentrated primarily on book I of the *Elements* because I believe that, at least as far as logic is concerned, it is Greek mathematics *par excellence* and because it seems to be the main contact point between later Greek logic and mathematics. However, I would like now to consider book V of the *Elements*, which has been described by some scholars as (more or less) formal in the logical sense.[20] There is no question that the theory of proportion of book V is in a way abstract; but, as I hope to make clear, the abstraction involved does not yield a theory based on logic. Rather it yields a theory only slightly less concrete than Greek geometry or arithmetic.

The theory of book V represents Eudoxus's solution to the problem of dealing mathematically with the relation of one quantity to another when the relation cannot be represented as a ratio between two integers. Aristotle apparently refers to this theory and praises it for a kind of abstraction.

> Another case is the theorem about proportion, that you can take the terms alternately; this theorem used at one time to be proved separately for numbers, for lines, for solids, and for times, though it admitted of proof by one demonstration. But because there was no name comprehending all these things as one – I mean numbers, lengths, times, and solids, which differ in species from one another – they were treated separately. Now however, the proposition is proved universally; for the property did not belong to the subjects *qua* lines or *qua* numbers, but *qua* having a particular character which they are assumed to possess universally.
>
> (*Posterior Analytics*, 1.5.74a17–25, transl. by T. Heath)

Aristotle here writes as if the whole matter were terminological, as if separate proofs of the law

$$A \text{ is to } B \text{ as } C \text{ is to } D \rightarrow A \text{ is to } C \text{ as } B \text{ is to } D$$

were given for different kinds of objects simply because no one term covered them all. But it is generally agreed that Eudoxus did not just supply a new term, 'magnitude' (*megethos*), in the *Elements*; he provided a new foundation for the theory of proportion. This foundation survives in Definitions 5 and 7 of book *V*.

DEFINITION 5. Magnitudes are said to be in the same ratio, the first to the second and the third to the fourth, when, if any equimultiples whatever be taken of the first and the third and any equimultiples whatever of the second and the fourth, the former equimultiples alike exceed, are alike equal to, or alike fall short of, the latter equimultiples respectively, taken in corresponding order.

DEFINITION 7. When of equimultiples the multiple of the first magnitude exceeds the multiple of the second but the multiple of the third does not exceed the multiple of the fourth, then the first is said to have a greater ratio to the second than the third has to the fourth.

In modern notation:[21]

$$\begin{aligned}(5)\qquad & (A, B)=(C, D)\leftrightarrow \\ & (m)(n)[(m\cdot A>n\cdot B\rightarrow m\cdot C>n\cdot D)\ \& \\ & (m\cdot A=n\cdot B\rightarrow m\cdot C=n\cdot D)\ \&\ (m\cdot A<n\cdot B\rightarrow m\cdot C<n\cdot D)].\end{aligned}$$

$$(7)\qquad (A, B)>(C, D)\leftrightarrow(Em)(En)(m\cdot A>n\cdot B\ \&\ -(m\cdot C>n\cdot D)).$$

In these definitions comparisons of size between ratios are reduced to comparison of size between multiples of magnitudes. To see what the definitions mean, one need only think of A, B, C, D as real numbers, (X, Y) as X/Y, 'm' and 'n' as ranging over integers, and give '$\cdot$', '$>$', '$<$', and '$=$' their standard meanings. Definition 5 is then equivalent to

$$(5')\qquad \frac{A}{B}=\frac{C}{D}\leftrightarrow(m)(n)\left[\left(\frac{A}{B}>\frac{n}{m}\rightarrow\frac{C}{D}>\frac{n}{m}\right)\&\left(\frac{A}{B}=\frac{n}{m}\rightarrow\frac{C}{D}=\frac{n}{m}\right)\&\left(\frac{A}{B}<\frac{n}{m}\rightarrow\frac{C}{D}<\frac{n}{m}\right)\right].$$

But A/B and C/D may be thought of as arbitrary real numbers, since any real can be represented as a ratio of two reals and any such ratio represents a real. Thus, Definition 5 can be thought of as saying that two reals are equal if they make the same cut in the system of rationals – Dedekind's account of equality for reals.[22] If the same interpretation is applied to Definition 7, it becomes

$$(7')\qquad \frac{A}{B}>\frac{C}{D}\leftrightarrow(Em)(En)\left(\frac{A}{B}>\frac{n}{m}\ \&\ -\left(\frac{C}{D}>\frac{n}{m}\right)\right),$$

i.e., a first real is greater than a second if and only if there is a rational n/m separating them.

In terms of Greek mathematics one remarkable feature of Definitions 5 and 7 is that they attach relatively abstract explanations to the relatively intuitive notions of equality and inequality of ratio. And the explanations are the basis for proving some intuitively obvious facts, e.g.,

$$(\text{V}.7)\qquad A=B\rightarrow(A, C)=(B, C);$$

$$(\text{V}.11)\qquad (A, B)=(C, D)\ \&\ (E, F)=(C, D)\rightarrow(A, B)=(E, F).$$

Intuitions concerning ratios are undoubtedly intended to play no role in the derivations of book V. However, the derivations are not purely logical. Euclid makes constant use of addition, subtraction, multiplication, and division of magnitudes – operations which are characterized nowhere in Greek mathematics. He also assumes laws governing the performance of these operations and laws governing comparisons of size.[23]

The tacit assumptions in book V should probably not be attributed to intuitions about magnitudes and operations on them. For Aristotle's remarks show that 'magnitude' is intended in a general sense. And there is no single intuitive notion of, say, addition for all the different kinds of objects to which the word is supposed to apply. Moreover, in other parts of Greek mathematics which are either Eudoxus's work or stem from it, the operations in question are performed on geometric objects (e.g., circles in *Elements* XII,2; parabolic segments in Archimedes's *Quadrature of the Parabola*) for which the operations could not be given a precise intuitive (i.e., constructive) sense. This deviation from the generally constructive tendency of Greek mathematics is probably not an oversight. Rather, the deviation represents the only available means of solving certain problems. So too in the theory of proportion Eudoxus deviates from the generally intuitive character of Greek mathematics, reducing the theory to generalized notions of magnitude, addition, multiplication, etc. But these notions remain informal. No attempt is made to characterize them by means of first principles. Hence the underpinning of the theory of proportion is the theory of magnitudes rather than logic.

3. MATHEMATICS IN THE *Prior Analytics*

In his systematic presentation of the categorical syllogism in the first twenty-two chapters of the *Prior Analytics*, Aristotle never invokes mathematics. His examples are always of the 'white'-'man'-'animal' variety, and they suggest a close connection between Aristotle's logic and the somewhat mysterious dialectical activities associated with Plato's Academy.[24] The difficulty of fitting mathematical argument into syllogistic form may explain the absence of mathematical references in these chapters. But even in later chapters where Aristotle does invoke mathematics to support some points, a substantial majority of his considerations are either directly pointed at dialectical argument or more obviously relevant to it than to anything else. It seems clear to me that mathematics could not have played in the development of Aristotle's syllogistic anything like the role it played in the development of modern quantification theory. However, it is perhaps worthwhile to examine the mathematical references in the *Prior Analytics* to determine what role mathematics did pay. I first describe references which have no special relevance to the categorical syllogism.

(A) I.30.46a19–22. Aristotle illustrates the empirical basis of our knowledge of the first principles of a deductive science by reference to astronomy, presumably of the kind found in Euclid's *Phenomena* and Autolycus's *On the Moving Sphere* and *On Risings and Settings.*

(B) I.31.46b26–35. Aristotle invokes the incommensurability of the side of a square with its diagonal to illustrate the impossibility of establishing an unknown fact by means of Platonic division.

(C) I.41.49b33–50a4 is a difficult passage to interpret. Aristotle compares his use of *ekthesis* to the geometer's calling 'this line a foot long and that line straight and breadthless when it is not.'[25] Apparently Aristotle is thinking of the *ekthesis* of a geometric proposition and pointing out that the diagram to which the geometer seems to be referring may not satisfy the description he gives and yet does not affect the correctness of his argument. Ross[26] points out the different ways in which Aristotle uses the word *ekthesis*. None of them provide a satisfactory basis for interpreting Aristotle's remark here. Yet, whatever Aristotle means, he is clearly only making an analogy between his use of *ekthesis* and geometric *ekthesis.* His point would apply equally well whatever logical principles are taken to be involved in mathematical argument.

(D) II.16.65a4–7. Aristotle illustrates 'begging the question' with a brief reference to "those who think they draw parallel lines". A satisfactory explanation of this passage would throw light on the history of mathematics but not on syllogistic. For the illustration occurs in a general description of 'begging the question' and would be compatible with any deductive logic.

(E) II.17.65b16–21 and 66a11–15 are equally general. In the former Aristotle gives a presumably fictitious example of a *reductio ad absurdum* in which the absurdity is not attributable to the hypothesis refuted, namely, an attempt to derive a Zenonian paradox from the hypothesis of the commensurability of the side of a square with its diagonal. In the second he illustrates that a falsehood may follow from more than one set of premisses by means of another mathematically fascinating example: 'Parallels meet' follows from 'The interior [angle] is greater than the external' and from 'The angles of a triangle are greater than two right angles'. Since what Aristotle says does not depend in either case on the form of derivation involved, there is no reason to connect these passages with the categorical syllogism.

The remaining references to mathematics in the *Prior Analytics* have a much more obvious connection with syllogistic. The first is perhaps the most important. Having run through the various figures of the various forms of syllogism, Aristotle turns in I.23 to establishing a very general claim: every syllogism in the general sense (i.e., every deductive proof) is a syllogism in the technical sense (i.e., a categorical syllogism). He repeats this claim more than once in the *Prior Analytics*, and there can be no doubt that Aristotle includes *mathematical* proofs among syllogisms in the general sense. His first step in establishing the claim is to assert, without justification, that the conclusion of every proof is a categorical proposition.

Necessarily every proof and every syllogism proves that something belongs [to something] or does not belong, and either universally or in part. (40b23–25)

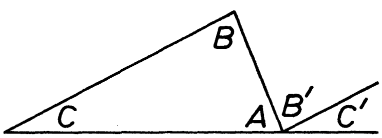
It is easy enough from our standpoint to produce counterinstances to this assertion, but from Aristotle's it is not. Consider an example he uses commonly, the proposition which Euclid states as "the three interior angles of any triangle are equal to two right angles" (*Elements* I,32, second part). Aristotle renders this proposition rather succinctly as 'Every triangle has two right angles'.[27] A more precise rendering would be 'Every triangle has its interior angles equal to two right angles'. The imprecision is indicative of Aristotle's casual attitude toward translation into categorical form. Even more significant is his casual attitude toward the analysis of categorical propositions into terms. According to him, the terms in 'Every triangle has two right angles' are 'triangle' and 'two right angles'. It seems clear, however, that the verb 'have' must be included in the predicate of the proposition, since what is predicated of every triangle in I,32 is having two right angles, not being two right angles. Aristotle apparently considers such distinctions irrelevant as far as deduction is concerned. In *Prior Analytics* I.38 he considers a number of valid arguments which, according to him, differ from categorical syllogisms only because of the grammatical case of one of the terms, e.g., "If wisdom is knowledge and wisdom is of the good, the conclusion is that knowledge is of the good" and "Opportunity is not the right time because opportunity is god's, but the right time is not". For Aristotle these arguments are syllogisms with the terms 'wisdom', 'knowledge', 'good' and 'opportunity', 'right time', 'god' respectively.

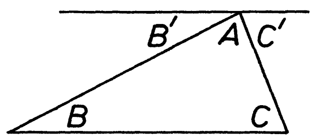
We say generally about all instances that the terms are always to be set out in the nominative case, e.g., 'man' or 'good' or 'opposites', not 'of man' or 'of good' or 'of opposites', but the premisses are to be taken with the appropriate case, e.g., 'equal' with the dative, 'double' with the genitive, 'striking' or 'seeing' with the accusative, or in the nominative, e.g., 'man' or 'animal', or if the noun occurs in the premiss in some other way. (I.36.48b39–49a5)

As Łukasiewicz has pointed out, "Aristotelian logic is formal without being formalistic."[28] That is to say, Aristotle is throughly aware that the validity of an argument depends on its form, but he is not very strict in his determination of the form of a statement in an argument. The freedom of paraphrase which he allows himself in representing statements may well have been a major factor in his conclusion that a proof is always of a categorical statement. Certainly, given Aristotle's liberal standards, all the theorems in Euclid could be transformed into categorical statements. When Aristotle wrote the *Prior Analytics* probably no one was aware of the possibility of a formalistic logic. But the Stoics apparently did move toward one.[29] Unfortunately the idea does not seem to have spread outside Stoic circles. Alexander of Aphrodisias, commenting on Aristotle's remark that words and phrases with the same meaning may be interchanged in arguments, asserts: "The syllogism does not have its being in the words but in what they signify".[30] Even if one believes this assertion, one cannot deny that the insistence on strict formalization characteristic of modern logic has made clear a number of things which reliance on meaning obscures. As we shall see, later Peripatetics were able to defend Aristotle's claim of universality for the categorical syllogism because they were content with rather loose formulations of arguments.

It would be impossible to refute Aristotle's liberal attitude toward translation into categorical form, although the success of modern logic surely shows the attitude to be unfortunate. However, one might even concede that only categorical propositions are proved in mathematics without admitting the syllogistic character of mathematical proof. The analysis of *Elements* I,1 was intended to show how far from the categorical syllogism Euclidean reasoning is. Aristotle, however, produces in *Prior Analytics* I.23 a general argument for the universality of the categorical syllogism. The main point of the argument is the need for a middle term to establish a categorical proposition. There is no reason to examine the argument in detail, since it presupposes the universality of reasoning based on the predicational relation of terms. The important point is that no thorough investigation of mathematical proof would support Aristotle's claim.

Aristotle's own mathematical examples are consistently vague. In I.35 he writes as though the proof that the angles of a triangle are equal to two right angles requires only the proper specification of a middle term. Almost certainly the proof he has in mind involves the drawing of a parallel line, as in the first or second diagram, and arguing that angle B = angle B',





angle C = angle C', and angle A + angle B + angle C = two right angles. In such a proof the terms 'triangle' and 'two right angles' cannot function as categorical terms because the proof involves breaking the triangle and the two right angles into parts, and the spatial relations of the parts are crucial. Elsewhere Aristotle simply asserts that categorical syllogisms are used in the derivation of a contradiction from the assumption of the commensurability of side and diagonal (I.23.41a21–37 and I.44.50a29–38). And, to take the most extreme case of all, he is content to describe a very elaborate attempt of Hippocrates to square the circle[31] with the following cryptic remark:

> If D is 'to be squared', E 'rectilinear', F 'circle', if there be only one middle for the [proposition] EF, the circle with lunes becoming equal to a rectilineal [figure], we should be close to knowledge. (II.25.69a30–34)

Here Aristotle apparently thinks of Hippocrates's quadrature of a circle plus a lune as the insertion of a middle term between 'rectilinear' and 'circle'. In itself this interpretation of the quadrature is dubious, but the crucial point is that no concern is shown for the details of Hippocrates's reasoning. Aristotle is contented with a vague statement of the general result.

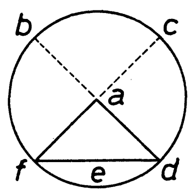
The closest Aristotle comes in the *Prior Analytics* to considering a mathematical proof in detail is in I.24 where he wishes to show that at least one premiss of a valid syllogism must be universal. This wish is somewhat strange, since a simple survey of the detailed presentation in the first twenty-two chapters would suffice to establish the point. Aristotle uses examples to make it plausible. The first is non-mathematical.

For let it be put forward that musical pleasure is worthwhile. If pleasure is assumed to be worthwhile but 'all' is not added, there won't be a syllogism. And if it is taken to be some pleasure, then, if it is a different pleasure [than musical pleasure], it does not help for the thesis, and if it is the same, the question is begged. (41b9–13)

Here Aristotle seems to lose sight completely of the notion of formal validity which is so crucial in his original presentation. He could have simply pointed out that the argument with 'some' is invalid because it is of a form already shown to be invalid, or, more directly, because there are interpretations which make the premisses true and the conclusion false. In any case, Aristotle continues:

This is made clearer in geometrical propositions, e.g., that the angles at the base of an isosceles triangle are equal. Let the straight lines *A* and *B* be drawn to the center. Then if one takes (1) the angle *AC* to be equal to the angle *BD* without assuming (A1) the angles of a semicircle to be equal in general, and again that (2) *C* is equal to *D* without adding that (A2) all angles of the segment are equal, and further that since the whole angles are equal and the subtracted angles are equal, (3) the remainders *E*, *F* are equal without assuming that (A3) if equals are subtracted from equals the results are equal, he will beg the question. (41b13–22)

Aristotle's presentation here is somewhat obscure and hardly rigorous by Euclidean standards. But the drift of the proof which he describes is clear. In the diagram, *bcdef* is a circle with center *a*. According to Aristotle,



the following argument involves *petitio principii*:

(1) mixed angle ade = mixed angle afe;

(2) mixed angle *fde* = mixed angle *dfe* ;
(3) therefore, rectilineal angle *adf* = rectilineal angle *afd*.

The addition of three general premisses is required to correct the reasoning:

(A1) The angles made by diameters and circumferences of circles are always equal.
(A2) The two angles made by a chord and the circumference of a circle and on the same side of the chord are equal.
(A3) If equals are subtracted from equals, the results are equal.

Quite clearly the proof which Aristotle has in mind here is logically very similar to the *apodeixis* of *Elements* I,1. This proof is slightly more complicated (and less syllogistic) because there is a subtraction involved between steps (2) and (3). Exactly how Aristotle would have tried to syllogize the proof is anybody's guess. There is no evidence that he ever did try, and I suspect that he never considered the problem of reducing mathematical proof to syllogistic form in a systematic way. In the present passage he is simply using a mathematical example as inductive evidence for his claim that a valid syllogism requires a universal premiss. And perhaps Aristotle is here using the word 'syllogism' in the broader rather than the narrower sense. His failure to refer to the earlier chapters of the *Prior Analytics* for a clear substantiation of his claim, his inconclusive treatment of the argument yielding 'Musical enjoyment is worthwhile', and the vagueness of his discussion of the mathematical proof incline me to think so. I would be certain except for Aristotle's references to the modes and figures of the syllogism at the end of I.24.

It looks, then, as though Aristotle did not study mathematical proof carefully or make any detailed attempt to vindicate his claims for the universality of syllogistic. A general argument based on a rather superficial analysis of mathematical theorems was sufficient for his purposes. This point of view is confirmed by the semi-mathematical arguments in other Aristotelian and pseudo-Aristotelian works. None of them show any closer relation to syllogistic than the main texts of Greek mathematics do. Further evidence is provided by Eudemus's presentation of Hippocrates's quadratures of lunes and circles plus lunes.[32] Eudemus was a pupil of Aristotle with at least some interest in logic,[33] but nothing in his presentation suggests an interest in connecting mathematics with syllogistic. Alexander of Aphrodisias is too late a figure to serve as a direct indicator of Aristotle's own ideas, but the surviving parts of his commentaries on the *Organon* are our best source of information on what became of those ideas among the later Peripatetics. Alexander makes clear in many passages that, for him, the doctrine of the universality of the categorical syllogism has the status of a dogma. In one such passage he discusses Aristotle's claim that the derivation of a contradiction from the assumption of the commensurability of the side of a square with its diagonal is syllogistic.[34] Alexander reproduces a protracted but essentially correct derivation that is no more syllogistic in style than any proof in the *Elements*. He simply asserts that the derivation is syllogistic. For him any interesting conclusive argument must be a categorical syllogism.

Thus far I have argued as if Aristotle acknowledged no form of conclusive argument other than the categorical syllogism. In fact he does acknowledge a general class of non-syllogistic argument which he calls argument from a hypothesis.[35] An especially important member of the class is the *reductio ad absurdum*. However, Aristotle always treats the general class and its most important member separately, and I shall follow him in my discussion. Argument from a hypothesis is for Aristotle basically *modus ponendo ponens*. Wishing to prove Q, one adds $P \rightarrow Q$ as a hypothesis and proves P. Aristotle represents argument from a hypothesis as a form of dialectical reasoning. The hypothesis $P \rightarrow Q$ is a matter of agreement between two opponents. The opponent who denies P but concedes $P \rightarrow Q$ is declaring a proof of Q unnecessary once a proof of P has been found; he is not providing a premiss which might be used in a proof of Q. Thus Aristotle does not conceive of *modus ponens* as a rule of logical inference. As far as he is concerned, the proof in an argument from a hypothesis is the proof of P. Since he assumes that P will be categorical, he assumes that the proof of P will be a series of categorical syllogisms. Łukasiewicz argued that Aristotle was oblivious to the use of rules of propositional logic in his own development of syllogistic.[36] His obliviousness to their use in mathematics seems at least as clear.

On the other hand, *reductio* arguments are an obvious feature of mathematics. And Aristotle's standard example of a *reductio* proof is the indirect derivation of the incommensurability of the side of a square and its diagonal. Aristotle's analysis of *reductio* is obviously intended to be like his analysis of argument from a hypothesis, but the details of the analysis of *reductio* are less clear. *Prima facie*, one would expect the hypothesis of a *reductio* to be the assumption refuted; but, if it is, the analogy with argument from a hypothesis breaks down. Unfortunately Aristotle contents himself with saying that the hypothesis in a *reductio* is not agreed to in advance "because the falsehood is obvious" (I.44.50a35–38). The obvious falsehood would seem to be the contradiction derived from the assumption refuted. In saying that no advance agreement is made, Aristotle is apparently again envisaging a dialectical situation: one person claims P; the other derives a contradiction from P; the falsehood is so blatant that no explicit agreement is needed to get the first person to abandon P. One might then consider the hypothesis of a *reductio* to be the law of propositional logic '$(P \rightarrow (Q \ \& \ -Q)) \rightarrow -P$', but there is no evidence that Aristotle even tried to reformulate it. For him the crucial points are (1) the *reductio* part of an indirect proof is syllogistic, and (2) the nonsyllogistic part is a matter of tacit agreement rather than logic.

However, *reductio* is a part of mathematics and is recognized as such by Aristotle. Was he then forced to recognize a non-syllogistic feature of mathematics? Apparently not, for Aristotle also realized that "everything which can be inferred directly (*deiktikos*) can be inferred by *reductio* and *vice versa*, and by the same terms" (II.14.62b38–40). In other words, $(A \ \& \ B) \rightarrow C$ is a valid categorical syllogism if and only if $(A \ \& \ -C) \rightarrow -B$ is (with negated statements properly formulated). Thus any theory whose logic is syllogistic has no need of *reductio* proof. It is unfortunate that no one ever tried to illustrate this truth about the categorical syllogism by recasting indirect proofs from mathematics into direct ones. An attempt to do so would have made the limitations to the categorical syllogism obvious.

Aristotle seems, then, to have had a largely *a priori* conception of the relation between his logic and mathematical proof. He may have taken the formulation of mathematical theorems into account in trying to justify his estimation of the significance of the categorical proposition in demonstrative science, but his notion of the categorical proposition was so broad that virtually any general statement would satisfy it. On the other hand, Aristotle does not seem to have looked at mathematical proof in any detail, at least as far as its logic is concerned. He recognizes some common features of mathematical proof, e.g., the use of *reductio ad absurdum* and the reliance on universal assumptions, but he is apparently content to rely on the abstract argument of I.23 to establish the adequacy of syllogistic for mathematics. His Peripatetic successors do not seem to have gone much beyond him either in logic or in the logical analysis of mathematical proof.

4. Stoic Logic and Greek Mathematics

Some of the Stoics do seem to have shown an awareness of the complexity of mathematical proof. Unfortunately the scatteredness and scantiness of the evidence makes it difficult to determine the details of Stoic logical theory and, in particular, to assign a chronology to its development. Recent interpreters of Stoic logic have disagreed sharply with their predecessors on questions of analysis and evaluation, but both have forsworn the attempt to provide a chronology. And certainly there is little hope of reconstructing a precise and detailed chronology, since probably the majority of sources describe only "what the Stoics (or dogmatists or recent philosophers) say" about some question. On the other hand, some sources attribute particular doctrines to particular people. The material quoted by Diogenes Laertius from Diocles Magnes is especially rich in these attributions, and they are almost certainly reliable. Of course, when a doctrine is assigned to a person we cannot be sure that he was the first person to espouse it, but it seems to me we should assume he was in the absence of other negative evidence or of countervailing systematic considerations. Almost equal strength, I think, should be assigned to associations of doctrines with students or followers of a person, usually referred to as "those about" (*hoi peri*) him. Normally there are no grounds for distinguishing the views of "those about a person" from the views of the person himself.

What I have said so far about scholarly methodology is uncontroversial. The crucial issue arises with respect to ascriptions to "the more recent philosophers" (*hoi neōteroi*). The more recent philosophers are almost always Stoics, but it is difficult to determine the chronological boundary between more recent philosophers and others. In some authors the *neōteroi* seem to be Stoics in general or at least to include Chrysippus. Iamblichus[37] speaks generally of the original philosophers and more recent ones and goes on to discuss the views of Plato, Aristotle, and Chrysippus. Galen associates with the more recent philosophers two terms (*diezeugmenon axiōma, sunēmmenon axiōma*[38]) which are certainly Chrysippean, as Galen himself says elsewhere in the case of one of them.[39] However, the important source to be evaluated is Alexander of Aphrodisias, who uses the phrase *hoi neōteroi* more often than anyone else. As far as I have been able to determine, the following characterization holds for his usage. On occasion Alexander does contrast the *neōteroi* with the older Peripatetics (rather than the older Stoics).[40] He also sometimes uses the word *neōteroi* interchangeably with 'Stoics'[41] and sometimes associates with *neōteroi* doctrines or practices common in the Stoic school.[42] But he never ascribes to the *neōteroi* terminology or doctrine elsewhere attributed explicitly to Chrysippus. And in some cases terminology or doctrine associated with the *neōteroi* by Alexander can be determined with reasonable plausibility to be post-Chrysippean.

The most certain case is the idea of the argument with one premiss, e.g., 'You breathe; therefore you are alive',[43] which Sextus Empiricus explicitly dissociates from Chrysippus and attributes to Antipater (*flor.* 2nd cent. B.C.).[44] Another almost equally certain case is the use of the word *proslambamenon* or *proslēpsis*[45] for the 'minor premiss' of a hypothetical syllogism. At least Diocles Magnes ascribes to those about Crinis, a contemporary of Antipater, the description of an argument as consisting of *lēmma, proslēpsis*, and *epiphora.*[46] In his commentary on the *Topics* Alexander says that the *neōteroi* call a certain kind of question a *pusma*, a word used for questions requiring more than a 'yes' or 'no' answer.[47] There is some reason to regard this word as post-Chrysippean, since from the book titles in Diogenes Laertius it appears that Chrysippus used the word *peusis* with the same meaning.[48] The ground, however, is not very firm because *peusis* and *pusma* seem to have been used interchangeably in later antiquity.

In the matter of arguments, what can be attached most firmly to Chrysippus are the five *anapodeiktoi.*[49] None of the obscure four *themata* are ever ascribed explicitly to him, nor does the word *thema* occur in the list of his works given by Diogenes Laertius. Alexander attributes a second and a third *thema* to the *neōteroi.*[50] Perhaps Chrysippus did put forward some *themata* for reducing arguments to his five *anapodeiktoi.* But Alexander's ascription of the second and third *themata* to the *neōteroi*, combined with the absence of any clear presentation of the *themata* in surviving discussions of Stoic logic, suggests at least that the *themata* never became fixed in the way in which the *anapodeiktoi* more or less were. The other arguments which Alexander attributes to the *neōteroi* are, according to him, useless. They are the *diphoroumenoi* (e.g., 'If it is day, it is day; but it is day; therefore it is day'), the *adiaphorōs perainontes* ('Either it is day or it is night; but it is day; therefore it is day'), the so-called infinite matter,[51] arguments semantically but not formally equivalent to categorical syllogisms and called hyposyllogisms,[52] and correct arguments which are not formally valid – called *amethodōs perainontes*, unsystematically conclusive ('The first is greater than the second; the second is greater than the third; therefore the first is greater than the third').[53] None of these arguments is ever associated with a specific person. To dissociate them from Chrysippus there is only Alexander's apparently consistent use of the word *neōteroi* and the absence of any titles containing the words *diphoroumenoi, adiaphorōs perainontes*, 'infinite matter', 'hyposyllogism,' or 'unsystematically conclusive' in Diogenes Laertius's long list of the works of Chrysippus. If the arguments are dissociated from Chrysippus, a rather clear picture of one aspect of the development of Stoic logic emerges. In the mid-third century B.C. Chrysippus developed or codified the propositional logic which became the core of Stoic logic. After him, in the period of transition from the old to the middle Stoa, other Stoics introduced into consideration certain curious propositional arguments and other apparently valid arguments not satisfying either Stoic or Peripatetic accounts of validity.

With this rough chronological framework it is possible to investigate the relation between Stoic logic and Greek mathematics somewhat more precisely. I shall consider propositional logic first. I have already given an example of a propositional argument in the *Elements*. Familiarity with modern logic makes it easy to find many more, both explicit and implicit. However, the evidence indicates rather strongly that no Stoic ever conceived of propositional logic as a basic tool of mathematics. Mathematical illustrations of propositional arguments are practically non-existent. There are none in Sextus Empiricus or Diogenes Laertius or Alexander, for example. Indeed, the only extended illustrations are given in the sixth century A.D. by John Philoponus in his discussion of Aristotle's treatment of argument from a hypothesis.[54] The most interesting part of the discussion for my purposes is Philoponus's claim that *reductio ad absur-*

dum involves application of two Stoic *anapodeiktoi*, the second and the fifth. He illustrates his claim in terms of Aristotle's example, the proof that the side and the diagonal of a square are incommensurable.

Fifth *anapodeiktos*:

(1) The diagonal is either commensurate or incommensurate with the side;
(2) But it is not commensurate (as I will show);
(3) Therefore it is incommensurate.

Second *anapodeiktos*:

(4) If the diagonal is commensurate with the side, the same number will be even and odd;
(5) But the same number is not even and odd;
(6) Therefore the diagonal is not commensurate with the side.

Philoponus presumably thinks of (1) and (5) as immediate truths, and, like Aristotle and Alexander, he insists that (4) requires a proof by categorical syllogism. Thus, although Philoponus grants Stoic propositional logic more status than Alexander does, he still maintains the false Peripatetic view of the dominance of the categorical syllogism.

It is, of course, possible that the propositional part of Philoponus's analysis ultimately derives from an early Stoic source. But such a derivation seems unlikely. For Philoponus does not formulate arguments in the Stoic manner. He does not place the word 'not' at the front of the sentence in (2), (5), and (6); he does not formulate (1) as a disjunction but as a simple sentence with a disjunctive predicate; and he formulates (4) artificially, perhaps to make it seem more categorical. (Literally (4) runs: The diameter with the side, if it is commensurate, the same number will be even and odd.') There are similar features of Philoponus's whole discussion of hypothetical syllogisms which indicate that its origin is in later eclectic thinking. However, the exact origin is not known to me. I have traced it back as far as Proclus who, in commenting on proposition 6 of book I of the *Elements*, refers to the role of the second *anapodeiktos* in indirect proofs:

In reductions to impossibility the construction corresponds to the second of the hypotheticals. For example, if in triangles having equal angles the sides subtending the equal angles are not equal, the whole is equal to the part; but this is impossible; therefore, in triangles having equal angles the sides subtending the equal angles are themselves equal.[55]

Proclus, of course, taught Ammonius on whose lectures Philoponus's commentary on the *Prior Analytics* seems to have been based.

Thus, Chrysippean propositional logic would not seem to have been developed out of reflection on mathematics. Any connection between Stoic logic and Greek mathematics must be sought in the later refinements already mentioned. And among these there is one obvious candidate for consideration, the unsystematically conclusive argument. The example given above is clearly mathematical. So is another, also due to Alexander, the inference to the equality of *CA* and *CB* in *Elements* I, 1.[56] But the following fairly common example shows that the domain of unsystematically conclusive argument extends beyond mathematics: 'It is day; but you say that it is day; therefore you speak the truth'[57]

The first question I wish to consider is how the conception of these arguments arose. After discussing categorical and hypothetical syllogisms, Galen introduces in chapter xvi of his *Institutio Logica* a third form of syllogism, namely, the relational (*kata to pros ti genesthai*). He gives examples analogous to the unsystematically conclusive arguments above and mentions the frequency of relational syllogisms in mathematics. Galen apparently takes credit for the name 'relational' and for recognizing that relational syllogisms depend for their validity on some axiom, by which he means a self-evident proposition. There is no reason to deny Galen's origination of the term 'relational', since it is used in this way only in the *Institutio*. However, it is important not to read too many modern connotations into the term. For there is no evidence that Galen made any attempt to explain the validity of a relational syllogism by reference to what are now called the logical properties of a relation, such as transitivity or asymmetry, or to classify relations in terms of such properties. Indeed, there is no general treatment of relations at all. Each relational argument is to be examined in isolation to determine if there is an axiom which makes it valid.[58] Moreover, many of Galen's examples do not depend on logical properties of relations but on mathematical or semantic truths, e.g., '$(a=2b \ \& \ b=2c) \rightarrow a=4c$' or "'son' is the converse of 'father'". It seems fair to say that Galen calls the arguments he is considering relational because they contain a relation word. He does not conceive of the idea of a logic of relations. And his account of the validity of relational syllogisms as deriving from an axiom would apply to any argument turning on the meaning of some of its terms, even if the terms were not relation words.

In the last chapter of the *Institutio* Galen dismisses from consideration several kinds of argument as being redundant in his presentation of logic. One is "called unsystematic, with which one must syllogize when there is no systematic argument at all".[59] There is no reason to doubt that Galen is referring to unsystematically conclusive arguments and classing them with his own relational arguments. It is not clear, however, in what way relational syllogisms constitute a broader class than unsystematically conclusive arguments. Perhaps all Galen did was to produce a few new examples of such arguments and provide a new label for them. A more important question concerns Galen's claim to originality in his account of the validity of relational syllogisms. At the end of his discussion of the relational syllogism[60] he admits that the Stoic Posidonius (ca. 135-ca. 50 B.C.) called such arguments "valid on the strength of an axiom". It looks, then, as though the fundamental idea of Galen's account was put forward more than two centuries before him.

Moreover, it looks as though the Peripatetics held the same view of unsystematically conclusive arguments as Galen, but in a more specific form. Galen criticizes the Aristotelians for trying by force to count relational syllogisms as categorical.[61] The subsequent discussion in the *Institutio*, supplemented with Alexander's logical commentaries, makes it virtually certain that the Peripatetic way of treating Galen's relational syllogisms was to add a universal premiss corresponding to Galen's axiom and to reformulate the argument as a 'categorical syllogism'. To give one example, Alexander transforms the unsystematically conclusive argument '*A* is greater than *B*; *B* is greater than *C*; therefore *A* is greater than *C*' into

> Everything greater than a greater is greater than what is less than the latter;
> *A* is greater than *B* which is greater than *C*;
> Therefore *A* is greater than *C*.[62]

Galen's criticism of such transformations as forced is mild, to say the least. The transformations make no logical sense. Alexander makes them only because he is bent on defending Aristotle's general claims about the universality of the categorical syllogism.

Galen's own attitude toward the added axiom and the resulting argument is harder to figure out. After criticizing the Aristotelians for forcing relational syllogisms into an arbitrary mold, he goes on to propose reducing the syllogisms to categorical form.[63] But shortly thereafter, he considers a 'reduction' apparently as ridiculous as the one just given and clearly prefers a 'reduction' to a propositional argument by adding a conditional premiss.[64] Galen is so antiformalistic that it is impossible to tell how serious he is about reduction to categorical form. His main stress is on the tacit assumption in relational syllogisms of an axiom, which he usually describes as universal. But one cannot tell whether for him the result of adding the axiom is always a categorical syllogism, always either a categorical or hypothetical syllogism, or sometimes neither. I am inclined to accept the last alternative, but with inconclusive reasons. Galen introduces the relational syllogism as a third form or species of syllogism, and if he believed it was really an enthymemic form of the first two, he could easily have said so. Alexander accepts the first alternative for unsystematically conclusive arguments and is very explicit about it. In any case, Galen and probably every other logician in antiquity showed no interest in developing a special logic to account for relational arguments.

In describing what Galen calls relational syllogisms as valid on the strength of an axiom, Posidonius was probably offering an explanation of the conclusiveness of unsystematically conclusive arguments, which, as Galen's description of unsystematic arguments suggests, were regarded as simply unsystematic – i.e., incapable of analysis. It is uncertain when the *amethodōs perainontes* arguments were first introduced, or in what connection. But the evidence I have given suggests dating their introduction in the second century B.C., that is, between Chrysippus and Posidonius. Probably the connection between these arguments and mathematical proof was not at first recognized or, at least, emphasized. I have already pointed out that some of the acknowledged unsystematically conclusive arguments were not mathematical. However, even the mathematical argument 'A is equal to B; C is equal to B; therefore A is equal to C' cannot have originally been considered in a context like *Elements* I, 1. For there the role of the axiom (common notion) 'Things equal to the same thing are equal to each other' is clear. But apparently *amethodōs perainontes* arguments were thought of as containing no general premisses of this kind.[65] Perhaps, then, Posidonius used mathematical examples like the proof of I, 1 to explain the unsystematically conclusive arguments as valid on the strength of an axiom.[66] Subsequently the Peripatetics claimed that the axiom was always a universal statement which, when added to the argument, turned it into a categorical syllogism.

Posidonius's use of the word 'axiom' (*axioma*) is curious. For the Stoics any proposition is an axiom.[67] Galen's use of 'axiom' to mean 'self-evident proposition' is derived ultimately from Aristotle.[68] Posidonius could, of course, have been using 'axiom' in the standard Stoic sense. He could have been pointing out the possibility of turning any *amethodōs perainōn* argument into a valid propositional argument by adding as an additional premiss the so-called corresponding conditional: the conditional with the conjunction of the original premisses as antecedent and the conclusion as consequent. But to suppose he did this is to accuse Galen of misrepresentation or misunderstanding. Moreover, Posidonius is known to have been a philosophical eclectic. There is no great surprise in his using a Stoic word with a Peripatetic sense.

Proclus's commentary on book I of the *Elements* contains enough references to Posidonius and to his pupil Geminus to confirm Posidonius's interest in the fundamentals of Greek mathematics. Particularly interesting in connection with logic are Proclus's references to Posidonius's replies to an attack on geometry by the Epicurean Zeno of Sidon. Zeno's motivation was probably destructive skepticism,[69] although Vlastos has tried to represent Zeno as a 'not unfriendly' and 'constructive' critic of Euclid's *Elements*.[70] I shall not pursue the question of motivation here because the crucial thing for my purposes is the form of Zeno's criticism. He is classed by Proclus as one who concedes the truth of geometric first principles but insists on the need for further assumptions in order to complete the proofs.[71]

According to Proclus, Posidonius wrote a 'whole book' refuting Zeno's attack on geometry.[72] Unfortunately Proclus refers to this controversy in an explicit way only in connection with *Elements* I, 1.[73] He reproduces only two replies by Posidonius to Zeno. In both Posidonius denies the need for the additional assumption which Zeno claims is required. Nevertheless it seems quite possible that the controversy with Zeno is the source of Posidonius's account of relational arguments as valid on the strength of an axiom. The evidence which I have given for this possibility is sparse and basically circumstantial. To this evidence I would like to add one more consideration. In discussing *Elements* I, 10, the bisection of a straight line,[74] Proclus refers to 'some' who say that "this appears to be an agreed principle in geometry, that a magnitude consists of parts infinitely divisible". In reply Proclus invokes Geminus's statement that the geometers do assume, "in accordance with a common notion", that the continuous is divisible. Later Proclus refers to this assumption as an axiom. Crönert has identified Zeno with the 'some' referred to by Proclus.[75] Perhaps Crönert is right, but in any case replies like the one ascribed to Geminus in the passage under consideration would have to be attributed to Posidonius if a connection is to be made between his controversy with Zeno and his analysis of Galen's relational arguments. The hypothesis I propose is the following: Posidonius may have been unable to fill some of Zeno's alleged gaps in mathematical proofs and may have noticed the correspondence between Stoic unsystematically conclusive arguments and the proofs with gaps. Obviously it is no reply to a critic to call a proof unsystematically conclusive. Nor will it do to invoke the corresponding conditional, since establishing that is tantamount to establishing the correctness of the conclusion directly.[76] Hence Posidonius may have invoked self-evident principles – axioms – to fill the gaps he could not analyze away. And he may have described the proofs with gaps, and unsystematically conclusive arguments in general, as valid on the strength of an axiom.

After the composition of the *Elements* the common notions or axioms were a matter of great controversy, which centered on the need or lack of need for more axioms than the first three.[77] The result of this controversy was the incorporation of a total of ten axioms into the main texts of the *Elements*. The additions are undoubtedly due to a desire to fill alleged gaps in Euclid's argumentation. The date of the inception of this controversy is uncertain. I would like to suggest that it begins with the skeptical attack of Zeno and the more positive reply of Posidonius. The earliest person mentioned by Proclus in connection with the controversy is Heron, who attempted to limit the axioms to three, apparently the first three.[78] It would seem that by Heron's time the list of axioms had already been expanded. Unfortunately Heron's dates are uncertain; scholars have placed him everywhere between 200 B.C. and 300 A.D. Neugebauer's dating of Heron's *floruit* in the first century A.D.[79] seems now to have won general acceptance. We do not know who added to the *Elements* the common notions rejected by Heron. Proclus never mentions Posidonius in connection with the axioms and postulates but does mention Geminus, who wrote extensively on mathematics,[80] several times. Geminus seems to be a plausible but by no means certain candidate.

5. Recapitulation

(1) Aristotle's formulation of syllogistic in the fourth century is basically independent of Greek mathematics. There is no evidence that he or his Peripatetic successors did careful study of mathematical proof.

(2) Similarly, the codification of elementary mathematics by Euclid and the rich development of Greek mathematics in the third century are independent of logical theory.

(3) Likewise, Stoic propositional logic, investigated most thoroughly by Chrysippus in the third century, shows no real connection with mathematical proof.

(4) Subsequent to Chrysippus, *hoi neōteroi* considered various new forms of argument, including the unsystematically conclusive. Some of these new forms of argument may have come from mathematics. However, as the name 'unsystematically conclusive' suggests, no attempt was made to provide a logic for these arguments.

(5) Around the end of the second century B.C. Zeno of Sidon (and perhaps other skeptics and Epicureans) tried to undermine mathematics by pointing out gaps in proofs. Posidonius replied to Zeno, in many cases denying the existence of the gaps. But Posidonius also recognized that some geometric arguments, which resemble unsystematically conclusive arguments, depended on unstated principles. He considered the unstated principles self-evident and therefore called the arguments valid on the strength of an axiom. However, he made no progress in developing a logic to apply to these arguments. The debate over the need for further axioms in geometry continued for centuries and affected the text of the *Elements* itself.

(6) The reawakening of interest in Aristotle's works in the first century B.C.[81] produced a Peripatetic reaction to Posidonius's analysis of ordinary mathematical argument. Aristotle's general remarks about the universality of the categorical syllogism became a dogma to be defended at all costs. Unsystematically conclusive arguments were made systematic by adding

a universal premiss and attempting to transform the result into a categorical syllogism. The attempt was uniformly a failure.

(7) In Galen's *Institutio Logica* there is a more balanced view of unsystematically conclusive arguments, which Galen calls relational. Relational arguments depend for their validity on an additional axiom which is usually universal and usually categorical, but relational syllogisms are distinct from both categorical and hypothetical syllogisms. However, there is no evidence that Galen made any attempt to formulate a logic of relational syllogisms.

The University of Chicago

NOTES

[1] The translations of the *Elements* are by T. Heath, *The Thirteen Books of Euclid's Elements*, 3 vols., Cambridge, England, 1925.

[2] John Philoponus, *In Aristotelis Analytica Priora Commentaria* (ed. by M. Wallies), Berlin, 1905, 246.3–4, gives a similar illustration of the fifth *anapodeiktos*: 'The side is either equal to or greater than or less than the side; but it is neither greater nor less; therefore it is equal'. For details on the *anapodeiktoi* and other aspects of Stoic logic, see B. Mates, *Stoic Logic*, Berkeley 1961.

[3] For example, one reads in *Studies in History and Philosophy of Science* **1** (1970), 372: "And what of Greek geometry? What are its characteristics? It employs no symbols, for it is concerned not with structures formed by relations between mathematical objects, but with the objects themselves and their essential properties. It is not operational, but contemplative; its logic is the predicate logic of Aristotle's *Organon*". A footnote adds: "Indeed, the *Organon* includes, with one or two rare exceptions, no elements of relational logic".

[4] These divisions and their names are taken from Proclus, *In Primum Euclidis Elementorum Librum Commentarii* (ed. by G. Friedlein), Leipzig, 1873, 203.1–210.16. The rigidity which they suggest is fully confirmed by Euclid's *Elements*; and the terms themselves, or forms of them, can all be found in third-century mathematical works. For references, see C. Mugler, *Dictionnaire Historique de la terminologie géométrique des grecs*, Paris 1958.

[5] See 'Investigations into Logical Deduction', in *The Collected Papers of Gerhard Gentzen* (ed. by M. E. Szabo), Amsterdam 1969, pp. 68–81.

[6] As in D. Hilbert, *Foundations of Geometry* (transl. by L. Unger), La Salle, Ill., 10th ed., 1971.

[7] Proclus, *In Primum Elementorum*, 77.7–81.22.

[8] J. Łukasiewicz, *Aristotle's Syllogistic*, Oxford, 2nd ed., 1957, p. 1, asserts that Aristotle does not allow singular terms in syllogisms. If Łukasiewicz is right, then no Euclidean argument would be an Aristotelian syllogism.

[9] See, for example, H. Zeuthen, *Geschichte der Mathematik im Altertum und Mittelalter*, Copenhagen 1896, p. 117.

[10] At the beginning of Galen's *Institutio Logica* (ed. by C. Kalbfleisch), Leipzig 1896, 1.2, the reader is introduced to the idea of proof by means of the following example:

'Theon is equal to Dion; Philon is equal to Dion; things equal to the same thing are also equal to one another; therefore Theon is equal to Philon'.

[11] See Alexander of Aphrodisias, *In Aristotelis Analyticorum Priorum Librum I Commentarium* (ed. by M. Wallies), Berlin 1883, 344.13–20.

[12] For inadequate attempts to explain the move, see Proclus, *In Primum Elementorum*, 49.4–57.8; and J. S. Mill, *A System of Logic*, London, 9th ed., 1875, III.ii.2.

[13] E.g., in III, 5, 6.

[14] E.g., in VII, 9, 15.

[15] E.g., in VIII, 11, 12.

[16] The cases with the stylized proof are VIII, 16, 17 and X, 7, 8, 9. The cases where the possibility of the stylized proof is apparently overlooked are X, 16, 18. Each of these examples except X, 7, 8 actually contains two instances of failure to recognize the elementary logical equivalence.

[17] See, for example, F. Klein, *Elementary Mathematics from an Advanced Standpoint* (transl. by E. R. Hedrick and C. A. Noble), New York 1939, **II**, pp. 196–202.

[18] Heath gives five common notions in his translation of the *Elements*, but in discussing the fourth and fifth (I, 225, 232) he admits that they are probably interpolations.

[19] In the standard edition of the *Elements* (Leipzig 1883), I, 10, now reissued under the direction of E. S. Stamatis (Leipzig 1969).

[20] H. Hasse and H. Scholz, 'Die Grundlagenkrisis in der griechischen Mathematik', *Kant-Studien* **XXXIII** (1928), 17, call it a first attempt at an axiomatization in the modern sense.

[21] The notation '(X, Y)' for 'the ratio of X to Y' is taken from E. J. Dijksterhuis. See his *Archimedes* (transl. by C. Dikshoorn), Copenhagen 1956, p. 51. The symbols '$\cdot$', '$<$', '$>$', and '$=$' do not have their usual numerical sense, since '$\cdot$' designates an operation on magnitudes, and the other three symbols designate relations of size holding between either magnitudes or ratios of magnitudes to one another.

[22] See 'Continuity and Irrational Numbers' in R. Dedekind, *Essays on the Theory of Numbers* (transl. by W. Beman), Chicago 1924, pp. 15–17.

[23] F. Beckmann, in 'Neue Gesichtspunkte zum 5. Buch Euklids', *Archive for History of Exact Sciences* **IV** (1967/8), 106–107, lists twenty-four 'tacit assumptions' of Book V.

[24] Łukasiewicz, *Aristotle's Syllogistic*, p. 6, denies Platonic influence. But see W. and M. Kneale, *The Development of Logic*, Oxford 1962, pp. 44, 67–68.

[25] Translation by T. Heath, *Mathematics in Aristotle*, Oxford 1949, p. 26. This is an excellent work to consult for details of the mathematical aspects of the passages I am discussing.

[26] W. D. Ross, *Aristotle's Prior and Posterior Analytics*, Oxford 1949, pp. 412–414.

[27] See, for example, *Prior Analytics*, II.21.67a12–16, or I.25.48a33–37.

[28] *Aristotle's Syllogistic*, p. 15.

[29] See, for example, Alexander, *In Analyticorum Priorum*, 373.29–31, or Galen, *Institutio*, IV.6.

[30] *In Analyticorum Priorum*, 372.29–30.

[31] For the details, see Simplicius, *In Aristotelis Physicorum Libros Quattuor Priores Commentaria* (ed. by H. Diels), Berlin 1882, 60.22–68.32.

[32] See the passage cited in n. 31.

[33] The evidence is collected in F. Wehrli, *Die Schule des Aristoteles*, Basel/Stuttgart, 2nd ed., 1969, VIII, 11–20.

[34] *In Analyticorum Priorum*, 260.9–261.28.

[35] Aristotle discusses argument from a hypothesis briefly in I.23.41a22–41b5 and in somewhat more detail in I.44.
[36] *Aristotle's Syllogistic*, pp. 49, 74.
[37] Quoted by Simplicius, *In Aristotelis Categorias Commentarium* (ed. by C. Kalbfleisch), Berlin 1907, 394.13–395.31.
[38] *Institutio*, III.3, 4.
[39] *Institutio*, V.5 (said of those about Chrysippus). Diogenes Laertius, *Vitae Philosophorum* (ed. by H. S. Long), Oxford 1964, VII.190, lists among Chrysippus's works 'On a true *diezeugmenon*' and 'On a true *sunēmmenon*'.
[40] See *In Analyticorum Priorum*, 262.28–32.
[41] See, for example, *In Analyticorum Priorum*, 21.30–31; 22.18.
[42] Following the words, not their meanings (*In Analyticorum Priorum*, 373.29–30); espousing the hypothetical syllogism (*ibid.*, 262.28–29); using the words *adiaphora* and *proēgmena* (*In Aristotelis Topicorum Libros Octo Commentarium* (ed. by M. Wallies), Berlin 1891, 211.9–10).
[43] Attributed to the *neōteroi* by Alexander, *In Analyticorum Priorum*, 17.11–12.
[44] Sextus Empiricus, *Adversus Mathematicos*, VIII.443, in *Opera* II (ed. by H. Mutschmann and J. Mau), Leipzig 1914. Alexander associates the one-premissed arguments with 'those about Antipater' (*In Topicorum*, 8.16–19). Other relevant passages are collected in C. Prantl, *Geschichte der Logik im Abendlande*, Leipzig 1855, I, 477–478.
[45] Alexander, *In Analyticorum Priorum*, 19.4–6, 262.9, 263.31–32, 324.17–18.
[46] Diogenes Laertius, *Vitae Philosophorum*, VII.76.
[47] *In Topicorum*, 539.18.
[48] *Vitae Philosophorum*, VII.191.
[49] *Vitae Philosophorum*, VII.79–81; Sextus Empiricus, *Adversus Mathematicos*, VIII.223–226; Galen, *Institutio*, VI.6.
[50] *In Analyticorum Priorum*, 164.30–31, 278.6–14. At 284.13–17 Alexander ascribes to *hoi apo tēs Stoas* the second, third, and fourth themata. On the themata, see O. Becker, *Über die vier Themata des stoischen Logik*, in *Zwei Untersuchungen zur antiken Logik*, *Klassisch-philologische Studien* XVII (1957), 27–49.
[51] Explicit attribution of these three arguments to the *neōteroi* is found at *In Anaylticorum Priorum*, 164.28–30. The examples of the first two are taken from Alexander's commentary on the *Topics*, 10.8–12. Nothing is known about the infinite matter argument. For a guess as to its character, see O. Becker, *Über die vier Themata*, 38.
[52] *In Analyticorum Priorum*, 84.12–15.
[53] Ascribed by Alexander to the *neōteroi* (*In Analyticorum Priorum*, 22.18; 345.13), but elsewhere simply to the Stoics. However, these arguments are discussed in close conjunction with the one-premissed arguments at 21.10–23.2, the source of the example in the text. This example and others are discussed below, p. 27ff.
[54] *In Analytica Priora*, 245.24–246.32.
[55] In *Primum Elementorum*, 256.1–8.
[56] *In Analyticorum Priorum*, 22.3–7.
[57] *In Analyticorum Priorum*, 22.17–19.
[58] *Institutio*, XVII.7. Alexander takes the same approach (*In Analyticorum Priorum*, 344.9–345.12).
[59] *Institutio*, XIX.6.
[60] *Institutio*, XVIII.8.
[61] *Institutio*, XVI.1.
[62] *In Analyticorum Priorum*, 344.23–27.

[63] *Institutio*, XVI.5.
[64] *Institutio*, XVI.10–11. The argument in question is of the form '*a* is the son (father) of *b*; therefore *b* is the father (son) of *a*'. The conditional premiss to be added is, of course, 'If *a* is the son (father) of *b*, then *b* is the father (son) of *a*'. The categorical premiss is unfortunately lacking in the manuscript.
[65] Alexander's and Galen's discussions would seem to presuppose this. See especially Alexander, *In Analyticorum Priorum*, 68.21–69.1; 345.13–346.6.
[66] In his article 'Posidonius d'Apamée, théoricien de la géometrie', *Revue des études grecques* **XXVII** (1914), 44–45 (reprinted in *Études de philosophie antique*), E. Bréhier argues that Posidonius was the first (and also the last) Stoic with a "theory of the logic of geometry".
[67] See, for example, Diogenes Laertius, *Vitae Philosophorum*, VII.65. Other references are given in Mates, *Stoic Logic*, pp. 132–133.
[68] See the passages in H. Bonitz, 'Index Aristotelicus', in *Aristotelis Opera* (ed. by I. Bekker), Berlin 1831–70, V, 70b4–13.
[69] According to Cicero's *Academica*, I.xii.46 (ed. by O. Plasberg), Leipzig 1922, Zeno attended lectures by the skeptic Carneades and admired him very much.
[70] G. Vlastos, 'Zeno of Sidon as a Critic of Euclid', in *The Classical Tradition* (ed. by L. Wallach), Ithaca, N.Y., 1966, pp. 154–155.
[71] Proclus, *In Primum Elementorum*, 199.11–200.1.
[72] *In Primum Elementorum*, 200.1–3.
[73] *In Primum Elementorum*, 214.15–218.11. I shall discuss the details of this passage in another paper.
[74] *In Primum Elementorum*, 277.25–279.11.
[75] W. Crönert, *Kolotes und Menedemos*, *Studien zur Palaeographie und Papyruskunde* **VI** (1906), 109.
[76] This is a very common ancient criticism of the first *anapodeiktos*. See, for example, Sextus Empiricus, *Adversus Mathematicos*, VIII.440–442.
[77] See Proclus, *In Primum Elementorum*, 193.10–198.15.
[78] *In Primum Elementorum*, 196.15–18.
[79] O. Neugebauer, *Über eine Methode zur Distanzbestimmung Alexandria–Rom bei Heron*, *Det Kgl. Danske Videnskabernes Selskab* **XXVI** (1938), 21–24.
[80] See. K Tittel, *De Gemini Stoici Studiis Mathematicis Quaestiones Philologae*, Leipzig 1895. Bréhier ('Posidonius d'Apamée', pp. 46–49) thinks that Geminus's work on mathematics is derived entirely from Posidonius.
[81] See E. Zeller, *Die Philosophie der Griechen in ihrer geschichtlichen Entwicklung*, Leipzig, 5th ed., 1923, pt. 3, sec. 2, pp. 642–645. The same material is found in E. Zeller, *A History of Eclecticism in Greek Philosophy* (transl. by S. F. Alleyne), London 1883, pp. 113–117. The importance of the reawakening of interest in Aristotle's work for the history of logic is stressed by J. Mau, 'Stoische Logik', *Hermes* LXXXV (1957), 147–158. The historical reconstruction of the present paper seems to provide support for Mau's views.

JOHN MULHERN

MODERN NOTATIONS AND ANCIENT LOGIC

To what extent does ancient logic admit of accurate interpretation in modern terms? Blanché [3] and Dürr [14] published general surveys of research on ancient logic in the mid-1950's. My aim in the present paper is to identify studies made available during the quarter-century 1945–1970 that illustrate the influence modern notations have had on our understanding of ancient logical texts. Accepting Bocheński's division of ancient logic into four temporally distinct stages, I mention research on the Prearistotelian, Aristotelian, Stoic and Commentatorial logics in Sections 1–4. In Section 5, I offer some generalizations on the utility of modern notations in writing the history of ancient logic.

1. PREARISTOTELIAN LOGIC

Of the four stages of Greek logic, the Prearistotelian, which goes back perhaps as far as Parmenides (sixth century, B.C.) or beyond, has received least attention during the quarter-century of this study. The sources for Prearistotelian logic – the Presocratic fragments and the dialogues of Plato – contain many arguments that exemplify argument schemata but none of the schemata themselves. Bocheński wrote in 1951 ([4], p. 15), "we know of no *correct* logical principle *stated* and *examined* for its own sake before Aristotle"; and he gave no example of even an incorrect logical principle stated and examined at this stage. Where there are no principles stated in the natural language of the text, there are none to be transcribed directly into a modern notation. Accordingly, historians of logic have had to settle for discovering and recording the logical principles exemplified by philosophical arguments stated in the natural language materials of this first stage.

While little work of this sort has been done with the Presocratic fragments, some inroads have been made on the dialogues of Plato (427–347). In 1945, Dürr [13], 'Moderne Darstellung der platonischen Logik', made extensive use of modern notations to clarify the argument of parts of

J. Corcoran (ed.), Ancient Logic and Its Modern Interpretations, 71–82.

Plato's *Sophist*. A year later, Beth [2] summarized the results of his partly transcriptional analyses of the Theory of Ideas and the Theory of Ideal Numbers, along with similar analyses of Presocratic logic, Aristotelian logic, and Stoic logic. Then, in 1951, Bocheński [4] cast doubts on the enterprise of Dürr, Beth, and the like by stating flatly of Plato: "Correct logic we find none in his work".

Bocheński had transcribed what he considered a flagrant example of the intolerable goings-on in the dialogues – the false principle $\ulcorner SaP \supset \bar{S}a\bar{P} \urcorner$ exemplified in *Gorgias* 507A – as evidence for his view. Sprague [31] pointed out in 1962, however, that the text in question needn't be transcribed as Bocheński had transcribed it, that otherwise transcribed it exemplified a true principle, that similar but true principles were exemplified elsewhere in the Platonic writings, and that the principle's being false, if false it was, might be accounted for by the literary form of the dialogue as well as by the logical ineptitude of its author: an author of dramatic literature need not be held responsible for the logical deficiencies of his mixed bag of characters. To Sprague belongs the credit for distinguishing intellectual biography from history of logic and for showing that the clear and unmistakable exemplification of a false logical principle in a dialogue, if it is supposed deliberate, may well interest the historian of logic just as much as would the exemplification of a true principle.

Since 1954, many scholarly papers have been written that call attention to a false metalogical principle – a violation of type rules – in Plato's *Parmenides*. A bibliography of the literature is given in Vlastos [35]. One scholar writes of this as "a still-rising flood of literature, intended to clarify Plato's text but tending to whelm it with the symbols of modern logic" ([8], p. 369). Actually, the use of modern notations in these papers is comparatively modest, although it is fair to say that the controversy over the *Parmenides* has given currency and respectability to transcriptions of the logical material that one finds in this and other dialogues. In 1955, this violation of type rules was considered systematically in Wedberg [37], *Plato's Philosophy of Mathematics*, which used some importations from formal language to reconstruct the Theory of Ideas and the Platonic philosophies of geometry and arithmetic.

Readers of this literature are not agreed that the Platonic writings contain any interesting logical doctrines. Some, such as Vlastos, find at crucial points only a "record of honest perplexity" where arguments in the dialogues seem to go astray ([36], p. 254); others suggest "it is a work of Plato's genius that some of the problems he confronted are closely related to current problems in logical theory" (Van Fraassen [34], p. 498). My own judgment is that both true and untrue logical principles are exemplified in the dialogues and that the treatment of syntactical and semantical problems in the dialogues is almost always instructive. The literary form of the dialogues allows the historian of logic neither to affirm nor to deny that the author of the dialogues subscribed to this or that logical doctrine; but it does not prevent his affirming that Plato was acquainted with a variety of metalogical doctrines and that he knew how to perform numerous interesting logical operations (cf. [25]).

2. Aristotelian Logic

The basic assumptions that governed early postwar research on Aristotelian logic are traceable to prewar works by Łukasiewicz (notably [19] and [20]). Łukasiewicz supposed that there were two quite distinct ancient systems of logic, the Aristotelian and the Stoic, and that these systems differed from one another in that only term variables occurred in Aristotelian syllogisms while only propositional variables occurred in Stoic syllogisms. In drawing this distinction, Łukasiewicz was attributing to Aristotle (384–322) exactly those analytical syllogisms that belong to the traditional four figures, counting in the non-Aristotelian fourth figure syllogisms while excluding all the other logical material that Aristotle's definition of syllogism (*An. Pr.* 24b18–22; *Top.* 100a25–27) provides for. Łukasiewicz adapted the *A*, *E*, *I*, and *O* of the mediaeval syllogistic mnemonics for use as a functorial notation with term variables to represent antecedent and consequent sentences. This notation he supplemented with the truth-functional prefixes '*K*' and '*C*', the latter taking the place of the Greek expression εἰ, English 'if', which commonly occurs at the beginning of Aristotle's syllogisms. Rendering εἰ by '*C*' was tantamount to embracing the view that Aristotle's analytical syllogisms were implicational rather than inferential. It was a short step from this to distributing the moods of syllogistic – construed as logical theses – among axioms and theorems, and then using the former to derive the latter. Łukasiewicz took this step directly.

By using this functorial notation for syllogistic antecedent and consequent sentences instead of a quantificational notation with truth-functors, and by leaving this notation unanalyzed, Łukasiewicz was able to keep the problems of existential import from arising in his transcription of syllogistic, even if at the cost of failing to provide any analysis of syllogistic sentence structure. Indeed, Łukasiewicz expressly rejected the use of quantifiers in representing syllogistic antecedent and consequent sentences, although he did settle on the universal quantifier as an appropriate sign for indicating the necessity of syllogistic moods.

In 1951, Łukasiewicz restated his prewar view of syllogistic in monographic form under the title *Aristotle's Syllogistic from the Standpoint of Modern Formal Logic* [18], adding to it certain historical observations and a chapter on the problem of decision for assertoric analytical syllogistic.

The year of Łukasiewicz's monograph was the year also of Bocheński [4], *Ancient Formal Logic*, which has been cited already. Bocheński surveyed not only the assertoric but also the modal analytical syllogistic, differing from Łukasiewicz in admitting quantifiers to the transcription of analytical syllogisms. Bocheński avoided the problems of existential import by letting the laws of subalternation hold and construing the term variables accordingly.

In his treatment of Aristotle, Bocheński was building on his *La logique de Théophraste* [6] – a study completed before the war but not generally circulated until 1947. Here Bocheński had shown that the schema of Theophrastus' sentences κατὰ πρόσληψιν was expressible with quantifiers as

$$C\Pi x\phi x\Pi x\psi x \text{ ([6], p. 48).}$$

Subsequently he found that Aristotle's syllogisms also were susceptible of a quantificational transcription showing function and argument, and he cited *An. Pr.* 49b14 ff in this connection (he might have cited 32b25 ff as well). Once the analytical syllogisms had been provided with a quantificational transcription, it became plausible to regard Łukasiewicz's functorial notation as an abbreviation for a set of quantificational formulae; Prior suggested four years later that the functorial notation be so regarded ([27], p. 121).

The most distinctive feature of Bocheński's writing on Aristotelian logic, however, was the concern it displayed for non-analytical formulae. As he wrote himself,

> Modern commentators of Aristotle were fascinated by the Aristotelian [sc. analytical] syllogistics to an extent that they often overlooked the wealth of non-analytical formulae which the Organon contains ([4], p. 63; see also [7]).

These included formulae belonging to the logic of classes, predicates, identity, and relations, as well as to propositional calculus. Bocheński ended his exposition of this material by remarking: "Further research would probably discover more non-analytical laws in the Organon, especially in the Topics" ([4], p. 71). Bocheński used the familiar notations of the several parts of non-analytical logic, mainly those of *Principia Mathematica*, for transcribing this material.

Already in 1951, then, Bocheński had gone beyond the prewar view according to which only term variables occurred in Aristotelian syllogisms and these syllogisms themselves were object language implications rather than inference schemata. In his *Formale Logik* [5], which appeared in 1956, to be followed by an English translation [5e] in 1961, Bocheński presented an ordinary language transcription of the principal texts used for the history of ancient logic, supplemented by a bare minimum of special notation.

Łukasiewicz added three chapters on Aristotle's modal logic to a second edition of *Aristotle's Syllogistic* [18_2] in 1957. In the course of this exercise, Łukasiewicz broke with the concepts and notations of the older modern modal logic that Becker [1] had relied on in his Aristotelian studies and other scholars had used afterward. Łukasiewicz believed that a satisfactory modal logic would have to be four-valued, and that only a satisfactory modal logic would suffice for understanding Aristotle. From the standpoint of his new four-valued modal logic (the C–n–δ–p system), Łukasiewicz claimed to be able to "explain the difficulties and correct the errors of the Aristotelian modal syllogistic" ([18_2], p. v). His procedure was to set up the system, transcribe Aristotle's modal syllogisms into its notation, and then see how the transcript compared to the system.

Łukasiewicz's work was criticized and built upon in 1959 by Patzig [26] – five essays presented under the collective title *Die Aristotelische Syllogistik* – which appeared in an English translation [26e] in 1968. Patzig picked up many points of detail in earlier historians that wanted correction. His thematic, however, amounted to assigning to analytical syllogistic the status of a special part of the logic of binary relations. He did not drop the *A–E–I–O* functorial notation in favor of the usual notation for relations, but rather construed the functorial notation as having to do with binary relations; and he supplemented this with notations drawn from the logic of predicates and classes.

If we leave out of account W. and M. Kneale [17], *The Development of Logic* (1962), which avoids the use of modern notations for Aristotelian logic, the next important item to appear was McCall [24], *Aristotle's Modal Syllogisms* (1963). Unlike Łukasiewicz, McCall adopted an intuitive approach to Aristotle's modal logic, working from the intuition to the formalism rather than the other way round. Rejecting Łukasiewicz's four-valued apparatus as well as the quantified modal logic of Becker [1] and the non-formal approach of Rescher [28], "Aristotle's Theory of Modal Syllogisms and Its Interpretation" (which was not published until 1964), McCall presented a complicated axiom system of unquantified modal logic that was claimed to coincide exactly with Aristotle's intuitions about 'apodeictic' analytical syllogisms and, to a lesser extent, with his intuitions about 'contingent' syllogisms.

Two years later, in 1965, the revival of interest in Aristotle's non-analytical logic was rewarded by the appearance of de Pater [11], *Les Topiques d'Aristote et la dialectique platonicienne*. This work consolidated a great deal of the research done on the *Topica* during the last hundred years and more. De Pater transcribed Aristotle's non-analytical formulae into an amalgam of ordinary language and logical notation, using sentence schemata with name and predicate variables. In order to reflect Aristotle's distinction of predicables from one another according to their logical features and powers, de Pater provided that, in his transcriptions, ϕ should be replaced by the names of properties only, ψ by the names of accidents, τ by the names of differentiae, and D by the names of definitions.

The last important monograph on Aristotle's logic in this quarter-century was Rose [30], *Aristotle's Syllogistic*. Rose followed an aside of Prior ([27_2], p. 116) in suggesting that Aristotle had formulated his assertoric analytical syllogisms as inference schemata in the metalanguage rather than as laws in the object language. Accordingly, he denied that these syllogisms ought to be construed as implications. In denying this, Rose was not recommending a return to the four schemata of traditional syllogistic as instruments for interpreting Aristotle; he was proposing instead a return to Aristotle's own abbreviated capital letter variable notation, in which the capital letter variables have predicables as their substitution instances. Rose's Aristotelian notation had the advantage that it allowed for only three figures of analytical syllogisms rather than four and that it thus countered the view held by Bocheński, Łukasiewicz, and Ross that either Aristotle was wrong in finding only three figures or else he was wrong when he said he was dividing the figures according to the position of the middle term.

In 1970 (cf. Corcoran [10]) 'A Mathematical Model of Aristotle's Syllogistic', argued against viewing the assertoric analytical syllogistic as an axiom system, in this respect introducing a major revision of the Łukasiewicz interpretation. According to Corcoran, Aristotle's syllogistic was concerned not merely with the validity of syllogistic arguments or the truth of syllogistic laws but also, even mainly, with the structure of syllogistic proofs, after the manner of a modern natural deduction system. Corcoran represented Aristotelian deductions first in ordinary language, sentence by sentence, and then in an abbreviatory notation using the four traditional functors (renamed *A*–*N*–*S*–$) with term variables. These deductions were identical with the traditional reductions of imperfect to perfect syllogisms. His result was a representation that showed the details of Aristotelian deductions in an obvious fashion. Corcoran did not conditionalize these deductions but left his premisses marked as assumptions, thus avoiding the implicational interpretation of syllogistic.

3. Stoic Logic

The major achievements of modern research on Stoic logic are accessible in Mates [22] and W. and M. Kneale [17]. Among these have been the identification of inference schemata that belong to the modern propositional calculus. The Stoics distinguished five indemonstrable (ἀναπόδεικτοι) propositional inference schemata. These have been discussed by Mates and the Kneales, as have the theorems derived from them. Both Mates and the Kneales use modern notations to clarify the derivations of theorems from the indemonstrables.

According to these historians, the sort of implication one finds in the indemonstrables and in the theorems is material implication. This matter appears to be settled. The Stoics, however, recognized other varieties of implication as well; and these other varieties of implication have been a problem for modern scholars.

The prewar assumptions of Łukasiewicz concerning two ancient logics were as influential with historians of Stoic logic as with their Aristotelian counterparts. In 1934, Łukasiewicz [20] had assimilated Philonian implication to material implication, as apparently all scholars continue to do, and Diodorean implication to Lewis's strict implication. Other scholars, notably Hurst Kneale [16] and Chisholm [9], naturally followed this precedent, since there were no varieties of implication commonly known except material and strict for propositional calculus, and no notations for these implications commonly used except the three main ones (Peano-Russell, Hilbert, and Polish prefix) for material implication and the Lewis fishhook for strict implication. Further clarification of Diodorean implication awaited further development in specialized logics and their notations.

The first important breakthrough appeared in Mates [21], 'Diodorean Implication' (1949), later incorporated into [22], *Stoic Logic* (1953). Diodorus, according to Mates, had held the view that "a conditional holds ... if and only if it holds *at all times* in the Philonian [i.e. material] sense" (cf. [22], p. 45). Expressing this required the invention of a tense operator that worked like a quantifier, and Mates settled on the following definition, in which '$\rightarrow$' represents Diodorean implication:

$$(F \rightarrow G) \equiv (t)\,(F(t) \supset G(t)).$$

Starting from this point, Prior and others began to reconstrue temporal operators by analogy with modal operators, opening up interesting if controversial avenues of research both in the history of Stoic logic and in the contemporary logic of tense and modality. The development of research on Diodorean implication thus appears to exemplify a pattern of trial and error in transcription. This pattern is exemplified again in research on the logic of the commentators.

4. Commentatorial Logic

Probably the most striking example of this pattern to occur in recent research on Commentatorial logic, which, like research on Prearistotelian logic, remains underdeveloped, is that concerning certain formulae in the *De syllogismo hypothetico* of Boethius (480–524). Here the first important work was Dürr [12], 'Aussagenlogik im Mittelalter' (1938). Dürr, under the influence of Łukasiewicz, began by transcribing the Latin expressions 'si', 'cum', and 'aut' into the prefix notation for propositional calculus. It turned out subsequently, however, that, in Dürr's transcription, several Boethian formulae were false. This situation was remedied when van den Driessche [33], 'Le *De syllogismo hypothetico* de Boèce' (1949), showed that a uniform transcription of these Latin expressions each by a single truth-functor was mistaken, and that Boethius had intended by 'si' sometimes 'C' (for implication) and sometimes 'E' (for equivalence), by 'cum' sometimes 'C' and sometimes 'K' (for conjunction), and by 'aut' sometimes, but not always, 'A' (for non-exclusive alternation) (cf. Mates [23]). Dürr subsequently published a monograph on Boethius [15] which he had written before the war.

Two other figures of this last stage of ancient logic – Apuleius (125–171) and Galen (129–199) – have been the subject of recent monographs. According to the thorough researches of Sullivan [32], Apuleius now appears to have exercised a much greater influence on early mediaeval logic than was recognized formerly, either in the original or through the excerpts of Martianus Capella, Cassiodorus, and Isidore of Seville. Sullivan presents Apuleius' syllogistic as a system of conditionalized laws of inference; sentence schemata that occur in the conditionals are stated in the traditional notation of term variables with mnemonic letters. The rules on which Apuleius is supposed to have based his syllogistic reductions are transcribed as rules of propositional calculus. Whereas Sullivan has made extensive use of modern notations, however, Rescher [29] has not had occasion to make use of them in his discussion of Galen.

5. Generalizations

At the beginning of this paper, I asked to what extent ancient logic admits of accurate interpretation in modern terms. While no final answer to this question will be available until research in the field has gone a good deal further than it has so far, still the progress since 1945 has been remarkable, and it is not too early to consider its causes.

In his history of the history of logic, Bocheński wrote as follows:

The rise of modern history of logic concerning all periods save the mathematical was made possible by the work of historians of philosophy and philologists in the 19th century. These published for the first time a series of correct texts edited with reference to their context in the history of literature. But the majority of ancient philologists, medievalists and Sanskrit scholars had only slight understanding of and little interest in formal logic. History of logic could not be established on the sole basis of their great and laborious work.

For its appearance we have to thank the fact that formal logic took on a new lease of life and was reborn as mathematical. *Nearly all* the more recent researches in this history were carried out by mathematical logicians or by historians trained in mathematical logic. ([5e], pp. 9–10.)

The trained researchers who have worked on the ancient materials have had to do much more than merely transcribe into modern notations logical treatises originally written in ancient natural languages. Just finding suitable transcriptions has had to wait on considerable analysis of the ancient texts. Transcription into modern notations presupposes some community of understanding and purpose with the ancient logicians, and this community is something that needs to be argued for. In general, a department of ancient logic lends itself to being dealt with in notation if and only if its corresponding department of modern logic lends itself to being dealt with in notation. Logistic systems and their interpretations lend themselves to this to a great extent, theoretical syntax and especially semantics to a much lesser extent. Where a modern notation follows or reproduces or elucidates the logical form of a sentence or inference or schema that interests an ancient logician, then its use is in order. The studies discussed in Sections 1–4 of this paper point to the conclusion that the judicious use of modern notations has been one cause of progress – over the last two decades and a half – in our understanding of ancient logic.

Bryn Mawr College

BIBLIOGRAPHY

[1] Becker, A., *Die Aristotelische Theorie der Möglichkeitschlüsse*, Berlin 1933.
[2] Beth, E. W., 'Historical Studies in Traditional Philosophy', *Synthese* **5** (1946–1947), 258–270.
[3] Blanché, R., 'Vues nouvelles sur l'ancienne logique', *Les Études Philosophiques* **11** (1956), 183–208.
[4] Bocheński, I. M., *Ancient Formal Logic*, Amsterdam 1951.
[5] Bocheński, I. M., *Formale Logik*, Freiburg 1956.
[5e] Bocheński, I. M., *A History of Formal Logic* (transl. by I. Thomas), Notre Dame 1961.

[6] Bocheński, I. M., *La Logique de Théophraste*, Freiburg 1947.
[7] Bocheński, I. M., 'Non-Analytical Laws and Rules in Aristotle', *Methodos* 3 (1951), 70–80.
[8] Cherniss, H. F., 'The Relation of the *Timaeus* to Plato's Later Dialogues', in *Studies in Plato's Metaphysics* (ed. by R. E. Allen), New York 1965, pp. 339–378.
[9] Chisholm, R., 'Sextus Empiricus and Modern Empiricism', *Philosophy of Science* **8** (1941), 371–384.
[10] Corcoran, J., 'A Mathematical Model of Aristotle's Syllogistic', *Archiv für Geschichte der Philosophie* **55** (1973), 191–219.
[11] de Pater, W. A., *Les Topiques d'Aristote et la dialectique platonicienne*, Freiburg 1965.
[12] Dürr, K., 'Aussagenlogik im Mittelalter', *Erkenntnis* 7 (1938), 160–168.
[13] Dürr, K., 'Moderne Darstellung der platonischen Logik: Ein Beitrag zur Erklärung des Dialoges *Sophistes*', *Museum Helveticum* **2** (1945), 166–194.
[14] Dürr, K., 'Moderne historische Forschung im Gebiet der antiken Logik', *Studia Philosophica* **13** (1953), 72–98.
[15] Dürr, K., *The Propositional Logic of Boethius*, Amsterdam 1951.
[16] Hurst Kneale, M., 'Implication in the Fourth Century B.C.', *Mind* **44** (1935), 484–495.
[17] Kneale, W. and M., *The Development of Logic*, Oxford 1962.
[18] Łukasiewicz, J., *Aristotle's Syllogistic from the Standpoint of Modern Formal Logic*, Oxford 1951.
[18₂] Łukasiewicz, J., *Aristotle's Syllogistic from the Standpoint of Modern Formal Logic*, 2nd ed., Oxford 1957.
[19] Łukasiewicz, J., *Elementy logiki matematycznej*, Warsaw 1929.
[20] Łukasiewicz, J., 'Z historii logiki zdań', *Przeglad Filozoficzny* **37** (1934), 417–437.
[21] Mates, B., 'Diodorean Implication', *Philosophical Review* **58** (1949), 234–242.
[22] Mates, B., *Stoic Logic*, Berkeley and Los Angeles 1953.
[23] Mates, B., Review of van den Driessche, R., 'Le *De Syllogismo Hypothetico* de Boèce', *Journal of Symbolic Logic* **76** (1951), 150.
[24] McCall, S., *Aristotle's Modal Syllogisms*, Amsterdam 1963.
[25] Mulhern, J. J., *Problems of the Theory of Predication in Plato's 'Parmenides', 'Theaetetus', and 'Sophista'* (Ph.D. dissertation in Philosophy, State University of New York at Buffalo) 1970.
[26] Patzig, G., *Die Aristotelische Syllogistik*, Göttingen 1959.
[26₂] Patzig, G., *Aristotle's Theory of the Syllogism* (transl. by J. Barnes), Dordrecht 1968.
[27] Prior, A. N., *Formal Logic*, Oxford 1955.
[27₂] Prior, A. N., *Formal Logic*, 2nd ed., Oxford 1962.
[28] Rescher, N., 'Aristotle's Theory of Modal Syllogisms and Its Interpretation', in *The Critical Approach to Science and Philosophy* (ed. by M. Bunge), Glencoe 1964, pp. 152–177.
[29] Rescher, N., *Galen and the Syllogism*, Pittsburgh 1966.
[30] Rose, L. E., *Aristotle's Syllogistic*, Springfield 1968.
[31] Sprague, R. K., *Plato's Use of Fallacy*, New York 1962.
[32] Sullivan, M. W., *Apuleian Logic*, Amsterdam 1967.
[33] van den Driessche, R., 'Le *De syllogismo hypothetico* de Boèce', *Methodos* **1** (1949), 293–307.

[34] Van Fraassen, B. C., 'Logical Structure in Plato's *Sophist*', *Review of Metaphysics* **22** (1969), 482–498.
[35] Vlastos, G., "Plato's 'Third Man' Argument (*Parm.* 132A1-B2): Text and Logic", *Philosophical Quarterly* **19** (1969), 289–301.
[36] Vlastos, G., 'The Third Man Argument in the Parmenides', (1954), in *Studies in Plato's Metaphysics* (ed. by R. E. Allen), New York 1965, pp. 231–263.
[37] Wedberg, A., *Plato's Philosophy of Mathematics*, Stockholm 1955.

PART THREE

ARISTOTLE'S LOGIC

JOHN CORCORAN

ARISTOTLE'S NATURAL DEDUCTION SYSTEM

> Here and elsewhere we shall not obtain the best insight into things until we actually see them growing from the beginning.
> *Aristotle*

In the present article we attempt to show that Aristotle's syllogistic is an *underlying logic* which includes a natural deductive system and that it is not an axiomatic theory as had previously been thought. We construct a mathematical model which reflects certain structural aspects of Aristotle's logic and we examine both the mathematical properties of the model and the relation of the model to the system of logic envisaged in certain scattered parts of *Prior* and *Posterior Analytics*.

Our interpretation restores Aristotle's reputation as a logician of consummate imagination and skill. Several attributions of shortcomings and logical errors to Aristotle are shown to be without merit. Aristotle's logic is found to be self-sufficient in several senses. In the first place, his theory of deduction is logically sound in every detail. (His indirect deductions have been criticized, but incorrectly on our account.) In the second place, Aristotle's logic presupposes no other logical concepts, not even those of propositional logic. In the third place, the Aristotelian system is seen to be complete in the sense that every valid argument expressible in his system admits of a deduction within his deductive system; i.e., every semantically valid argument is deducible.

There are six sections in this article. The first section includes methodological remarks, a preliminary survey of the present interpretation and a discussion of the differences between our interpretation and that of Łukasiewicz. The next three sections develop the three parts of the mathematical model. The fifth section deals with general properties of the model and its relation to the Aristotelian system. The final section contains conclusions.

J. Corcoran (ed.), Ancient Logic and Its Modern Interpretations, 85–131.

1. Preliminaries

1.1. *Mathematical Logics*

Logicians are beginning to view mathematical logic as a branch of applied mathematics which constructs and studies mathematical models in order to gain understanding of logical phenomena. From this standpoint mathematical logics are comparable to the mathematical models of solar systems, vibrating strings, or atoms in mathematical physics and to the mathematical models of computers in automata theory[1] (cf. Kreisel, p. 204). Thus one thinks of mathematical logics as mathematical models of real or idealized logical systems.

In the most common case a mathematical logic can be thought of as a mathematical model composed of three interrelated parts: a 'language', a 'deductive system' and a 'semantics'. The language is a syntactical system often designed to reflect what has been called the logical form of propositions (cf. Church, pp. 2, 3). The elements of the language are called sentences. The deductive system, another syntactical system, contains elements sometimes called formal proofs or formal deductions. These elements usually involve sequences of sentences constructed in accord with syntactical rules themselves designed to reflect actual or idealized principles of reasoning (cf. Church, pp. 49–54). Finally, the semantics is usually a set-theoretic structure intended to model certain aspects of meaning (cf. Church, pp. 54ff), e.g., how denotations attach to noun phrases and how truth-values attach to sentences.[2]

Many theories of logic involve a theory of propositional forms, a theory of deductive reasoning and a theory of meaning (cf. Church, pp. 1, 3, 23). Such theories are intended to account for logical phenomena relating to a natural language or to an ideal language perhaps alleged to underlie natural language, or even to an artificial language proposed as a substitute for natural language. In any case, it is often possible to construct a mathematical model which reflects many of the structural aspects of 'the system' envisaged in the theory. Once a mathematical logic has been constructed, it is possible to ask definite, well-defined questions concerning how well, or to what degree and in what respects, the model reflects the structure of 'the system' envisaged by the theory. Such activity usually contributes toward the clarification of the theory in question. Indeed any attempt to construct such a model necessarily involves an organized and detailed study of the theory and often raises questions not considered by the author of the theory.

1.1.1. *Underlying logics.* Because some articulations of the above viewpoint admit of certain misunderstandings, a few further comments may be in order. Consider a deductive science such as geometry. We may imagine that geometry presupposes its own subject matter which gives rise to its own laws, some of which are taken without deductive justification. In addition, geometry presupposes a geometrical language. The activity of deductively justifying some laws on the basis of others further presupposes a system of demonstrative discourses (the deductions). The activity of establishing by means of reinterpretations of the language of geometry that certain geometrical statements are independent of others further presupposes a system of reinterpretations of the language. The last three presupposed systems taken together from the *underlying logic* (cf. Church, p. 58, 317; Tarski, p. 297) of geometry.

Although the underlying logic is not a science it can be the subject matter of a scientific investigation. Of course, there is much more to be said about this approach to the study of deductive sciences, but what has been said should be sufficient to enable the reader to see that there is a clear distinction to be made between *logic* as a scientific study of underlying logics on one hand, and the *underlying logic* of a science on the other. It is roughly the difference between zoology and fishes. A science has an underlying logic which is treated scientifically by the subject called logic. Logic, then, is a science (in our sense, not Aristotle's), but an underlying logic of a science (Aristotle's sense) is not a science; rather it is a complex, abstract system presupposed by a science. Some of the possibility for confusion could be eliminated by using the term 'science' in Aristotle's sense and the term 'metascience' to indicate activities such as logic. Then we could say that a science presupposes an underlying logic which is then studied in a metascience, viz. logic.

It is unfortunate that in a previous article (Corcoran, 'Theories') I spoke of the 'science of logic' for what I should have termed 'the metascience, logic' or 'the science of logics'. That unfortunate usage, among other things, brought about Mary Mulhern's justified criticism (cf. her paper below) to the effect that I am myself guilty of blurring a distinction which I take to be crucial to understanding Aristotle's logic (metascience).

Readers of Mulhern's article should be advised that the present paragraphs were added as a result of Mulhern's remarks, which are still important and interesting but, hopefully, no longer applicable to me.

1.2. *The Data*

In the present paper we consider only Aristotle's theory of non-modal logic, which has been called 'the theory of the assertoric syllogism' and 'Aristotle's syllogistic'. Aristotle presents the theory almost completely in Chapters 1, 2, 4, 5 and 6 of the first book of *Prior Analytics*, although it presupposes certain developments in previous works – especially the following two: first, a theory of form and meaning of propositions having an essential component in *Categories* (Chapter 5, esp. 2a34–2b7); second, a doctrine of opposition (contradiction) more fully explained in *Interpretations* (Chapter 7, and cf. Ross, p. 3). Bocheński has called this theory 'Aristotle's second logic' because it was evidently developed after the relatively immature logic of *Topics* and *Sophistical Refutations*, but before the theory of modal logic appearing mainly in Chapters 3 and 8–22 of *Prior Analytics* I. On the basis of our own investigations we have come to accept the essential correctness of Bocheński's chronology and classification of the *Organon* (Bocheński, p. 43; Łukasiewicz, p. 133; Tredennick, p. 185).

Although the theory is rather succinctly stated and developed (in five short chapters), the system of logic envisaged by it is discussed at some length and detail throughout the first book of *Prior Analytics* (esp. Chapters 7, 23–30, 42 and 45) and it is presupposed (or applied) in the first book of *Posterior Analytics*. Book II of *Prior Analytics* is not relevant to this study.

1.3. *Theories of Deduction Distinguished From Axiomatic Sciences*

We agree with Ross (p. 6), Scholz (p. 3) and many others that the theory of the categorical syllogisms is a logical theory concerned in part with deductive reasoning (as this term is normally understood). Because a recent challenge to this view has gained wide popularity (Łukasiewicz, Preface to 2nd ed.) a short discussion of the differences between a theory of deduction (whether natural or axiomatic) and an axiomatic science is necessary.

A theory of deduction puts forth a number of principles (logical axioms and rules of inferences) which describe deductions of conclusions from premises. All principles of a theory of deduction are necessarily metalinguistic – they concern constructions involving object language sentences and, as was said above, a theory of deduction is one part of a theory of logic (which deals with grammar and meaning as well). Theories of deduction (and, of course, deductive systems) have been classified as 'natural' or 'axiomatic' by means of a loose criterion based on the prominence of logical axioms as opposed to rules – the more rules the more natural, the more axioms the more axiomatic. On one extreme we find the so-called Jaskowski-type systems which have no logical axioms and which are therefore most properly called 'natural'. On the other extreme there are the so-called Hilbert-type systems which employ infinitely many axioms though only one rule and which are most properly called 'axiomatic'. The reason for the choice of the term 'natural' may be attributed to the fact that our normal reasoning seems better represented by a system in which rules predominate, whereas axiomatic systems of deduction seem contrived in comparison (cf. Corcoran, 'Theories', pp. 162–171).

A science, on the other hand, deals not with reasoning (actual or idealized) but with a certain universe or domain of objects insofar as certain properties and relations are involved. For example, arithmetic deals with the universe of numbers in regard to certain properties (odd, even, prime, perfect, etc.) and relations (less than, greater than, divides, etc.). Aristotle was clear about this (*Posterior Analytics* I, 10, 28) and modern efforts have not obscured his insights (Church, pp. 57, 317–341). The laws of a science are all stated in the object language whose non-logical constants are interpreted as indicating the required properties and relations and whose variables are interpreted as referring to objects in the universe of discourse. From the axioms of a science other laws of the science are deduced by *logical reasoning*. Thus an axiomatic science, though not itself a logical system, presupposes a logical system for its deductions (cf. Church, pp. 57, 317). The logic which is presupposed by a given science is called the *underlying logic* of the science (cf. Church, p. 58 and Tarski, p. 297).

It has been traditional procedure in the presentation of an axiomatic science to leave the underlying logic implicit. For example, neither in Euclid's geometry nor in Hilbert's does one find any codification of the logical rules used in the deduction of the theorems from the axioms and definitions. It is also worth noting that even Peano's axiomatization of arithmetic and Zermelo's axiomatization of set theory were both presented originally without explicit description of the underlying logic (cf. Church, p. 57). The need to be explicit concerning the underlying logic developed late in modern logic.

1.4. *Preliminary Discussion of the Present Interpretation*

We hold that in the above-mentioned chapters of *Prior Analytics,* Aristotle developed a logical theory which included a theory of deduction for deducing categorical conclusions from categorical premises. We further hold that Aristotle treated the logic thus developed as the underlying logic of the axiomatic sciences discussed in the first book of *Posterior Analytics.* The relation of the relevant parts of *Prior Analytics* to the first book of *Posterior Analytics* is largely the same as the relation of Church's Chapter 4, where first order logic is developed, to the part of Chapter 5 where the axiomatic science of arithmetic is developed with the preceding as its underlying logic. This interpretation properly includes the traditional view (cf. Ross, p. 6 and Scholz, p. 3) which is supported by reference to the *Analytics* as a whole as well as to crucial passages in the *Prior Analytics* where Aristotle tells what he is doing (*Prior Analytics* I, 1; and cf. Ross, p. 2). In these passages Aristotle gives very general definitions – in fact, definitions which *seem* to have more generality than he ever uses (cf. Ross, p. 35).

In this article the term *syllogism* is not restricted to arguments having only two premises. Indeed, were this the case, either here or throughout the Aristotelian corpus, the whole discussion would amount to an elaborate triviality. Barnes (*q.v.*) has argued that *at least* two premises are required. Additional reasons are available. That Aristotle did *not* so restrict his usage *throughout* is suggested by the form of his definition of syllogism (24b19–21), by his statement that every demonstration is a syllogism (25b27–31; cf. 71b17, 72b28, 85b23), by the content of Chapter 23 of *Prior Analytics* I and by several other circumstances to be mentioned below. Unmistakable evidence that Aristotle applied the term in cases of more than two premises is found in *Prior Analytics* I, 23 (esp. 41a17) and in *Prior Analytics* II, 17, 18 and 19 (esp. 65b17, 66a18 and 66b2). However, it is equally clear that in many places Aristotle does restrict the term to the two-premise case. It may be possible to explain

Aristotle's emphasis on two-premise syllogisms by reference to his discovery (*Prior Analytics* I, 23) that *if* all two-premise syllogisms are deducible in his system *then* all syllogisms without restriction are so deducible. As mentioned above, in this article the term has the more general sense. Thus 'sorites' are syllogisms (but, of course, enthymemes are not).

The *Analytics* as a whole forms a treatise on scientific knowledge (24a, 25b28–31). On Aristotle's view every item of scientific knowledge is either known in itself by experience (or some other non-deductive method) or else deduced from items known in themselves (*Posterior Analytics, passim*, esp. II, 19). The *Posterior Analytics* deals with the acquisition and deductive organization of scientific knowledge. It is the earliest general treatise on the axiomatic method[3] in sciences. The *Prior Analytics*, on the other hand, develops the underlying logic used in the inference of deductively known scientific propositions from those known in themselves; but the logic of the *Prior Analytics* is not designed solely for such use (cf., e.g., 53b4–11; Kneale and Kneale, p. 24).

According to Aristotle's view, once the first principles have been discovered, all subsequent knowledge is gained by means of 'demonstrative syllogisms', syllogisms having antecedently known premises, and it is only demonstrative syllogisms which lead to 'new' knowledge (*Posterior Analytics* I, 2). Of course, the knowledge thus gained is in a sense not 'new' because it is already implicit in the premises (*Posterior Analytics* I,1).

According to more recent terminology (cf. Mates, *Elementary Logic*, p. 3) a *premise-conclusion argument* (*P-c* argument) is simply a set of sentences called the *premises* together with a single sentence called the *conclusion*. Of course the conclusion need not follow from the premises, if it does then the argument is said to be *valid*. If the conclusion does not follow, the argument is *invalid*. It is obvious that even a valid argument with known premises does not *prove anything* – one is not expected to come to know the conclusion by reading the argument because there is no reasoning expressed in a *P-c* argument. For example, take the premises to be the axioms and definitions in geometry and take the conclusion to be any complicated theorem which actually follows. Such a valid argument, far from demonstrating anything, is the very kind of thing which needs 'demonstrating'. *In 'demonstrating' the validity of an argument one adds more sentences until one has constructed a chain of reasoning pro-*

ceeding from the premises and ending with the conclusion. The result of such a construction is called a *deductive argument* (premises, conclusion, plus a chain of reasoning) or, more briefly, a *deduction.* If the reasoning in a deduction actually shows that the conclusion follows from the premises the deduction is said to be *sound*; otherwise *unsound.* Given this terminology we can say that by *perfect syllogism* Aristotle meant precisely what we mean by *sound deduction* and that Aristotle understood the term *syllogism* to include both valid *P-c* arguments and sound deductions[4] (cf. 24b19–32). For Aristotle an invalid premise-conclusion argument is *not* a syllogism at all (cf. Rose, pp. 27–28). In an imperfect syllogism the conclusion follows, but it is not evident that it does. An imperfect syllogism is 'potentially perfect' (27a2, 28a16, 41b33, and Patzig, p. 46) and it is made perfect by adding more propositions which express a chain of reasoning from the premises to the conclusion (24b22–25, 28a1–10, 29a15, *passim*). Thus a demonstrative syllogism for Aristotle is a sound deduction with antecedently known premises (71b9–24, 72a5, *passim*).

That 'a demonstrative syllogism', for Aristotle, is not simply a valid *P-c* argument with appropriately known premises is already obvious from his view that such syllogisms are productive of knowledge and conviction (73a21; Ross, pp. 508, 517; also cf. Church, p. 53). *A fortiori*, a syllogism cannot be a single sentence of a certain kind, as other interpreters have suggested (see below; cf. Corcoran, 'Aristotelian Syllogisms' and cf. Smiley).

Aristotle is quite clear throughout that treatment of scientific knowledge presupposes a treatment of syllogisms (in particular, of perfect syllogisms). In order to be able to produce demonstrative syllogisms one must be able to reason deductively, i.e., to produce perfect syllogisms. Demonstration is a kind of syllogism but not vice versa (25b26–31, 71b22–24). According to our view outlined above, Aristotle's syllogistic includes a theory of deduction which, in his terminology, is nothing more than a theory of perfecting syllogisms. More specifically and in more modern parlance, Aristotle's syllogistic includes a *natural deduction system* by means of which categorical conclusions are deduced from categorical premises. The system countenances two types of deductions (direct and indirect) and, except for 'conversions', each application of a rule of inference is (literally) a first figure syllogism. Moreover, as will be clear below, *Aristotle's theory of deduction* is fundamental in the sense that it *pre-*

supposes no other logic, not even propositional logic.[5] It also turns out that the Aristotelian system (cf. Section 5 below) is complete in the sense that every valid *P-c* argument composed of categorical sentences can be 'demonstrated' to be valid by means of a formal deduction in the system. In Aristotelian terminology this means that every imperfect syllogism can be perfected by Aristotelian methods.

As will become clear below in Section 4, our interpretation is able to account for the correctness of certain Aristotelian doctrines which previous scholars have had to adjudge incorrect. For example, both Łukasiewicz (p. 57) and Patzig (p. 133) agree that Aristotle believed that all deductive reasoning is carried out by means of syllogisms, i.e., that imperfect syllogisms are perfected by means of perfect syllogisms, but they also hold that Aristotle was wrong in this belief (Łukasiewicz, p. 44; Patzig, pp. 135). Rose (p. 55) has wondered how one syllogism can be used to prove another but he did not make the mistake of disagreeing with Aristotle's view. Indeed, in the light of our own research one can see that Rose was very close (p. 53) to answering his own question. We quote in part:

> We have seen how Aristotle establishes the validity of… imperfect [syllogisms]… This amounts to presenting an extended argument with the premises of the imperfect [syllogism]… as… premises… using several intermediate steps, … finally reaching as the ultimate conclusion the conclusion of the imperfect [syllogism]… being established. A natural reaction… is to think of the first figure [syllogisms]… as axioms and the imperfect [syllogisms]… as theorems and to ask to what extent Aristotle is dealing with a formal deductive system.

This would be natural indeed to someone not concerned with formal 'natural' deductive systems. To someone concerned with the latter, it would be natural to consider the first figure syllogisms as 'applications' of rules of inference, to consider the imperfect syllogisms as derived arguments, and then to scrutinize Chapters 2 and 4 (*Prior Analytics* I) in search of parts needed to complete the specification of a natural deductive system. What Rose calls 'an extended argument' is simply a deduction or, in Aristotle's terms, a discourse got by perfecting an imperfect syllogism. Rose had already seen the relevance of pointing out (p. 10) that the term 'syllogism' had been in common use in the sense of 'mathematical computation'. One would not normally apply the term 'computation' to mere data-and-answer reported in the form of an equation, e.g. $(330+1955=$ $=2285)$. The *sine qua non* of a computation would seem to be the intermediate steps, and one might be inclined to call the mere data-plus-answer complex an 'imperfect computation' or a 'potential computation'. A 'perfect' or 'completed' computation would then be the entire complex of data, answer and intermediate steps. At one point Patzig seems to have been closer to our view than Rose. We quote from Patzig (p. 135), who sometimes uses 'argument' for 'syllogism'.

> ... the odd locution 'a potential argument' (synonymous with 'imperfect argument'...) which, as was shown, properly means 'a potentially *perfect* argument'... has no clear sense unless we assume that Aristotle intended to state a procedure by which 'actual' syllogisms could be produced from these 'potential' ones, i.e., actually evident syllogisms produced from potentially evident ones.

Although Rose seems to have missed our view by failing to consider the possibility of a natural deduction system in Aristotle, Patzig was diverted in less subtle ways, as well. In the first place Patzig uncritically accepted the false conclusion of previous interpreters that all perfect syllogisms are in the first figure and thus arrives at the strange view that imperfect syllogisms are "as it were disguised first figure syllogisms" (*loc. cit.*). Secondly, and surprisingly, Patzig (p. 136) seems to be unaware of the distinction between a valid *P-c* argument and a sound deduction having the same premises and conclusion.

1.5. *The Łukasiewicz View and Its Inadequacies*

In order to contrast our view with the Łukasiewicz view it is useful to represent categorical statements with a notion which is mnemonic for readers of twentieth century English.

Amd	*All m* are *d*.
Smd	*S*ome *m* is *d*.
Nmd	*N*o *m* is *d*.
$md	*S*ome *m* is *not d*.

Łukasiewicz holds that Aristotle's theory of syllogistic is an axiomatic science which presupposes 'a theory of deduction' unknown to Aristotle (p. 14, 15, 49). The *universe* of the Łukasiewicz science is the class of secondary substances (man, dog, animal, etc.) and the relevant *relations* are those indicated above by *A*, *N*, *S*, and $, i.e., the relations of inclusion, disjointness, partial inclusion and partial non-inclusion respectively (pp. 14–15). Accordingly, he understands Aristotle's schematic letters (*alpha,* *beta, gamma, mu, nu, xi, pi, rho* and *sigma*) as variables ranging over the class of secondary substances and he takes A, N, S and \$ as non-logical constants (*ibid.*). Some of the axioms of the Łukasiewicz science correspond to Aristotelian syllogisms. But his axioms are single sentences (not arguments) and they are generalized with respect to the schematic letters (see Mates, *op. cit.*, p. 178). For example, the argument scheme

All Z are Y.
All X are Z.
So All X are Y.

corresponds to the following sort of axiom in the Łukasiewicz system

$$\forall xyz((Azy \,\&\, Axz) \supset Axy).$$

It should be noted, however, that Łukasiewicz does not *use* quantifiers in his reconstruction of Aristotle's syllogistic (p. 83). Universal quantification is nevertheless expressed in the theorems of the Łukasiewicz reconstruction – it is expressed by means of 'free variables', as can be verified by noticing the 'Rule of Substitution' that Łukasiewicz uses (p. 88). Indeed, the deductive system of the underlying logic presupposed by Aristotle (according to Łukasiewicz) is more than a propositional logic – it is what today would be called a free variable logic, a logic which involves truth-functions *and* universal quantification (expressed by free variables). Łukasiewicz refers to the deductive system of the underlying logic as 'the theory of deduction' and he sometimes seems to ignore the fact that a free variable logic is more than simply a propositional logic. [Using propositional logic alone one cannot derive Ayy from Axx (i.e., $\forall yAyy$ from $\forall xAxx$) but in a free variable logic it is done in one step.]

The Łukasiewicz view is ingenious and his book contains a wealth of useful scholarship. Indeed it is worth emphasizing that without his book the present work could not have been done in even twice the time. Despite the value of the book, its viewpoint must be incorrect for the following reasons. In the first place, as mentioned above, Łukasiewicz (p. 44) does not take seriously Aristotle's own claims that imperfect syllogisms are "proved by means of syllogisms". He even says that Aristotle was wrong in this claim. In the second place, he completely overlooks the many passages in which Aristotle speaks of perfecting imperfect syllogisms (e.g., *Prior Analytics*, 27a17, 29a30, 29b1–25). Łukasiewicz (p. 43) understands 'perfect syllogism' to indicate only the [valid] syllogisms in the first figure. This leads him to neglect the crucial fact that Chapters 4, 5 and 6 of *Prior Analytics* deal with Aristotle's theory of deduction. Thirdly, Aristotle is clear in *Posterior Analytics* (I, 10) about the nature of axiomatic sciences and he nowhere mentions syllogistic as a science (Ross, p. 24), but Łukasiewicz still wants to regard the syllogistic as such. (Łukasiewicz does seem uneasy (p. 44) about the fact that Aristotle does not call his basic syllogisms 'axioms'.) Indeed, as Scholz has already noticed (p. 6), Aristotle could not have regarded the syllogistic as a science because to do so he would have had to take the syllogistic as its own underlying logic. Again, were the Łukasiewicz system to be a science in Aristotle's terms then its universe of discourse would have to form a genus (e.g., *Posterior Analytics* I, 28) – but Aristotle nowhere mentions the class of secondary substances as a genus. Indeed, on reading the tenth chapter of the *Posterior Analytics* one would expect that *if* the syllogistic were a science *then* its genus would be mentioned on the first page of *Prior Analytics*. Not only does Aristotle fail to indicate the subject matter required by the Łukasiewicz view, he even indicates a different one – viz. demonstration – but not as a genus (*Prior Analytics*, first sentence).[6] In the fourth place, if the syllogistic were an axiomatic science and *A*, *N*, *S* and $ were relational terms, as Łukasiewicz must have it, then awkward questions ensue: (a) Why are these not mentioned in *Categories*, Chapter 7, where relations are discussed? (b) Why did Aristotle not seek for axioms the simplest and most obvious of the propositions involving these relations, i.e., 'Everything is predicated of all of itself' and 'Everything is predicated of some of itself'? In fact Aristotle may have deliberately avoided 'self-predication', although he surely knew of several reflexive *relations* (identity, equality, congruence). Łukasiewicz counts this as an oversight and adds the first of the above self-predications as a 'new' axiom. In connection with the above questions we may also note that the relations needed in the Łukasiewicz science are of a different 'logical type' than those considered by Aristotle in *Categories* – the former relate secondary substances whereas the latter relate primary substances, Fifth, if indeed Aristotle is axiomatizing a system of true relational sentences on a par with the system of true relational sentences which characterize the ordering of the numbers, as Łukasiewicz must and does claim (pp. 14, 15, 73), then again awkward questions ensue: (a) Why is there no discussion anywhere in the second logic of the general topic of relational sentences? (b) Why does Aristotle axiomatize only one such system? The 'theory of congruence' (equivalence relations) and the 'theory of the ordering of numbers' (linear order) are obvious, similar systems and nowhere does Aristotle even hint at the analogies. Sixth, as Łukasiewicz himself implicitly recognizes in a section called 'Theory of Deduction' (pp. 79–82), if the theory of syllogisms is understood as an axiomatic science then, as indicated above, it would presuppose an underlying logic (which Łukasiewicz supplies). But all indications in the Aristotelian corpus suggest not only that Aristotle regarded the theory of syllogistic as the most fundamental sort of reasoning (Kneale and Kneale, p. 44, and even Łukasiewicz, p. 57) but also that he regarded its logic as *the* underlying logic of all axiomatic sciences.[7] Łukasiewicz himself says, "It seems that Aristotle did not suspect the existence of a system of logic besides his theory of the syllogism" (p. 49). Seventh, the view that syllogisms are sentences of a certain kind and not extended discourses is incompatible with Aristotle's occasional but essential reference to *ostensive* syllogisms and to *per impossibile* syllogisms (41a30–40, 45a23, 65b16, e.g.). These references imply that some syllogisms have *internal structure even over and above 'premises' and 'conclusion'*. Finally, although Łukasiewicz gives a mathematically precise system which obtains and rejects 'laws' corresponding to those which Aristotle obtains and rejects, the Łukasiewicz system neither justifies nor accounts for the methods that Aristotle used. Our point is that the method is what Aristotle regarded as most important. In this connection, Aristotle obtained metamathematical results using methods which are clearly accounted for by the present interpretation but which must remain a mystery on the Łukasiewicz interpretation.[8]

It will be seen that Aristotle's theory of deduction contains a self-sufficient natural deduction system which presupposes no other logic. Perhaps the reason that Aristotle's theory of deduction has been overlooked is that it differs radically from many of the 'standard' modern systems. It has no axioms, it involves no truth-functional combinations and it lacks both the explicit and implicit quantifiers (in the modern sense).

1.6. *The Importance of the Issue*

Universally absent from discussions of this issue is reference to why it is important. My opinion is this: *if* the Łukasiewicz view is correct *then* Aristotle cannot be regarded as the founder of logic. Aristotle would merit this title no more than Euclid, Peano, or Zermelo insofar as these men are regarded as founders, respectively, of axiomatic geometry, axiomatic arithmetic and axiomatic set theory. (Aristotle would be merely the founder of 'the axiomatic theory of universals'.) Each of the former three men set down an axiomatization of a body of information *without* explicitly developing the underlying logic. That is, each of these men put down axioms and regarded as theorems of the system the sentences obtainable from the axioms by logical deductions *but* without bothering to say what a logical deduction is. Łukasiewicz is claiming that this is what Aristotle did. In my view, logic must begin with observations explicitly related to questions concerning the nature of an underlying logic. In short, logic must be explicitly concerned with deductive reasoning.

If Łukasiewicz is correct *then* the Stoics were the genuine founders of logic. Of course, my view is that in the *Prior Analytics* Aristotle developed the underlying logic for the axiomatically organized sciences that he discussed in the *Posterior Analytics* and that he, therefore, is the founder of logic.

2. THE LANGUAGE L

In formulating a logic which is to serve as the underlying logic for several axiomatic sciences it is standard to define a 'master language' which involves: (1) punctuation, (2) finitely many logical constants, (3) infinitely many variables and (4) infinitely many non-logical constants or content words (cf. Church, p. 169). Any given axiomatic science will involve all of the logical constants and all of the variables, but only finitely many content words. The full infinite set of content words plays a role only in abstract theoretical considerations. In Aristotle there is no evidence of *explicit* consideration of a master language, although theoretical considerations involving infinitely many content words do occur in *Posterior Analytics* (I, 19, 20, 21). It is worth noticing that there is no need to postulate *object language* variables for Aristotle's system.

The vocabulary of the master language (L) involved in the present development of Aristotle's logic consists in the four logical constants (A, N, S and \$) and an infinite set U of non-logical constants (u_1, u_2, u_3,...). The latter play the roles of 'categorical terms'. The rule of formation which defines 'sentence of L' is simply the following: a *sentence of L* is the result of attaching a logical constant to a string of two distinct non-logical constants. Thus each sentence of L is one of the following where x and y are distinct content words: Axy, Nxy, Sxy, $\$xy$.

It is to be emphasized that no sentence of L has two occurrences of the same content word (or non-logical constant). This means, in the above terminology, that the system eschews self-predication. Self-predication is here avoided because Aristotle avoids it in the system of the *Prior Analytics* (so our model needs to do so for faithfulness) and also because, as J. Mulhern (pp. 111–115) has argued, Aristotle had theoretical reasons for such avoidance. Thus, contrary to the Łukasiewicz interpretation (p. 45), Aristotle's 'omission of the laws of identity' (All X are X; Some X are X) need not be construed as an oversight. The textual situation is the following: In the whole of the passages which contain the 'second logic' there is no appearance of self-predication. The only appearance of self-predication in *Analytics* is in the second book of *Prior Analytics* (63b40–64b25), which was written later. In this passage the sentences 'No knowledge is knowledge' and 'Some knowledge is not knowledge' appear as conclusions of syllogisms with contradictory premises and there are ample grounds for urging the extrasystematic character of the examples. In any case, no affirmative self-predications occur at all. Indeed, it may be possible to explain the absence of a doctrine of logical truth in Aristotle as being a practical 'consequence' of the fact that there are no logically true sentences in his abstract language.

It is readily admitted, however, that the reader's subjective feelings of 'naturalness' will color his judgment concerning which of the choices is an interpolation. If self-predications are thought to be 'naturally present' then our decision to exclude them will seem an interpolation. On the other hand, if they are thought to be 'naturally absent' then the Łukasiewicz inclusion will seem an interpolation. The facts that they do not occur in the second logic and that the system works out without them may tip the scales slightly in favor of the present view. Perhaps further slight evidence that Aristotle *needed* to exclude them can be got by noticing that the mood *Barbara* with a necessary major and necessary conclusion (regarded as valid by Aristotle) is absurdly invalid when the predicate and middle are identical.

Some may also question our omission of the 'indefinite propositions' like 'Men are greedy' which lack 'quantification' (cf. M. Mulhern, p. 51). Although these are mentioned by Aristotle, he seems to treat them as extra-systematic insofar as his system of scientific reasoning is concerned. In the first book of *Prior Analytics* (43a24–44) Aristotle also seems to exclude both adjectives and proper names from scientific languages. Łukasiewicz (p. 7) seems correct in saying that both the latter were banned because neither can be used both in subject and in predicate positions (also see Kneale and Kneale, p. 67 and Patzig, p. 6). It must also be noted that our model makes no room for relatives (and neither does the Łukasiewicz interpretation).

Even if subsequent research shows that these opinions are incorrect, our model need not be changed. However, its significance will change. Inclusion of proper nouns, adjectives, relatives and/or indefinite propositions would imply only *additions* to our model; no other changes would be required. Our language seems to be a sublanguage, at least, of any faithful analogue of the abstract language of Aristotle's system.[9]

The language L (just defined) is an abstract mathematical object designed in analogy with what might be called the ideal language envisaged in Aristotle's theory of scientifically meaningful statements. In effect each sentence in L should be thought of as representing a specific categorical proposition. The structure of a sentence in L is supposed to reflect the structure of the specific categorical proposition it represents. For example, if u and v represent the universals 'man and 'animal' then the structure of Auv should reflect the structure of the proposition 'All men are animals'. It is to be emphasized that a sentence in L is supposed to represent a particular proposition (as envisaged by Aristotle's theory) and *not* a propositional form, propositional function, proposition scheme or anything of the sort. There is no need within Aristotle's theory, nor within our model, of postulating the existence of propositional functions, propositional schemes or even object language variables. Our view is that Aristotle used metalinguistic variables, but that he neither used nor had a doctrine concerning object language variables.[10]

2.1. *Topical Sublanguages*

As was said above, Aristotle developed his logic largely (but not solely) as the underlying logic of the various sciences. In the first book of *Posterior Analytics*, Aristotle develops his view of the organization of sciences and at several places therein he makes it clear that each science has its own genus and its own peculiar terms (*Posterior Analytics* I; 7, 9, 10, 12, 28). A given science can have only finitely many terms (88b6–7; cf. Barnes, p. 123; Ross, p. 603) and it is somehow wrong (impossible?) to mix terms from different sciences.[11] Aristotle even goes so far as to claim that a proposition which seems common to two sciences is really two analogous propositions (76a37–b2).

We conclude that each science has its own *finite* language. We call such a special language a 'topical sublanguage' of the 'master' language. The notion of 'base' in Lewis and Langford (p. 348) corresponds to the finite vocabulary of terms of a topical sublanguage. It is very likely that Aristotle would have regarded his master language not as literally infinite but rather as indefinitely large or perhaps as potentially infinite.

2.2. *Grammatical Concepts*

Once the language has been defined, we can define some useful concepts which depend only on the language, i.e., which are independent of semantic and/or deductive notions. As above, a *premise-conclusion argument* (*P-c* argument) is a set *P* of sentences together with a single sentence *c*; *P* is called the *premises* and *c* is called the *conclusion*. Four things are to be noted at this point. First, Aristotle seems to have no term equivalent in meaning to '*P-c* argument'; each time he refers by means of a common noun to a *P-c* argument it is always by means of the term 'syllogism' which carries the connotation of validity (cf. Rose, p. 27). Second, Aristotle never refers to *P-c* arguments having the empty set of premises (which is not surprising, if only because none are valid). Third, although the 'laws of conversion' involve arguments having only a single premise, Aristotle did not recognize that fact, insisting repeatedly that every syllogism must have at least two premises (e.g., *Prior Analytics*, 42a8, 53b19; *Posterior Analytics* 73a9). Fourth, there is no question that Aristotle treated, in detail, syllogisms with more than two premises (e.g., *Prior Analytics* I, 23, 25, 42; *Posterior Analytics* I, 25, also see above). In fact, *Posterior Analytics* implicitly considers syllogisms whose premises are all of the axioms of a science (*Posterior Analytics* I, 10) and it explicitly considers the possibility of syllogisms with infinitely many premises (*Posterior Analytics* I, 19, 20, 21).

Underlying much of Aristotle's thought (but never explicitly formulated) is the notion of *form of argument*, but only in the relational sense in which one argument can be said *to be in the same form as* another. This notion is purely syntactic and can be defined given the language alone. In particular, let (P, c) and (P', c') be two arguments. (P, c) *is in the same form as* (P', c') if and only if there is a one-one correspondence between their respective sets of content words so that substitution according to the correspondence converts one argument into the other. In order to exhibit examples let us agree to represent an argument by listing the premises and conclusion – indicating the conclusion by a question mark.

Example 1: The following two arguments are in the same form by means of the one-one correspondence on the right:

Aab	Acd	a	c
Sbc	Sda	b	d
$\$ab$	$\$cd$	c	a
$?Ncd$	$?Nae$	d	e

Example 2: In the following pairs the respective arguments are not in the same form:

Aab	Aab	Aab	Aab	Aab	Aab
Sbc	Sbc	Sac	$\$ac$	$?Nac$	$\$ac$
$?Nac$	$?Nca$	$?\$ac$	$?\$ac$		$?Nac$

It follows from the definition that in order for two arguments to be in the same form, it is necessary that they have (1) the same number of premises, (2) the same number of distinct content words and (3) the same number of sentences of any of the four kinds.

It is obvious that one need know absolutely nothing about how the sentences in L are to be interpreted or how one 'reasons' about their logical interrelations in order to be able to decide whether two arguments are in the same form. Relative to this system, the notion of form is purely grammatical (cf. Church. pp. 2–3).

Define $P+s$ as the result of adjoining the sentence s with the set P.

Finally we define Nxy and Axy to be *contradictories* respectively of Sxy and $\$xy$ (and vice versa) and we define the function C which when applied to a sentence in L produces its contradictory. The table of the function is given below.

	C
Axy	*$xy*
Nxy	*Sxy*
Sxy	*Nxy*
$xy	*Axy*

3. The semantic system *S*

Aristotle regarded the truth-values of the non-modal categorical propositions as determined extensionally (*Prior Analytics*, 24a26 ff.).[12] Thus, for Aristotle: (1) 'All X is Y' is true if the extension of X is included in that of Y; (2) 'No X is Y' is true if the extension of X is disjoint with that of Y; (3) 'Some X is Y' is true if an object is in both extensions and (4) 'Some X is not Y' is true if some object in the extension of X is outside of the extension of Y. Thus, given the meanings of the logical constants, the truth-values of the categorical sentences are determined by the extensions of the universals involved in the manner just indicated. Now imagine that the content words (characters in U) are correlated with the secondary substances (sortal universals) and consider the following *interpretation* i of L. The interpretation ix of the content word x is the extension of the secondary substance correlated with x. Given i we can easily define a function V^i which assigns the correct truth-value to each sentence in L as follows:

(1) $V^i(Axy) = t$ if ix is included in iy,
$V^i(Axy) = f$ if ix is not included in iy.
(2) $V^i(Nxy) = t$ if ix is disjoint with iy,
$V^i(Nxy) = f$ if ix is not disjoint with iy.
(3) $V^i(Sxy) = t$ if ix is not disjoint with iy,
$V^i(Sxy) = f$ if ix is disjoint with iy.
(4) $V^i(\$xy) = t$ if ix is not included in iy,
$V^i(\$xy) = f$ if ix is included in iy.

The function i defined above may be regarded as the *intended interpretation* of L. In order to complete the construction of the semantics for L we must specify, in addition, the non-intended or 'possible' interpretations of L. The non-intended interpretations of a language are structures which share all 'purely logical' features with the intended interpretation. What is essential to the intended interpretation is that it assigns to each content word a set of primary substances (individuals) which 'could be' the extension of a secondary substance. Since Aristotle held that every secondary substance must subsume at least one primary substance (*Categories*, 2a34–2b7), we give the following general definition of an interpretation of L: j is an *interpretation* of L if and only if j is a function which assigns a non-empty set[13] to each member of U. The general definition of truth-values of sentences of L under an arbitrary interpretation j is exactly the same as that for the intended interpretation.

The absence of the notion of universe of discourse warrants special comment if only because it is prominent, not only in modern semantics but also in Aristotles treatment of axiomatic science (see above). In the first place, this concept plays no role in the system of the *Prior Analytics*, which is what we are building a model for. So we deliberately leave it out, although from a modern point of view it is unnatural to do so. [Of course, in an underlying logic based on a topical sublanguage, universes of discourse *are* needed (each science has its genus). To supply them we would require that, for each j, each jx is a subclass of some set, say Dj, given in advance. Its omission has no mathematical consequences.] In the second place there may be a tradition (cf. Jaskowski, p. 161; Patzig, p. 7) which holds that Aristotle prohibited his content words from having the universe as extension. (So both the null set *and* the universe would be excluded. Since the universe of sets is not itself a set, our definitions respect the tradition without special attention – and perhaps without special significance.[14])

It must be admitted that Aristotle nowhere makes specific reference to alternative interpretations nor does he anywhere perform operations which suggest that he had envisaged alternative interpretations. Rather it seems that at every point he thought of his ideal language as interpreted in what we would call its intended interpretation. Moreover, it is doubtful that Aristotle ever conceived of a language apart from its intended interpretation. In other words, it seems that Aristotle did not separate logical syntax from semantics (but cf. *De. Int.*, chapter 1 and *Soph. Ref.*, chapter 1).

3.1. *Semantic Concepts*

In terms of the semantics of L just given, we define some additional useful notions as follows. A sentence s is said to be *true* [*false*] *in an interpre-*

tation j if $V^j(s) = t$ [$V^j(s) = f$]. If s true in j then j is called *a true interpretation of s*. If P is a set of sentences all of which are true in j then j is called a *true interpretation of P* and if every true interpretation of P is a true interpretation of c then P is said to (logically) *imply c* (written $P \models c$). If P implies c then the argument (P, c) is *valid*, otherwise (P, c) is *invalid*. A *counter interpretation* of an argument (P, c) is a true interpretation of the premises, P, in which the conclusion, c, is false. When (P, c) is valid, c is said to be a *logical consequence*[15] of P.

By reference to the definitions just given one can show the following important semantic principle – which is suggested by Aristotle's 'contrasting instances' method of establishing invalidity of arguments (below and cf. Ross, pp. 28, 292–313 and Rose, pp. 37–52).

(3.0) *Principle of counter interpretations. A premise-conclusion argument is invalid if and only if it has a counter interpretation.*

The import of this principle is that whenever an argument is invalid it is possible to reinterpret its content words in such a way as to make the premises true and the conclusion false. It is worth remembering that the independence of the Parallel Postulate from the other 'axioms' of geometry was established by construction of a counter interpretation, a reinterpretation of the language of geometry in which the other axioms were true and the Parallel Postulate false (cf. Cohen and Hersh, and also, Frege, pp. 107–110).[16]

Perhaps the most important semantic principle underlying Aristotle's logical work is the following, also deducible from the above definitions.

(3.1.) *Principle of Form: An argument is valid if and only if every argument in the same form is also valid.*

Aristotle tacitly employed this principle[17] throughout the *Prior Analytics* in two ways. First, to establish the validity of all arguments in the same form as a given argument, he establishes the validity of an arbitrary argument in the same form as the argument in question (i.e. he establishes the validity of an argument leaving its content words unspecified). Second, to establish the invalidity of all arguments in the same form as a given argument, he produces a specific argument in the required form for which the intended interpetation is a counter interpretation.[18] The latter, of course, is the method of 'contrasting instances'. In neither of these operations, which are applied repeatedly by Aristotle, is it necessary to postulate either alternative interpretations or argument forms (over and above individual arguments; cf. Sections 3.2 and 3.3 below).

The final semantic consideration is the semantic basis of what will turn out to be Aristotle's theory of deduction. The clauses of the following principle are easily established on the basis of the above definitions.

(3.2.) *Semantic Basis of Aristotle's Theory of Deduction: let x, y, and z be different members of U. Let P be a set of sentences and let d and s be sentences.*

Law of Contradictions:

(C) For all j, $V^j(s) \neq V^j(C(s))$,
[i.e., in every interpretation, contradictions have different truth values].

Conversion Laws:

(C1) $Nxy \vDash Nyx$.
(C2) $Axy \vDash Syx$.
(C3) $Sxy \vDash Syx$.

Laws of Perfect Syllogisms:

(PS1) $\{Azy, Axz\} \vDash Axz$.
(PS2) $\{Nzy, Axz\} \vDash Nxy$.
(PS3) $\{Azy, Sxz\} \vDash Sxy$.
(PS4) $\{Nzy, Sxz\} \vDash \$xy$.

Reductio Law:

(R) $P \vDash d$ *if* $P + C(d) \vDash s$ and $P + C(d) \vDash C(s)$.

The law of contradictions, the conversion laws, and the laws of perfect syllogisms are familiar and obvious. The *reductio* law says that for d to follow from P it is sufficient that P and the contradiction of d together imply both a sentence s and its contradictory $C(s)$. Although Aristotle regarded all of the above clauses as obviously true, he does not completely neglect metalogical questions[19] concerning them.

As far as I can tell Aristotle did not raise the metalogical question concerning *reductio* reasoning in the *Analytics*. In Chapter 2 of the first book of the *Prior Analytics* he puts down the conversion laws and then offers what seem to be answers to the metalogical questions concerning their validity. Specifically, he establishes (C1) by a kind of metasystematic *reductio* proof which presupposes (1) non-emptiness of term-extensions, (2) contradictory opposition between Nxy and Sxy, and (3) that existence of an object having properties x and y precludes the truth of Nyx. Then, taking (C1) as established, he establishes (C2) and (C3) by *reductio* reasoning. Two chapters later he gives obviously semantic justification for the four laws of perfect syllogisms.

3.2. *An Alternative Semantic System*

Instead of having a class of interpretations some logicians prefer to 'do as much semantics as possible' in terms of the following two notions: (1) truth-valuation in the intended interpretation and (2) form (cf. Quine, *Philosophy*, p. 49 and Corcoran, 'Review'). Such logicians would have a semantic system containing exactly one interpretation, the intended interpretation, and they would *define* an argument to be valid if every argument in the same form with true premises (relative to the intended interpretation) has a true conclusion (relative to the intended interpretation). Ockham's razor would favor the new 'one-world' semantics over the above 'possible-worlds' semantics (Quine, *op. cit.*, p. 55). Within a framework of a one-world semantics *invalidity* would be established in the same way as above (and as in Aristotle).

It does not seem possible to establish by reference to the Aristotelian corpus whether one semantic system agrees better with Aristotle's theory than the other. The main objection to the one-world semantics is that it makes logical issues depend on 'material reality' rather than on 'logical possibilities'. For example, *if* the intended interpretation is so structured that for every pair of content words the extension of one is identical to the extension of the other or else disjoint with it *then* Axy 'logically implies' Ayx. Thus in order to get the usual valid arguments in a one-world semantics it is necessary to make additional assumptions about the intended interpretation (cf. Quine, *op. cit.*, p. 53). Proponents of the one-world semantics prefer additional assumptions concerning 'the real world' to additional assumptions about 'possible worlds'. Since the mathematics involved with the semantics of the previous section involves fewer arbitrary decisions than does the semantics of this section we have chosen to make the former the semantic system of our model of Aristotle's system. It is very likely that proponents of the one-world view could honestly weight the available evidence so that attribution of the one-world semantics to Aristotle is more probable. If the current dialogue between proponents of the two views continues the above may well become an important historical issue.

3.3. *Forms of Arguments*

Above we used the term *form* only in relational contexts: (P, c) is in the same form as (P^*, c^*). During previous readings of this paper, auditors insisted on knowing what logical forms 'really are' and whether Aristotle used them as theoretical entities. Perhaps the best way of getting clear about the first problem is to first see an 'explication' of the notion. The following explication is a deliberate imitation of Russell's explication of *number* in terms of the relation 'has the same number of members as'.

Consider the class of all arguments and imagine that it is partitioned into non-empty subsets so that all and only formally similar arguments are grouped together. Define *Forms* to be these subsets. If we use this notion of *Form*, then many of the traditional uses of the substantive *form* (not the relative) are preserved. Taking *in* in the sense of membership, we can say that (P, c) is in the same form as (P^*, c^*) if and only if (P, c) is a member of the same Form that (P^*, c^*) is a member of.

A Form is simply a set of formally similar arguments. Unfortunately, this clear notion of form is *not* the one that has been traditionally invoked. The traditional 'argument form' is supposed to be like a (real) argument except that it doesn't have (concrete) terms. Putting variables for the terms will not help because new variables can be substituted without changing the 'form'. Proponents of 'forms' fall back on saying that an 'argument form'is that which all formally similar arguments have in common, but (seriously) what can this be except membership in a class of formally similar arguments? In any case there are no textual grounds for imputing to Aristotle a belief in argument Forms (or, for that matter, in 'argument forms', assuming that sense can be made of that notion).

4. The Deductive System D

We have already implied above that a theory of deduction is intended to specify what steps of deductive reasoning may be performed in order to come to know that a certain proposition c follows logically from a certain set *P* of propositions. Aristotle's theory of deduction is his theory of perfecting syllogisms. As stated above, our view is that a perfect syllogism is a discourse which expresses correct reasoning from premises to conclusion. In case the conclusion is immediate, nothing need be added to make the implication clear (24a22). In case the conclusion does not follow immediately, then additional sentences must be added (24b23, 27a18, 28a5, 29a15, 29a30, 42a34, etc.). A valid argument by itself is only potentially perfect (27a2, 28a16, 41b33): it is 'made perfect' (29a33, 29b5, 29b20, 40b19, etc.) by, so to speak, filling its interstices.

According to Aristotle's theory, there are only two general methods[20] for perfecting an imperfect syllogism – either directly (ostensively) or indirectly (*per impossibile*) (e.g., 29a30–29bl, 40a30, 45b5–10, 62b29–40, *passim*). In constructing a direct deduction of a conclusion from premises one interpolates new sentences by applying conversions and first figure syllogisms to previous sentences until one arrives at the conclusion. Of course, it is permissible to repeat an already obtained line. In constructing an indirect deduction of a conclusion from premises one adds to the premises, as an additional hypothesis, the contradictory of the conclusion; then one interpolates new sentences as above until both of a pair of contradictory sentences have been reached.

Our deductive system *D*, to be defined presently, is a syntactical mathematical model of the system of deductions found in Aristotle's theory of perfecting syllogisms.

Definition of D. First restate the laws of conversion and perfect syllogisms as rules of inference.[21] Use the terms 'a *D*-conversion of a sentence' to indicate the result of applying one of the three conversion rules to it. Use the terms '*D*-inference from two sentences' to indicate the result of applying one of the perfect syllogism rules to the two sentences.

A *direct deduction in D of c from P* is a finite list of sentences ending with *c*, beginning with all or some of the sentences in *P*, and such that each subsequent line (after those in *P*) is either (a) a repetition of a previous line, (b) a *D*-conversion of a previous line or (c) a *D*-inference from two previous lines.

An *indirect deduction in D of c from P* is a finite list of sentences ending in a contradictory pair, beginning with a list of all or some of the sentences in *P* followed by the contradictory of *c*, and such that each subsequent additional line (after the contradictory of *c*) is either (a) a repetition of a previous line, (b) a *D*-conversion of a previous line or (c) a *D*-inference from two previous lines.

All examples of deductions will be annotated according to the following scheme: (1) Premises will be prefixed by '+' so that '+*Axy*' can be read 'assume *Axy* as a premise'. (2) After the premises are put down we interject the conclusion prefixed by '?' so that '?*Axy*' can be read 'we want to show why *Axy* follows'. (3) The hypothesis of an indirect (*reductio*) deduction is prefixed by '*h*' so that '*hAxy*' can be read 'suppose *Axy* for purposes of reasoning'. (4) A line entered by repetition is prefixed by '*a*' so that '*aAxy*' can be read 'we have already accepted *Axy*'. (5) Lines entered by conversion and syllogistic inference are prefixed by '*c*' and '*s*', respectively. (6) Finally, the last line of an indirect deduction has '*B*' prefixed to its other annotation so that '*BaAxy*' can be read 'but we have already accepted *Axy*', etc. We define an *annotated deduction in D* to be a deduction in *D* annotated according to the above scheme. In accordance with now standard practice we say that *c is deducible from P in D* to mean that there is a deduction of *c* from *P* in the system *D*. It is also sometimes convenient to use the locution 'the argument (*P*, *c*) is deducible in *D*'.

The following is a consequence of the above definitions (cf. Frege, pp. 107–11).

(4.1) *Deductive Principle of Form: An argument is deducible in D if and only if every argument in the same form is also deducible.*

The significance of *D* is as follows. We claim that *D* is a faithful mathematical model of Aristotle's theory of perfecting syllogisms in the sense that every perfect syllogism (in Aristotle's sense) corresponds in a direct and obvious way to a deduction in *D*. Thus what can be added to an imperfect syllogism to render it perfect corresponds to what can be 'added' to a valid argument to produce a deduction in *D*. In the case of a direct deduction the 'space' between the premises and conclusion is filled up in accordance with the given rules.

In order to establish these claims as well as they can be established (taking account of the vague nature of the data), the reader may go through the deductions presented by Aristotle and convince himself that each may be faithfully represented in *D*. We give four examples below; three direct deductions and one indirect deduction. The others raise no problems.

We reproduce two of Aristotle's deductions (27a5–15; Rose, p. 34), each followed by the corresponding annotated deductions in D.

(1)	Let M be predicated of no N	$+Nnm$
	and of All X	$+Axm$
	(*conclusion omitted in text*).	$(?Nxn)$
	Then since the negative premise converts	
	N belongs to no M.	$cNmn$
	But it was supposed that M belongs to all X.	$aAxm$
	Therefore N will belong to no X.	$sNxn$
(2)	Again, if M belongs to all N	$+Anm$
	and to no X,	$+Nxm$
	X will belong to no N.	$?Nnx$
	For if M belongs to no X,	$aNxm$
	X belongs to no M.	$cNmx$
	But M belonged to all N.	$aAnm$
	Therefore X will belong to no N.	$sNnx$

Below we reproduce Aristotle's words (28b8-12) followed by the corresponding annotated deduction in D.

(3)	For if R belongs to all S,	$+Asr$
	P belongs to some S,	$+Ssp$
	P must belong to some R.	$?Srp$
	Since the affirmative statement is convertible	
	S will belong to some P,	$cSps$
	consequently since R belongs to all S,	$aAsr$
	and S to some P,	$aSps$
	R must also belong to some P:	$sSpr$
	therefore P must belong to some R.	$cSrp$

To exemplify an indirect deduction we do the same for 28 b17–20.

(4)	For if R belongs to all S,	$+Asr$
	but P does not belong to some S,	$+\$sp$
	it is necessary that P does not belong to some R.	$?\$rp$
	For if P belongs to all R,	$hArp$
	and R belongs to all S,	$aAsr$
	then P will belong to all S:	$sAsp$
	but we assumed that it did not.	$Ba\$sp$

Readers can verify the following (by 'translating' Aristotle's proofs of the syllogisms he proved, using ingenuity in the other cases).

(4.2) *All valid arguments in any of the four traditional figures*[22] *are deducible in D.*

4.1. *Deductive Concepts*

As is to be expected given the above developments, a deductive concept is one which can be defined in terms of concepts employed in the deductive system without reference to semantics. In many cases one relies on semantic insights for the motivation to delimit one concept rather than another. This is irrelevant to the criterion for distinguishing deductive from semantic concepts; just as reliance on mechanical insight for motivation to define mathematical concepts is irrelevant to distinguishing physical and mathematical concepts.

Already several deductive notions have been used – 'direct deduction', 'indirect deduction', 'rule of inference', 'deducible from', 'contradictory' (as used here), etc. Relative to *D* the notion of consistency is defined as follows. A set *P* of sentences is *consistent* if no two deductions from *P* have contradictory conclusions. If there are two deductions from *P* one of which yields the contradictory of the conclusion of the other then, of course, *P* is *inconsistent.*

Aristotle did not have occasion to define the notion of inconsistency but he showed a degree of sophistication lacking in some current thinkers by discussing valid arguments having inconsistent premise sets[23] (63b40-64b25).

4.2. *Some Metamathematical Results in Aristotle*

Generally speaking, a metamathematical result is a mathematical result concerning a logical or mathematical system. Such results can also be called metasystematic. The point of the terminology is to distinguish the results codified by the system from results concerning the system itself. The latter would necessarily be stated in the metalanguage and codified in a metasystem. It is also convenient (but sometimes artificial) to distinguish intrasystematic and intersystematic results. The former would concern mathematical relations among parts of the given system whereas the latter would concern mathematical relations between the given system and another system. The artificiality arises when the 'other' system is acactually a part of the given system.

It is worth noting that the theorem/metatheorem confusion cannot arise in discussion of Aristotle's syllogistic for the reason that there are no theorems. This observation is important but it is not deep. It is simply a reflection of two facts: first, that within the passages treating the second logic Aristotle did not consider the possibility of 'logical truths' (object language sentences true in virtue of logic alone); second, and more importantly, that Aristotle regarded logic as a 'canon of inference' rather than as a codification of 'the most general laws of nature'.

Given the three-part structure of a logic one can anticipate four kinds of metasystematic results: 'grammatical' results which concern the language alone; 'semantic' results which concern the language and the semantic system; 'proof-theoretic' results which concern the language and the deductive system; and 'bridge' results which bridge or interrelate the semantic system with the deductive system. Since the Aristotelian grammar is so trivial, there is nothing of interest to be expected there. The semantics, however, is complex enough to admit of analogues to modern semantic results. For example, the analogue to the Löwenheim–Skolem theorem is that any satisfiable set of sentences of L involving no more than n content words is satisfiable in a universe of not more than 2^n objects (for proof see Corcoran, 'Completeness'). Unfortunately there are no semantic results (in this sense) in Aristotle's 'second logic'. As mentioned above, Aristotle may not have addressed himself to broader questions concerning the semantic system of his logic. As is explained in detail below, most of Aristotle's metasystematic results are proof-theoretic: they concern the relationship between the deductive system D and various subsystems of it. There is, however, one bridge result, viz., the completeness of the deductive system relative to the semantics. Unfortunately, Aristotle's apparent inattention to semantics may have prevented him from developing a rigorous proof of completeness.

There are several metasystematic results in the 'second logic', none of which have been given adequate explanation previously. We regard an explanation of an Aristotelian metasystematic result to be adequate only when it accounts for the way in which Aristotle obtained the result.

4.2.1. *Aristotle's Second Deductive System D2.* As already indicated above, the first five chapters of the 'second logic' (*Prior Analytics* I, 1, 2, 4, 5, 6) include a general introductory chapter, two chapters presenting the system and dealing with the first figure and two chapters which present deductions for the valid arguments in the second and third figures.[24] The next chapter (Chapter 7) is perhaps the first substantial metasystematic discussion in the history of logic.

The first interesting metasystematic passage begins at 29a30 and merely summarizes the work of the preceding three chapters. It reads as follows

It is clear too that all the imperfect syllogisms are made perfect by means of the first figure. All are brought to conclusion either ostensively or *per impossibile*.

From the context it is obvious that by 'all' Aristotle means 'all second and third figure'. Shortly thereafter begins a long passage (29b1–25) which states and proves a substantial metasystematic result. We quote (29b1–2)

It is possible also to reduce all syllogisms to the universal syllogisms in the first figure.

Again 'all' is used as above; 'reduce to' *here* means 'deduce by means of' and 'universal syllogism' means 'one having an *N* or *A* conclusion'. What Aristotle has claimed is that all of the syllogisms previously proved can be established by means of deductions which do not involve the 'particular' perfect syllogistic rules (PS3 and PS4). Aristotle goes on to explain in concise, general, but mathematically precise terms exactly how one can construct the twelve particular deductions which would substantiate the claim. Anyone can follow Aristotle's directions and thereby construct the twelve formal deductions in our system *D*.

In regard to the validity of the present interpretation these facts are significant. Not only have we accounted for the content of Aristotle's discovery but we have also been able to reproduce exactly the methods that he used to obtain them. Nothing of this sort has been attempted in previous interpretations (cf. Łukasiewicz, p. 45).

Let *D2* indicate the deductive system obtained by deleting PS3 and PS4 from *D*. Aristotle's metaproof shows that the syllogisms formerly deduced in *D* can also be deduced in *D2*. On the basis of the next chapter (*Prior Analytics* I, 23) of the 'second logic' (cf. Bocheński, p. 43; Łukasiewicz, p. 133; Tredennick, p. 185) it becomes clear that Aristotle thinks that he has shown that *every* syllogism deducible in *D* can also be deduced in *D2*. On reading the relevant passages (29b1-25) it is obvious that Aristotle has not proved the result. However, it is now known that the result is correct; it follows immediately from the main theorem of Corcoran 'Completeness' (*q.v.*). But regardless of the correctness of Aristotle's proof one must credit him with conception of the first significant hypothesis in proof theory.

4.2.2. *Redundancy of Direct Deductions.* Among indirect deductions it is interesting to distinguish two subclasses on the basis of the role of the added hypothesis. Let us call an indirect deduction *normal* if a rule of inference is applied to the added hypothesis and *abnormal* otherwise. In many of the abnormal cases, one reasons from the premises ignoring the added hypothesis until the desired conclusion is reached and then one notes 'but we have assumed the contradictory'.

Aristotle begins Chapter 29 (*Prior Analytics* I) by stating that whatever can be proved directly can also be proved indirectly. He then gives two examples of normal indirect deductions for syllogisms he has already deduced directly. Shortly thereafter (45b1–5) he says,

> Again if it has been proved by an ostensive syllogism that *A* belongs to no *E*, assume that *A* belongs to some *E* and it will be proved *per impossibile* to belong to no *E*. Similarly with the rest.

The first sentence means that by interpolating the added hypothesis *Sea* into a direct deduction of *Nea* one transforms it to an indirect deduction of the same conclusion. See the diagram below.

+———		+———
+———		+———
+———		+———
?*Nea*	Transforming to:	?*Nea*
		hSea
———		———
———		———
———		———
———		———
Nea		*Nea*
		BaSea

The second quoted sentence is meant to indicate that the same result holds regardless of the form of the conclusion. In other words, Aristotle has made clear the fact that whatever can be deduced by a direct deduction can also be deduced by an abnormal indirect deduction, i.e., that direct deductions are redundant from the point of view of the system as a whole.[25]

We feel that this is additional evidence that Aristotle was self-consciously studying interrelations among deductions – exactly as is done in Hilbert's 'proof theory' (e.g., cf. van Heijenoort, p. 137).

4.3. *Indirect Deductions or a Reductio Rule?*

To the best of my knowledge Aristotle considered indirect reasoning to be a certain style of deduction. After the premises are set down one adds the contradictory of what is to be proved and then proceeds by 'direct reasoning' to each of a pair of contradictory sentences. Imagine, however, the following situation: one begins an indirect deduction as usual and immediately gets bogged down. Then one sees that there is a pair of contradictories, say s and $C(s)$, such that (1) s can be got from what is already assumed by indirect reasoning and (2) that $C(s)$ can be got from s together with what is already assumed by direct reasoning.

In a normal context of mathematics there would be no problem – the outlined strategy would be carried out without a second thought. In fact the situation is precisely what is involved in a common proof of 'Russell's Theorem' (no set contains exactly the sets which do not contain themselves). It involves using *reductio* reasoning as a structural rule of inference (cf., e.g., Corcoran, 'Theories', pp. 162ff). The trouble is that the strategy requires the addition of a *second* hypothesis and this is not countenanced by the Aristotelian system (41a33–36).

The salient differences between a system with indirect deductions and a system with a *reductio* rule are the following. In the case of indirect deductions, one can add but one additional hypothesis (viz. the contradictory of the conclusion to be reached) and one cannot in general use an indirectly obtained conclusion later on in a deduction. Once the indirectly obtained conclusion is reached the indirect deduction is, by definition, finished. An indirectly obtained conclusion is never written as such in the deduction. In the case of the *reductio* rule one can add as many additional hypotheses as desired; once an indirectly obtained conclusion is reached it is written as an intermediate conclusion usable in subsequent reasoning.

The deductive system of Jeffrey (*q.v.*) consists solely of indirect deductions whereas the system of Anderson and Johnstone (*q.v.*) has a *reductio* rule.

Metamathematically, one important difference is the following. Where one has a *reductio* rule it is generally easy to prove the metamathematical result that $C(d)$ is (indirectly) deducible from P whenever each of a pair of contradictions is separately deducible from $P + d$. This result can be difficult in the case where one does not have a *reductio* rule – especially when each of the pair of contradictions was reached indirectly.

In order to modify the system (or systems) to allow such 'iterated or nested *reductio* strategies' one would abandon the distinction between direct and indirect deductions; in the place of the indirect deductions one would have (simply) deductions which employ one or more applications of a *reductio* rule. Statements of such *reductio* rules are in general easily obtained *but* they involve several ideas which would unnecessarily complicate this article. Let us assume that *D2* has been modified[26] to permit iterated or nested *reductio* deductions and let us call the new system *D3*.

Now we have two final points to make. In the first place, in one clear sense, nothing is gained by adding the *reductio* rule because, since *D2* is known to be complete and *D3* is sound, every argument deducible in *D3* is already deducible in *D2*. In the second place, Aristotle may well have been thinking of *reductio* as a rule of inference but either lacked the motivation to state it as such or else actually stated it as such only to have his statements deleted or modified by copyists. It may even be the case that further scholarship will turn up convincing evidence for a *reductio* rule in the extant corpus. This is left as an open problem in Aristotle scholarship.[27]

4.4. *Extended Deductions*

In the course of a development of an axiomatic science it would be silly, to say the least, to insist on starting each new deduction from scratch. We quite naturally use as premises in each subsequent deduction not only the axioms of the science but also any or all previously proved theorems. Thus at any point in a development of an axiomatic science the last theorem proved is proved not by a deduction from the axioms but rather by a deduction from the axioms *and* previously proved theorems. In effect, we can think of the entire sequence of deductions, beginning with that of the first theorem and ending with that of the last proved theorem as an 'extended deduction' with several conclusions. If the basic deductive system is D (above) then the 'extended deductions' can be defined recursively as follows. (In D we define '*deduction of c from P*' where c is an individual sentence. Now we defined '*extended deduction of C from P*' where C is a set of sentences.)

Definition of Deductive System DE.

(a) All direct and indirect deductions in D of c from P are extended deductions in DE of $\{c\}$ from P.

(b) *If* F' is an extended deduction in DE of C from P and F is a deduction in D of d from $P + C$ *then* the result of adjoining F to the end of F' is an extended deduction in DE of $C + d$ from P.

Thus an extended deduction in DE of $\{c_1, c_2, \ldots, c_n\}$ from P could be (the concatenation of) a sequence of *component deductions* (all in D) the $i + 1$st of which is a deduction of c_{i+1} from one or more members of $P + \{c_1, c_2, \ldots, c_i\}$. Soundness of the system of extended deductions is almost immediate given the following principle which holds in the 'possible-worlds' semantics of Section 3 above.

(4.0) *Semantic Principle of Extended Deduction:*

$$P \vDash d \text{ if } P + C \vDash d \text{ and, for all } s \text{ in } C, P \vDash s.$$

The significance of the system of extended deductions is as follows. In the first place, it is natural (if not inevitable) to consider such a system in the course of a study of axiomatic sciences. Thus, we must consider the possibility that the underlying logic of the axiomatic sciences discussed in *Posterior Analytics* had as its deductive system a system similar to the system of extended deductions. Secondly, this system loosens to some extent the constraint of not being able to *use* indirectly obtained results in deductions in D. (Although the constraint there resulted from an absence of a *reductio* rule, strictly speaking, there is still no *reductio* rule in DE.)[28]

It may be relevant to point out here that, since an Aristotelian science has only a finite number of principles (axioms *and* theorems), for formal purposes each science can be identified with a single extended deduction.

Here we wish to consider briefly the possibility that the underlying logic presupposed in *Posterior Analytics* is a system of extended deductions. At the outset, we should say that there are no grounds whatsoever for thinking that Aristotle restricted the use of the term 'demonstration' to the two-premise cases. Next we note that *if Posterior Analytics* requires a system of extended deductions *then* there are grounds for limiting the component deductions (direct and indirect) to ones having at most two premises. Thus we are considering the possibility that every 'demonstration' is an extended deduction whose components are all deductions having one or two premises. If this possibility were established, it could provide an alternative account for the passages where 'syllogism' is clearly used in the restricted sense, given that there are passages which refer to demonstrations *as chains of syllogisms*. The latter, however, do not seem to exist in *Analytics* (cf. 25b27, 71b17, 72b28, 85b23), but there is one tempting passage in *Topics* (100a27). In any case, we have been unsuccessful in our attempt to construct persuasive support for this possibility. (cf. Smiley.)

5. THE MATHEMATICAL LOGIC *I*

In the previous three sections we considered the components of several mathematical logics any one of which could be taken as a reasonably faithful model of the system (or systems) of logic envisaged in Aristotle's theory (or theories) of syllogistic. The model (hereafter called *I*) which we take to be especially important has *L* as language, *S* as semantics and *D* as deductive system. It is our view that *I* is the system most closely corresponding to Aristotle's explicit theory.[29]

Concerning any mathematical logic there are two kinds of questions. In the first place, there are *internal* questions concerning the mathematical properties of the system itself. For example, we have compared the deductive system *D* with the semantics *S* by asking whether every deducible argument is valid (problem of soundness) and conversely whether every valid argument is deducible (problem of completeness). Both of these questions and all other internal questions are perfectly definite mathematical questions concerning the logic *as a mathematical object*. And if they are answered, then they are answered by the same means used to answer any mathematical question – viz. by logical reasoning from the definitions of the systems together with the relevant mathematical laws. In the second place, there are *external* questions concerning the relationship of the model to things outside of itself. In our case the most interesting question is a fairly vague one – viz. how well does our model represent 'the system' treated in Aristotle's theory of the syllogism?

As the various components of the model were developed, we considered the external questions in some detail and concluded that the model can be used to account for many important aspects of the development of Aristotle's theory, as recorded in the indicated parts of *Analytics*. Moreover, the logic *I* adds nothing to what Aristotle wrote except for giving an explicit reference to 'possible worlds' and formulating a systematic definition of formal deductions. It is especially important to notice that the deductive system involves nothing different in kind from what Aristotle explicitly used – no 'new axioms' were needed and no more basic sort of reasoning was presupposed.

As far as internal questions are concerned it is obvious that *I* is sound, i.e., that all arguments deducible in *D* are valid. This is clear from Section 3 above. The completeness of *I* has been proved[30] – i.e., we have been able to demonstrate as a mathematical fact concerning the logic *I* that every argument valid according to the semantics *S* can be obtained by means of a formal deduction in *D*. Thus not only is Aristotle's logic self-sufficient in the sense of not presupposing any more basic logic but it is also self-sufficient in the sense that no further sound rules can be added without reduncancy.

5.1. *The Possibility of a Completeness Proof in Prior Analytics*

According to Bocheński's view (p. 43), in which we concur, Chapter 23 follows Chapter 7 in *Prior Analytics,* Book I. As already indicated Chapter 7 shows that all syllogisms in the three figures are "perfected by means of the universal syllogisms in the first figure". Chapter 23 (40b17–23) begins with the following words.

It is clear from what has been said that the syllogisms in these figures are made perfect by means of universal syllogisms in the first figure and are reduced to them. That every syllogism without qualification can be so treated will be clear presently, when it has been proved that every syllogism is formed through one or the other of these figures.

The same chapter (41b3–5) ends thus.

But when this has been shown it is clear that every syllogism is perfected by means of the first figure and is reducible to the universal syllogisms in this figure.

From these passages *alone* we might suppose that the intermediate material contained the main part of a completeness proof for *D2*, which depended on a 'small' unproved lemma. We might further suppose that the imagined completeness proof had the following three main parts. First, it would define a new deductive system which had the syllogisms in all three figures as rules. Second, it would prove the completeness of the new system. Third, it would show that every deduction in the new system can be transformed into a deduction in *D2* having the same premises and conclusion.

Unfortunately, the text will not support this interpretation. Before considering a more adequate interpretation one can make a few historical observations. In the first place, even raising a problem of completeness seems to be a very difficult intellectual achievement. Indeed, neither Boole nor Frege nor Russell asked such questions.[31] Apparently no one stated a completeness problem[32] before it emerged naturally in connection with the underlying logic of modern Euclidean geometry in the 1920's (Corcoran, 'Classical Logic', pp. 41, 42), and it is probably the case that no completeness result (in this *exact* sense) was printed before 1951 (cf. Corcoran, 'Theories', p. 177 for related results), although the necessary mathematical tools were available in the 1920's. In the second place, Aristotle does not seem to be clear enough about his own semantics to understand the problem. If he had been, then he could have solved the problem definitively for any finite 'topical sublogic' by the same methods employed in *Prior Analytics* (I, 4, 5, 6). In fact, in these chapters he 'solves' the problem for a 'topical sublogic' having only three content words.

In the intervening passages of Chapter 23 Aristotle seems to argue, not that *every* syllogism is deducible in *D2*, but rather that *any syllogism deducible at all* is deducible in *D2*. And, as indicated in his final sentence, he does not believe he has completed his argument. He reasons as follows. In the first place he asserts without proof that any syllogism deducible by means of syllogisms in the three figures is deducible in *D2* (but here he is overlooking the problem of iterated *reductio* mentioned in Section 4.3 above). In any case, granting him that hypothesis, he then argues that any syllogism deducible at all is deducible by means of the syllogisms in the three figures, thus: Every deduction is either direct or hypothetical – the latter including both indirect deductions and those involving *ecthesis* (see above). He considers the direct case first. Here he argues that every direct deduction must have at least two premises as in the three figures and that in the two-premise case the conclusion has already been proved. Then he simply asserts that it is "the same if several middle terms should be necessary" (41a18). In considering the hypothetical deductions he takes up indirect deductions first and observes that after the contradictory of the conclusion is also assumed one proceeds as in the direct case – concluding that the reduction to *D2* is evident in this case also (41a35ff). Finally, he simply asserts that it is the same with the other hypothetical deductions. But this he has immediate misgivings about (41b1). He leaves the proof unfinished to the extent that the non-indirect hypothetical deductions have not been completely dealt with.

6. Conclusion

As a kind of summary of our research we present a review of what we take to be the fundamental achievements of Aristotle's logical theory. In the first place, he clearly distinguished the role of deduction from the role of experience (or intuition) in the development of scienctific theories. This is revealed by his distinction between the axioms of a science and the logical apparatus used in deducing the theorems. Today this would imply a distinction between logical and nonlogical axioms; but Aristotle had no idea of logical axioms (but cf. 77a22–25). Indeed, he gave no systematic discussion of logical truth (*Axx* is not even mentioned once). In the second place, Aristotle developed a natural deduction system which he exemplified and discussed at great length. Moreover, he formulated fairly intricate metamathematical results relating his central system to a simpler one. It is also important to notice that Aristotle's system is sound and strongly complete. In the third place, Aristotle was clear enough about logical consequence so that he was able to discover the method of counter instances for establishing invalidity. This method is the cornerstone of all independence (or invalidity) results, though it probably had to be rediscovered in modern times cf. Cohen and Hersh). In the fourth place, his distinction between perfect and imperfect syllogisms suggests a clear understanding of the difference between deducibility and implication – a distinction which modern logicians believe to be their own (cf. Church, p. 323, fn. 529). In the fifth place, Aristotle used principles concerning form repeatedly and accurately, although it is not possible to establish that he was able to state them nor is even clear that he was consciously aware of them as logical principles.

The above are all highly theoretical points – but Aristotle did not merely theorize; he carried out his ideas and programs in amazing detail despite the handicap of inadequate notation. In the course of pursuing details Aristotle originated many important discoveries and devices. He described indirect proof. He used syntactical variables (alpha, beta, etc.) to stand for content words – a device whose importance in modern logic has not been underestimated. He formulated several rules of inference and discussed their interrelations.

Philosophers sometimes say that Aristotle is the best introduction to philosophy. This is perhaps an exaggeration. One of the Polish logicians once said that the *Analytics* is the best introduction to logic. My own reaction to this remark was unambiguously negative – the severe difficulties in reading the *Analytics* form one obstacle and I felt then that the meager results did not warrant so much study. After carrying out the above research I can compromise to the following extent. I now believe that Aristotle's logic is rich enough, detailed enough, and sufficiently representative of modern logics that a useful set of introductory lectures on mathematical logic could be organized around what I have called the main Aristotelian system.

From a modern point of view, there is only one mistake which can sensibly be charged to Aristotle: his theory of propositional forms is very seriously inadequate. It is remarkable that he did not come to discover this for himself, especially since he mentions specific proofs from arithmetic and geometry. If he had tried to reduce these to his system he may have seen the problem (cf. Mueller, pp. 174–177). But, once the theory of propositional forms is taken for granted, there are no important inadequacies attributable to Aristotle, given the historical context. Indeed, his work is comparable in completeness and accuracy to that of Boole and seems incomparably more comprehensive than the Stoic or medieval efforts. It is tempting to speculate that it was the oversimplified theory of propositional forms that made possible the otherwise comprehensive system. A more adequate theory of propositional forms would have required a much more complicated theory of deduction – indeed, one which was not developed until the present era.

Acknowledgments

Previous versions of sections of this paper have been presented at various places including: Buffalo Logic Colloquium (March 1971), Pennsylvania Logic Colloquium (April 1971), Hammond Society (Johns Hopkins, April 1971), Laval University (June 1971), University of Montreal (October, 1971), SUNY/Albany (October, 1971), Association for Symbolic Logic (December, 1971), University of Quebec (March, 1972), Cambridge University (June, 1972). Several persons at these talks, or afterward, made useful suggestions, especially Charles Kahn, John Glanville, Lynn Rose, and John Kearns. Throughout the Winter and Spring semesters of 1971–72, I consulted extensively with John Mulhern and Mary Mulhern. My research assistants, Keith Ickes, David Levin, John Herring and Terry Nutter, deserve thanks for conscientious editing and proof-reading of many drafts.

Other papers of mine complementing and overlapping this one appear in *Journal of Symbolic Logic, Mind* and *Archiv für Geschichte der Philosophie*. The first of these three contains the completeness proof. The second treats the question of whether syllogisms are arguments or conditional sentences (as Łukasiewicz holds). The third paper is the result of deleting parts from a paper to which additions were made in formulating the present article.

In February 1972, Smiley's article (*q.v.*) which is in remarkable agreement with the above was brought to my attention. It should be noted that Smiley has considered some questions which have not been treated here. Many ideas expressed in this paper have been colored by consultation with Smiley and by Smiley's article. In particular, my estimate of the value of the Łukasiewicz work has been revised downward as a result of discussion with Smiley.

State University of New York at Buffalo

NOTES

[1] It should be realized that the notion of a 'model' used here is the ordinary one used in discussion of, e.g., wooden models of airplanes, plastic models of boats, etc. Here the adjective 'mathematical' indicates the kind of material employed in the model. I.e., here we are talking about models 'constructed from' mathematical objects. Familiar

mathematical objects are numbers, (mathematical) points, lines, planes, (syntactic) characters, sets, functions, etc. Here we *need* as basic elements only syntactic characters, but the development below also presupposes sets *ab initio*. It should also be realized that a mathematical model is *not* a distinctive sort of mathematical entity – it is simply a mathematical entity conceived of as analogous to something else.

[In order to avoid excessive notes bracketed expressions are used to refer by author (and/or by abbreviated title) and location to items in the list of references at the end of this article. Unless otherwise stated, translations are taken from the Oxford translation (see 'Aristotle').]

[2] These ideas are scattered throughout Church's introductory chapter, but in Schoenfield (*q.v.*) Sections 2.4, 2.5 and 2.6 treat, respectively, languages, semantic systems and deductive systems.

[3] From the best evidence of the respective dates of the *Analytics* (Ross, p. 23) and Euclid's *Elements* (Heath, pp. 1, 2), one can infer that the former was written in the neighborhood of fifty years before the latter. The lives of the two authors probably overlapped; Aristotle is known to have been teaching in Athens from 334 until 322 (Edel, pp. 40, 41) and it is probable both that Euclid received his mathematical training from Aristotle's contemporaries and that he flourished c. 300 (Heath, p. 2). In any case, from internal evidence Ross (p. 56) has inferred that Euclid was probably influenced by the *Analytics*. Indeed, some scholarship on the *Elements* makes important use of Aristotle's theory of the axiomatic organization of science (cf. Heath, pp. 117–124). However, it should be admitted that Hilbert's geometry (*q. v.*) is much more in accord with Aristotle's principles than is Euclid's. For example, Hilbert leaves some terms 'undefined' and he states his universe of discourse at the outset, whereas Euclid fails on both of these points, which were already clear Aristotelian requirements.

[4] Aristotle may have included deductive arguments which would be sound were certain intermediate steps added; cf. Section 5.1 below.

[5] This will account somewhat for the otherwise inexplicable fact already noted by Łukasiewicz (p. 49) and others that there are few passages in the Aristotelian corpus which could be construed as indicating an awareness of propositional logic.

[6] In a doubly remarkable passage (p. 13) Łukasiewicz claims that Aristotle did not reveal the object of his logical theory. It is not difficult to see that Łukasiewicz is correct in saying that Aristotle nowhere admits to the purpose which Łukasiewicz imputes to him. However, other scholars have had no difficulty in discovering passages which do reveal Aristotle's true purpose (cf. Ross, pp. 2, 24, 288; Kneale and Kneale, p. 24).

[7] This point has already been made by Kneale and Kneale (pp. 80–81), who point out further difficulties with Łukasiewicz's interpretation. For yet further sensitive criticism see Austin's review and also Iverson, pp. 35–36.

[8] Although we have no interest in giving an account of how Łukasiewicz may have arrived at his view, it may be of interest to some readers to note the possibility that Łukasiewicz was guided in his research by certain attitudes and preferences not shared by Aristotle. The Łukasiewicz book seems to indicate the following: (1) Łukasiewicz preferred to consider logic as concerned more with truth than with either logical consequence or deduction (e.g., pp. 20, 81). (2) He understands 'inference' in such a way that correctness of inference depends on starting with true premises (e.g., p. 55). (3) He feels that propositional logic is somehow objectively more fundamental than quantificational or syllogistic logic (e.g., pp. 47, 79). (4) He tends to concentrate his attention on axiomatic deductive systems to the neglect of natural systems. (5) He

tends to underemphasize the differences between axiomatic deductive systems and axiomatic sciences. (6) He places the theory of the syllogism on a par with a certain branch of pure mathematics (pp. 14, 15, 73) and he believes that logic has no special relation to thought (pp. 14, 15). Indeed, he seems to fear that talk of logic as a study of reasoning necessarily involves some sort of psychologistic view of logic. (7) He believes that content words or non-logical constants cannot be introduced into logic (pp. 72, 96). The Łukasiewicz attitudes are shared by several other logicians, notably, in this context, by Bocheński (*q.v.*). It may not be possible to argue in an objective way that the above attitudes are incorrect but one can say with certainty that they were not shared by Aristotle.

[9] Exclusion of proper names, relatives, adjectives and indefinite propositions is based more on a reading of the second logic as a whole than on specific passages (but cf. 43a25–40). M. Mulhern, in substantial agreement with this view, has shown my previous attempts to base it on specific passages to be inconclusive as a result of reliance on faulty translation. Her criticisms together with related ones by Charles Kahn (University of Pennsylvania) and Dale Gottlieb (Johns Hopkins) have led to the present version of the last two paragraphs.

[10] Rose (p. 39) has criticized the Łukasiewicz view that no syllogisms with content words are found in the Aristotelian corpus. Our view goes further in holding that *all* Aristotelian syllogisms have content words, i.e., that Aristotle nowhere refers to argument forms or propositional functions. All apparent exceptions are best understood as metalinguistic reference to 'concrete syllogisms'. This view is in substantial agreement with the view implied by Rose at least in one place (p. 25).

[11] In many of the locations cited above Aristotle seems remarkably close to a recognition of 'category mistakes' – a view that nonsense of some sort results from mixing terms from different sciences in the same proposition (e.g., 'the sum of two triangles is a prime number').

[12] It must be recognized that other interpretations are possible – cf. Kneale and Kneale, pp. 55–67. However, in several places (e.g., 85a31–32) Aristotle seems to imply that a secondary substance is nothing but its extension.

[13] This would explain the so-called existential import of *A* and *N* sentences. Notice that, according to this view, existential import is a result of the semantics of the *terms* and has no connection whatever with the meaning of 'All'. In particular, the traditional concern with the meaning of 'All' was misplaced – the issue is properly one of the meaning of categorical terms. As far as we have been able to determine this is the first clear theoretical account of existential import based on textual material.

[14] Jaskowski (*loc. cit.*) gives no textual grounds. There are, however, some passages (e.g., 998b22) which imply that the class of all existent individuals is not a genus. In subsequent developments of 'Aristotelian logic' which include 'negative terms', exclusion of the universe must be maintained to save exclusion of the null set.

[15] This is the mathematical analogue of the classical notion of logical consequence which is clearly presupposed in traditional work on so-called 'postulate theory'. It is important to notice that we have offered only a mathematical analogue of the concept and not a definition of the concept itself. The basic idea is this: Each interpretation represents a 'possible world'. To say that it is logically impossible for the premises to be true and the conclusion false is to say that there is no possible world in which the premises actually are true and the conclusion actually is false. The analogue, therefore, is that no true interpretation of the premises makes the conclusion false. Church (p. 325) attributes this mathematical analogue of logical consequence to Tarski

(pp. 409–420), but Tarski's notion of true interpretation (model) seems too narrow (at best too vague) in that no mention of alternative universes of discourse is made or implied. In fact the limited Tarskian notion seems to have been already known even before 1932 by Lewis and Langford (p. 342), to whom, incidentally, I am indebted for the terms 'interpretation' and 'true interpretation' which seem heuristically superior to the Tarskian terms 'sequence' and 'model', the latter of which has engendered category mistakes – a 'model of set of sentences' in the Tarskian sense is by no means a model, in any ordinary sense, of a set of sentences.

[16] The method of 'contrasting instances' is a fundamental discovery in logic which may not yet be fully appreciated in its historical context. Because Łukasiewicz (p. 71) misconstrued the Aristotelian framework, he said that modern logic does not employ this method. It is obvious, however, that all modern independence (invalidity) results from Hilbert (pp. 30–36) to Cohen (see Cohen and Hersh) are based on developments of this method. Indeed, there were essentially no systematic investigations of questions of invalidity from the time of Aristotle until Beltrami's famous demonstration of the invalidity of the argument whose premises are the axioms of geometry less the Parallel Postulate and whose conclusion is the Parallel Postulate itself (Heath, p. 219). Although there is not a single invalidity result in the *Port Royal Logic* or in Boole's work, for example, modern logic is almost characterizable by its wealth of such results – all harking back to Aristotle's method of contrasting instances.

[17] The Principle of Form is generally accepted in current logic (cf. Church, p. 55). Recognition of its general acceptance is sometimes obscured by two kinds of apparent challenges – each correct in its own way but not to the point at issue. (1) Ryle wants to say (e.g.) that 'All animals are brown' implies 'All horses are brown' and, so, that implication is not a matter of form alone (Ryle, pp. 115–116). It is easy to regard the objection as verbal because, obviously, Ryle is understanding an argument to be 'valid' if addition of certain truths as premises will produce an argument valid in the above sense. (2) Oliver makes a more subtle point (p. 463). He attacks a variant of the Principle of Form by producing examples of the following sort.

If Axy then Nxy	If Sxy then Axy
Nxy	Axy
$?Axy$	$?Sxy$

According to Oliver's usage these two arguments are in the same form and yet the one on the left is obviously invalid (suppose x indicates 'men' and y 'horses') while the one on the right is obviously valid (in fact the conclusion follows immediately from the second premise). The resolution is that Oliver's notion of 'being in same form' is not the traditional one; rather it is a different but equally useful notion. Oliver takes two arguments to be in the same form if there is a scheme which subsumes both. Since both are subsumed under the scheme '(if P then Q, Q/P)' they are in the same form. It so happens that the scheme is not a valid scheme; it subsumes both valid and invalid arguments. He does allow the correctness of the above principle as stated (Oliver, p. 465).

[18] Rose (p. 39) emphasizes the fact that Aristotle would establish the invalidity of several arguments at once by judicious choice of interrelated counter interpretations.

[19] A logical question concerning the validity of an argument is settled by using presupposed procedures to deduce the conclusion from the premises. A metalogical question concerns the validity of the presupposed procedures and is usually 'answered' in terms of a theory of meaning (or a semantic system).

[20] One is impressed with the sheer number of times that Aristotle alludes to the fact that there are but two methods of perfecting syllogisms – and this makes it all the more remarkable that an apparent third method occurs, the so-called method of *ecthesis*. There are two ways of explaining the discrepancy. In the first place, *ecthesis* is not a method of proof on a par with the direct and indirect methods; rather it consists in a class of rules of inference on a par with the class of conversion rules and the class of perfect syllogism rules (see below). In the second place, and more importantly, *ecthesis* is clearly extrasystematic relative to Aristotle's logical system (or systems). It is only used three times (Łukasiewicz, p. 59), once in a clearly metalogical passage (25a17) and twice redundantly (28a23, 28b14).

[21] Specifically, for example with regard to the first conversion rule (C1), define the set-theoretic relation [RC1] on L such that for all s and s' in L, s[RC1]s' iff for some x and y in U, $s=Nxy$ and $s'=Nyx$. Thus the *rule* [RC1] is, in effect, the set of all 'its applications'. Generally speaking, an n-placed rule of inference is an $n+1$ – placed relation on sentences. But, of course, not necessarily vice versa (cf. Corcoran, 'Theories', pp. 171–175).

[22] Quine has conveniently listed all such arguments in pp. 76–79 of his *Methods of Logic*. Incidentally, the reader should regard the notion of 'valid argument' in principle 4.2 as convenient parlance for referring to Quine's list – so that no semantic notions have been used in this section in any essential way.

[23] There seems to be a vague feeling in some current circles that an argument with inconsistent premises should not be regarded as an argument at all and that an 'authentic' deduction cannot begin with an inconsistent premise set. However, the only way of determining that a premise set is inconsistent is by deducing contradictory conclusions from it. Thus it would seem that those who wish to withhold 'authenticity' from deductions with inconsistent premise sets must accept the 'authenticity' of those very deductions in order to ascertain their 'non-authenticity'. One must admit, however, that the issue does seem to involve convention (*nomos*) more than nature (*physis*). On the other hand, how does one determine the natural joints of the fowl except by noting where the neatest cuts are made? (cf. *Phaedrus*, 265e).

[24] For an interesting solution to 'the mystery of the fourth figure' (the problem of explaining why Aristotle seemed to stop at the third figure) see Rose, *Aristotle's Syllogistic*, pp. 57–79.

[25] It is in the interest of accuracy that we reluctantly admit that Aristotle also *seems* to claim the converse. It is germane also to observe that, although the above claim is substantiated not only by examples but also by a general formula, the converse is false.

It is also relevant to point out that the existence of this metaproof provides a negative answer to a question raised by William Parry concerning the nature of indirect deductions in Aristotle. Parry wondered whether Aristotle required that the contradiction explicitly involve one of the premises. An affirmative answer would rule out abnormal indirect deductions which, as indicated above, form the basis of Aristotle's metaproof.

[26] For example, the whole revised system $D3$ can be obtained from the system of Corcoran and Weaver (p. 373) by the following changes in the latter. (1) Change the language to L. (2) Replace negations by contradictions. (3) Replace the rules of conditionals and modal operators by the conversion and syllogism rules.

[27] As an indication that Aristotle's clarity concerning *reductio* is significant one may note with Iverson (p. 36) that Łukasiewicz (p. 55) misunderstood indirect proof.

[28] The consideration of extended deductions emerged from a suggestion by Howard Wasserman (Linguistics Department, University of Pennsylvania).

[29] Of course one should not overlook the historical importance of II (the logic having components *L*, *S* and *D*2) nor should the possible importance of *IE* (the logic having components *L*, *S* and *DE*) be minimized. In this connection we have been asked whether there are deductive systems other than *D*, *DE*, *D*2 and *D*3 implicit in the second logic. This question is confidently answered negatively, even though Patzig (p. 47) alleges to have found other systems in *Prior Analytics* I, 45. It is clear that this chapter merely investigates certain interrelationships among the three figures without raising any issues concerning alternative deductive systems. Although Aristotle speaks of 'reducing' first figure syllogisms to the other figures there is no mention of 'perfecting' first figure syllogisms (or any others for that matter) by means of syllogisms in the other figures. Indeed, because of Aristotle's belief that syllogisms can be *perfected* only through the first figure, one should not expect to find any deductive systems besides those based on first figure syllogistic rules. In addition, one may note that Bocheński (p. 79) alleges to have found other deductive systems outside of the second logic in *Prior Analytics* II, 10. But this chapter is the last of a group of three which together are largely repetitious of the material in *Prior Analytics* I, 45 which we just discussed.

[30] See Corcoran, 'Completeness' and/or 'Natural Deduction'.

[31] Mates (*Stoic Logic*, pp. 4, 81, 82, 111, 112) has argued that the Stoics believed their deductive system to be complete. But had the Aristotelian passage (from 40b23 up to but not including 41b1) been lost Mates would have equivalent grounds for saying that Aristotle believed his system complete. There are no grounds for thinking that the problem was raised in either case.

[32] Unfortunately, the Łukasiewicz formulation makes it possible to confuse these problems with the so-called decision problems. The two types of problems are distinct but interrelated to the extent that decidable logics are generally (but not necessarily) complete. It is hardly necessary to mention the fact that ordinary first order predicate logic is complete but not decidable (Jeffrey, pp. 195ff; Kneale and Kneale, pp. 733–734).

BIBLIOGRAPHY

Anderson, J. and Johnstone, H., *Natural Deduction*, Belmont, Calif. 1963.

Aristotle, *The Works of Aristotle Translated into English* (ed. by W. D. Ross), Vol. 1, Oxford 1928.

Austin, J. L., 'Review of Łukasiewicz's *Aristotle's Syllogistic*', *Mind* **61** (1952), 395–404.

Barnes, J., 'Aristotle's Theory of Demonstration', *Phronesis* **14** (1969), 123–152.

Bocheński, I. M., *History of Formal Logic*, (transl. by I. Thomas), Notre Dame, Indiana, 1961.

Church, A., *Introduction to Mathematical Logic*, Princeton 1956.

Cohen, P. J. and Hersh, R., 'Non-Cantorian Set Theory', *Scientific American*, December 1967, pp. 104–116.

Corcoran, J., 'Three Logical Theories', *Philosophy of Science* **36** (1969), 153–177.

Corcoran, J., 'Review of Quine's *Philosophy of Logic*', *Philosophy of Science* **39** (1972), 88–90.

Corcoran, J., 'Conceptual Structure of Classical Logic', *Philosophy and Phenomenological Research* **33** (1972), 25–47.

Corcoran, J., 'Aristotle's Natural Deduction System', presentation at December 1971 meeting of Association for Symbolic Logic, abstract in *Journal of Symbolic Logic* **37** (1972), 437.

Corcoran, J., 'Completeness of an Ancient Logic', presented at Laval University, June 1971 and at the Buffalo Logic Colloquium, September 1971; *Journal of Symbolic Logic* **37** (1972), 696–702.

Corcoran, J., 'Aristotelian Syllogisms: Valid Arguments or True Generalized Conditionals', forthcoming in *Mind.*

Corcoran, J., 'A Mathematical Model of Aristotle's Syllogistic', *Archiv für Geschichte der Philosophie* **55** (1973), 191–219.

Corcoran, J., and Weaver, G., 'Logical Consequence in Modal Logic', *Notre Dame Journal of Formal Logic* **19** (1969), 370–384.

Edel, A., *Aristotle*, New York 1967.

Frege, G., 'On the Foundations of Geometry', *Jahresbericht* **15** (1906); reprinted in Kluge (ed. and transl.), *Gottlob Frege On the Foundations of Geometry* etc., New Haven and London 1971.

Heath, T., *Euclid's Elements*, Vol. 1 (2nd ed.), New York 1956.

Hilbert, D., *Foundations of Geometry*, (transl. by E. J. Townsend), LaSalle, Illinois, 1965.

Iverson, S. L., *Reduction of the Aristotelian Syllogism*, M. A. Thesis in Philosophy, State University of New York at Buffalo, May, 1964.

Jaskowski, St., 'On the Interpretations of Aristotelian Categorical Propositions in the Predicate Calculus', *Studia Logica* **24** (1969), 161–174.

Jeffrey, R., *Formal Logic: Its Scope and Limits*, New York 1967.

Kneale, W. and Kneale, M., *The Development of Logic*, Oxford 1962.

Kreisel, G., 'Mathematical Logic: What Has it Done for the Philosophy of Mathematics?', in *Bertrand Russell Philosopher of the Century* (ed. by R. Schoenman), London 1967.

Lewis, C. I. and Langford, C. H., *Symbolic Logic*, New York 1959.

Łukasiewicz, J., *Aristotle's Syllogistic* (2nd. ed.), Oxford 1957.

Mates, B., *Stoic Logic*, Berkeley and Los Angeles 1961.

Mates, B., *Elementary Logic*, New York 1965.

Mueller, Ian, 'Stoic and Peripatetic Logic', *Archiv für Geschichte der Philosophie* **51** (1969), 173–187.

Mulhern, J. J., *Problems of the Theory of Predication in Plato's Parmenides, Theatetus, and Sophista*, Ph. D. dissertation in Philosophy, State University of New York at Buffalo, February, 1970.

Mulhern, M. M., *Aristotle's Theory of Predication: The Categoriae Account*, Ph. D. Dissertation in Philosophy, State University of New York at Buffalo, September, 1970.

Oliver, J. W., 'Formal Fallacies and Other Invalid Arguments', *Mind* **76** (1967), 463–78.

Patzig, G., *Aristotle's Theory of the Syllogism* (transl. by J. Barnes), Dordrecht 1968.

Quine, W., *Methods of Logic* (revised ed.), New York 1959.

Quine, W., *Philosophy of Logic*, Englewood Cliffs, N. J., 1970.

Rose, L., *Aristotle's Syllogistic*, Springfield Illinois, 1968.

Ross, W. D., *Aristotle's Prior and Posterior Analytics*, Oxford 1965.

Ryle, G., *Dilemmas* (paperback ed.), London 1960.

Schoenfield, J., *Mathematical Logic*, Reading, Mass., 1967.

Scholz, H., *Concise History of Logic* (transl. by K. Leidecker), New York 1961.

Smiley, T., 'What is a Syllogism?', *Journal of Philosophical Logic* **2** (1973), 136–154.
Tarski, A., *Logic, Semantics and Metamathematics* (transl. by J. Woodger), Oxford 1956.
Tredennick, H., 'Introduction', in *Aristotle, The Organon*, Vol. 1, Cambridge, Mass. 1949, pp. 182–195.
van Heijenoort, J., *From Frege to Godel*, Cambridge, Mass. 1967.
Weaver, G. (see Corcoran and Weaver).

MARY MULHERN

CORCORAN ON ARISTOTLE'S LOGICAL THEORY

Jan Łukasiewicz, by his own account, entered the lists in 1923 as an interpreter of ancient logic from the standpoint of modern formal logic. In that year he began defending his view of the contrast of Stoic logic with Aristotelian logic; this view appeared in print for the first time in 1930.[1] This was followed by the Polish version in 1934, and the German in 1935, of his landmark paper, 'On the History of the Logic of Propositions' [9]. During the same period Łukasiewicz was lecturing on Aristotle's syllogistic. An authorized version of his lectures on this and other logical topics was published by students at the University of Warsaw in 1929, republished in Warsaw in 1958, and finally translated into English in 1963 under the title *Elements of Mathematical Logic* [7]. Łukasiewicz elaborated his researches until he issued in 1951 his now famous monograph *Aristotle's Syllogistic from the Standpoint of Modern Formal Logic.* A second edition, enlarged but not revised, appeared in 1957, its author's death having occurred in the previous year [6].

Łukasiewicz thus has held the field for nearly half a century. Questions have been raised about some details of his interpretation, and corrections have been made of some of his mistakes in matters of fact, but, so far as I know, no one had brought a direct challenge against the main lines of Łukasiewicz's interpretation of Aristotle's syllogistic and its place in ancient logic until John Corcoran did so in 'A Mathematical Model of Aristotle's Syllogistic' [3]. Indeed, so spectacular a *tour de force* was Łukasiewicz's book that, despite his own protestations that he was setting out the system merely "in close connexion with the ideas set forth by Aristotle himself" ([6], p. 77) and "on the lines laid down by Aristotle himself" ([6], p. viii), his account has gained wide acceptance as the definitive presentation of Aristotle's syllogistic, and some writers lead one to believe that Aristotle's system is no more and no less than what Łukasiewicz proposes.

Łukasiewicz's view, very briefly put, is this: The logic of Aristotle is a theory of the relations *A*, *E*, *I*, and *O* (in their mediaeval senses) in the

J. Corcoran (ed.), Ancient Logic and Its Modern Interpretations, 133–148. *All Rights Reserved*

field of universal terms ([6], p. 14). It is a theory of special relations, like a mathematical theory ([6], p. 15). As a logic of terms, it presupposes a more fundamental logic of propositions, which, however, was unknown to Aristotle and was discovered by the Stoics in the century after him ([6], p. 49). Aristotle's theory is an axiomatized deductive system, in which the reduction of the other syllogistic moods to those of the first figure is to be understood as the proof of these moods as theorems by means of the axioms of the system ([6], p. 44).

Corcoran has proposed, on the other hand, that Aristotle's syllogistic is not an axiomatic science but rather a natural deduction system, and that the theory is itself fundamental, presupposing neither the logic of propositions nor any other underlying logic.

Corcoran's proposals have a good deal to recommend them. First, Corcoran provides a faithful reconstruction of Aristotle's method. Although Łukasiewicz gives a system that does arrive at Aristotle's results, obtaining and rejecting laws corresponding to the moods which Aristotle obtains and rejects, his derivations, by substitution and detachment from axioms, have nothing in common with Aristotle's own method. Indeed, Łukasiewicz must say that Aristotle's proposals about method are wrong, and that Aristotle did not and could not use the technique of perfecting syllogisms, which Aristotle claims over and over again that he is using.[2] Corcoran, on the other hand, not only makes perfect sense of the doctrine of perfecting syllogisms, but he is willing to take Aristotle at his word instead of being content to elaborate a system allegedly in close connexion with Aristotle's ideas. The upshot is that Corcoran succeeds, as Łukasiewicz did, in reproducing Aristotle's results, and he succeeds, as Łukasiewicz did not, in reproducing Aristotle's method step by step, so that the annotated deductions of his system D are faithful translations of Aristotle's exposition. Corcoran's concern for method is prompted by his belief that Aristotle shared this concern. I think there can be no doubt that he is correct. Aristotle sets out his method in detail which if concise is yet minute, and when, at the beginning of Chapter XXX of the first book of the *Priora* (46a4), he summarizes his work so far, he speaks not of the same *results* in philosophy and every kind of art and study whatsoever, but of the same *method* (ὁδός) in all these branches of inquiry.

Corcoran's interpretation also has the virtue of making sense of Aristotle's views concerning the place of syllogistic in his doctrine as a whole. While Łukasiewicz apparently held that syllogistic was a science which must take its place beside the other sciences in the Aristotelian scheme, Corcoran proposes to take syllogistic as the underlying logic of the demonstrative sciences. Łukasiewicz held further that syllogistic itself presupposes propositional logic as an underlying logic – of which Aristotle, however, was ignorant. Corcoran, by contrast, suggests that syllogistic is a fundamental logical system, presupposing no other.

This circumstance, rather than Aristotle's ignorance, Corcoran observes, accounts for their being few passages in the *corpus* which can be construed as references to propositional logic. But these passages are not so few nor so insignificant as Łukasiewicz and some other writers would have us believe. They include, for instance, the use of propositional variables (documented by Bocheński ([2], pp. 77, 97–98) at *An. Pr.* 41b36–42a5; 53b12 *sqq*; 34a5 *sqq*; and by Ross ([16], *ad loc.*) at *An. Post.* 72b32–73a6), the use and even the explicit statement of laws of propositional logic (documented by Bocheński ([2], p. 98) at *An. Pr.* 53b7–10; 57a36 *sqq*; and by Łukasiewicz ([6], pp. 49–50) at *An. Pr.* 57b1 [transposition]; 57b6 [hypothetical syllogism]; 57b3 [both laws]), and the use of, or the discussion of the use of, propositional units of argumentation (among others, *De Int.* 17a20–24; *An. Pr.* 48a29, 38–39; *Soph. E4.* 169a12–15; 181a22–30). It should be remembered, too, that at the beginning of the *Analytica Priora* Aristotle starts with premisses and resolves them into terms; he does not start with terms and build them up into premisses.

The evidence points rather to Aristotle's awareness of propositional logic but his rejection of it as an instrument unfit for the purposes he intended. Aristotle, I propose, knew enough about propositional logic to have recognized it as the underlying logic of syllogistic and of all the other sciences on a par with syllogistic if it really played this role.[3] We should then expect to find throughout the *Analytica* references to propositional logic as the underlying logic of syllogistic and of each of the demonstrative sciences. But we do not find them. What we do find, as Corcoran points out, is every indication both that Aristotle regarded syllogistic as a fundamental logic and that he considered it to be the underlying logic of the demonstrative sciences. My suggestion is, then, that Aristotle could have elaborated a system of propositional logic, but that the theory of demonstrative science which he envisioned required a system of analyzed propositions, in which the modality of predications could be clearly shown. Thus he rejected a logic of unanalyzed propositions in favor of syllogistic.

Corcoran, it seems to me, has made a very important contribution to our understanding of Aristotle's logic, and the suggestions offered in what follows should not be construed as impugning in any substantive way the value of that contribution.

Of the many points Corcoran raises, I intend to take up four: (1) whether syllogistic is a science; (2) whether the theory of propositional forms presupposed by syllogistic is adequate; (3) whether Aristotle had a doctrine of logical truth; and (4) whether Aristotle considered reasoning natural or conventional.

The first question needs to be divided. Corcoran notes that a theory of deduction is to be distinguished from an axiomatic science and further that theories of deduction have been distinguished as 'natural' or 'axiomatic'. He seeks to refute what he regards as Łukasiewicz's claim that syllogistic is an axiomatic science, and, moreover, a science in Aristotle's terms. However, there seems to be some ambivalence both in the claim and in the refutation. Łukasiewicz, if pressed, would probably not have insisted that syllogistic is a science in Aristotle's terms, since he was well aware of the quarrel between the Stoics and the Peripatetics about the relation of logic to philosophy. In this connection he quotes Ammonius to the effect that the Peripatetics following Aristotle treated logic as an instrument of philosophy, opposing the Stoics who treated it as part of philosophy ([6], p. 13). But Corcoran is correct in that Łukasiewicz's work as it stands leaves itself open to his charge: in contending that syllogistic is a science like a mathematical theory, Łukasiewicz has led us to believe that it would occupy a place beside physics, mathematics, astronomy, and theology in the Aristotelian scheme.[4]

Corcoran argues convincingly, I think, both that syllogistic is not a science in Aristotle's sense (because it has no genus) and that it is not an axiomatic science in any sense (because either it would be its own underlying logic, which is impossible, or it would presuppose an underlying logic, which is false). Having established that syllogistic is a theory of deduction and not an axiomatic science, Corcoran goes on to refute Łukasiewicz's more serious claim, that syllogistic is an axiomatic deductive system, by showing, again convincingly, that syllogistic is a natural deduction system.

But I wonder how hard and fast Corcoran himself draws the lines which he accuses Łukasiewicz of overstepping. Corcoran titles his study 'A Mathematical Model of Aristotle's Syllogistic', and in an earlier version he spoke of

> mathematical logic as a branch of applied mathematics which constructs and studies mathematical models in order to gain understanding of logical phenomena. From this standpoint mathematical logics are comparable to the mathematical models of solar systems, vibrating strings, or atoms in mathematical physics and to the mathematical models of computers in automata theory.

Thus it appears that, even if syllogistic itself is not a scientific exercise, at least Corcoran's reconstruction of it is a scientific exercise.

Furthermore, in 'Three Logical Theories', the comprehensive study laying the ground for his present work, Corcoran describes what he does therein as "a contribution to the philosophy of the science of logic" in the course of which he will "apply a certain methodological principle to logical systems *considered as theories* [Corcoran's italics]" ([4], p. 153). Hence, if Łukasiewicz, in comparing Aristotle's syllogistic to a part of the science of mathematics, merely intends that we should consider syllogistic as a theory, perhaps he is not so far wrong, even by Corcoran's standards.

On the other hand, suppose we take the apparently stricter criterion proposed by Corcoran in his study of syllogistic – that a theory of deduction deals metalinguistically with reasoning (it says how to perform constructions involving object language sentences), while a science deals with a domain of objects, insofar as certain properties and relations are involved, and states its axioms in an object language whose non-logical constants are interpreted as indicating the required properties and relations and whose variables are interpreted as referring to objects in the universe of discourse. I am still not sure that Łukasiewicz can be pinned.

Łukasiewicz calls the logic of Aristotle "a theory of the relations *A*, *E*, *I*, and *O* in the field of universal terms" ([6], p. 14). Note that Łukasiewicz says 'universal terms', not 'secondary substances', as Corcoran would have it. Further, Łukasiewicz states that the 'term-variables' of his formalization of Aristotle's system "have as values universal terms, as 'man' or 'animal'" ([6], p. 77). Here Łukasiewicz not only reiterates his provision that Aristotle's theory concerns not objects but expressions, he also uses the convention of single quotes to indicate, by mentioning and not using the values of his variables, that these values are not objects but expressions. So Łukasiewicz's reconstruction comes out as theory of object language sentences, not as a theory of objects. Łukasiewicz will not be pushed as far as Corcoran wants to push him, but Corcoran is undoubtedly right in challenging what appears to be Łukasiewicz's identification of syllogistic with axiomatized science. Łukasiewicz did only say that syllogistic was "like a mathematical theory" ([6], p. 13) and "similar to a mathematical theory" ([6], p. 73), but he failed to attach such riders to his claim as would have rendered that claim consistent with the details of his reconstruction as he actually performed it.

The second point I wish to take up is the charge Corcoran lays against Aristotle that the theory of propositional forms presupposed in syllogistic is "very seriously inadequate" and "oversimplified". Corcoran gives me to understand that by this charge he means especially that Aristotle's theory of propositional forms is inadequate to the expression of the axioms of science in his own day. He further invites my attention to three questions which he thinks ought to be distinguished: (i) is the theory of propositional forms presupposed in the second logic the entire Aristotelian theory (Corcoran answers his own question in the negative and says Aristotle would have admitted as much); (ii) is the theory of propositional forms of the second logic adequate for, say, geometry (answer: no, again Aristotle would have admitted this); and (iii) is Aristotle's whole theory of propositional forms (as found in the *Categoriae, De Interpretatione*, etc.) adequate for geometry (a much harder question, says Corcoran).

Now, not being a geometrician or even a historian of geometry, I shall not attempt to answer the question whether Aristotle's theory of propositional forms is adequate for geometry. What I shall try to point out is that the theory of propositional forms presupposed by analytical syllogistic is not so simple as Corcoran suggests. By 'analytical syllogistic', I mean the deductive system set out in the *Analytica Priora*; this is a part of syllogistic in general, which also includes non-analytical syllogistic, or dialectical syllogistic, as it is sometimes called, set out in the *Topica* and elsewhere.[5] I further distinguish, within analytical syllogistic, non-demonstrative syllogistic and demonstrative syllogistic.

It is true that the theory of propositional forms in use in demonstrative syllogistic is severely limited, and limited for cogent reasons connected with its intended interpretation, but there is no reason to suppose that the theory in use in Aristotle's exposition of analytical syllogistic is any less complex than the theory presented in the *Categoriae, De Interpretatione, Topica*, and elsewhere in the *corpus*. Corcoran takes the view that Aristotle's syllogistic 'master language' is made up of the logical constants *A*, *N*, *S*, $ (Corcoran's updated *A*, *E*, *I*, *O*) and the set *U* of non-logical constants or content words. Corcoran had formerly held, in an apparent effort to assimilate Aristotle's work to that of contemporary logicians, that the set *U* comprised infinitely many characters representing infinitely many secondary substances or universals. Corcoran held further, however – regarding what he saw as a contrast with contemporary comprehensive theories of deduction – that the only content words appearing in syllogistic premises were the names of secondary substances and that these premisses excluded proper names, adjectives, and relational expressions. He has modified this view so that he now holds simply that the set *U* of characters is non-empty, while he declines to say what these characters represent, and that, even if proper names, adjectives, and relational expressions are not excluded from syllogistic, still they are not "explicitly handled" therein. I believe that in what follows I present some of the evidence which helped to induce him to modify his view.

My evidence is designed to show: first, that proper names, adjectives, and relational expressions can appear in syllogistic premisses, although their roles in them are restricted; second, that the characters in the set *U* represent designata in all the ten categories and that according to Aristotle, although these designata are infinite in number, still the set *U* of characters representing them is finite; and, third, although it might be the case that Aristotle's theory of propositional forms is inadequate for some purposes, it is adequate for the purposes for which it was devised.

To begin, it should be pointed out that in Aristotle's logical syntax 'universal' (καθόλου) – a prepositional phrase which does not admit of a plural – is not a stand-in for 'secondary substance' or 'name of a secondary substance'. Aristotle recognizes quantifying conventions for subjects and for propositions but not for predicates (*De Int.* 17a39–b6). Designata (τῶν πραγμάτων, 17a39) of subject expressions are universal if they are such that their signs can be predicated of many subjects ('man', for example); they are individual if they are such that their signs cannot be predicated of many subjects ('Callias', for example).

A proposition may have either an individual or a universal subject

(*De Int.* 17b3). A proposition with an individual subject is a singular proposition. A proposition with a universal subject is either universal, if the predicate applies to all or to none of the subject, or not-universal, if the predicate applies to less than all and more than none of the subject. Aristotle modifies this analysis in the *Analytica Priora* only by introducing two sub-classifications of not-universal propositions – particular and indefinite.

Now it is true that, for Aristotle, only expressions whose designata are substances can take the subject place in sentences and only expressions whose designata are secondary substances – and these within certain additional limits – can take both the subject place and the predicate place in sentences. The name of an individual primary substance cannot be a predicate; sentences with names of individuals in the predicate place are ill-formed – they are predications only accidentally (κατὰ συμβεβηκός; cf. *An. Pr.* 43a34–35).

Names of accidental attributes, on the other hand, may take only the predicate place in sentences, never the subject place (*An. Post.* 83b19–22). When accidents appear to be treated as subjects, Aristotle holds, it is actually the object in which the accident is present which is the subject of predication (*Cat.* 5b *ad init.*; *An. Post.* 83a33).

But this doctrine of Aristotle's does not exclude proper names and adjectives from the premisses of syllogistic. All it accomplishes is the exclusion of proper names from the predicate place, since these are less general than their putative subjects, and the exclusion of disembodied accidents from the subject place, since there are no such things as disembodied accidents. Proper names are not excluded from the subject place, nor are adjective-qualified subjects excluded from the subject place. Examples of syllogistic premisses containing proper names (Aristomenes, 47b22; Miccalus, 47b30; Pittacus, 70a16, 26) occur in the *Priora*, as do examples containing adjectives (good, 24a10, 25a7, etc., white, 25b6 *sqq*, 26a38, etc.; inanimate, 26b14, 27b *ad fin.*, etc.). It is true, of course, that proper names are of little importance in Aristotelian scientific inquiry; his reasons for this are given in the well-known passage beginning at 43a25: individuals cannot be predicates, except in an accidental sense (κατὰ συμβεβηκός), highest genera cannot be subjects, except by way of opinion (κατὰ δόξαν); scientific inquiry is concerned chiefly with the orders intermediate between these two extremes. But this no more excludes proper names from the premisses of syllogistic than it excludes the names of highest genera therefrom.

As to adjectives, several points ought to be noted. The first is that the ancient Greeks did not distinguish parts of speech precisely as we do; moreover, they were especially wont to use adjectives as substantives (by prefixing a definite article or by some other device). The second point is that Aristotle's logical syntax does not distinguish adjectives from nouns, nor indeed from verbs. His logical syntax recognizes only the name (ὄνομα) and the verb (ῥῆμα). These are best understood, I think, as 'argument', and 'function' or 'predicate'. A name, for Aristotle, is "a sound significant by convention, which has no reference to time, and of which no part is significant apart from the rest" (*De Int.* 16a19–21). Names stand for states of affairs (πράγματος, 16b23), and verbs not conjoined with arguments are names in this sense, but they make no assertions about states of affairs unless conjoined with arguments. Names serve as arguments to proposition-forming functors; some inflexions of nouns are excluded because they do not meet this condition (*De Int.* 16a35–b5). A verb for Aristotle is

> that which, in addition to its proper meaning, carries with it the notion of time... it is a sign of something said of something else... i.e. of something either predicable of or present in some other thing. (*De Int.* 16b6–11.)

Verbs and tenses of verbs are proposition-forming functors; no expression, no matter how complex, is a proposition (λόγον ἀποφαντικόν) unless it contains a verb (*De Int.* 17a11–15).

The third point is that Aristotle's semantic theory recognizes ten categories, or varieties of designata of expressions – substance and the nine accidents. In the definition of 'verb' above, the expression 'something either predicable of or present in some other thing' makes it clear that a verb or predicate may designate any non-individual falling under any of the ten categories. For Aristotle, secondary substances are predicable of other subjects, that is, they effect definitory predications of those subjects. Accidents, on the other hand, are present in subjects, that is, they effect descriptive predications of the subjects in which they inhere (*Cat.* 1a20–1b9).

Aristotle's list of categories cuts across distinctions among parts of speech. Likewise, his thematic separation of definitory predication from descriptive predication cuts across those distinctions. Thus it is the case that for Aristotle adjectives, as well as more complex expressions, expressive of quantity, quality, relation,[6] action, passion, time, place, *habitus* and *situs* are admitted to the premisses of syllogistic.[7] They are excluded from the premisses of demonstrative syllogistic, but not because they are adjectives – rather because they are mere descriptive predicates, since their designata are accidents. Predicates in demonstrative premisses must be assigned to all of the subjects to which they might belong, and must be assigned to those subjects because of what they are (καθ'αὑτό).[8] Demonstrative premisses are definitory predications. Some adjectives, however, by contrast with expressions whose designata are mere accidents, can effect the definitory or derivatively definitory predications requisite for demonstration. For instance, 'inanimate', an adjective we have already seen exemplified, since it represents a differentia,[9] could occur in a demonstrative premiss.

To sum up, then, the vocabulary of analytical syllogistic –and that of dialectical syllogistic as well – draws on expressions in all the ten categories. The characters in the set *U* of non-logical constants in Corcoran's syllogistic master language *L* should be said to represent not only secondary substances but also primary substances (as long as they occur in the subject place only) and accidents (as long as they occur in the predicate place only). These designata represented by the set *U* are infinite in number, but the set *U* of characters itself is finite. Aristotle's view, as expressed at *Sophistici Elenchi* 165a5 *sqq*, is that while designata are infinite in number (τὰ δὲ πράγματα τὸν ἀριθμὸν ἄπειρά), these designata themselves are not introduced into discussion, but names are used to stand for them, and names and the sum total of formulae are finite (τὰ… ὀνόματα πεπέρανται καὶ τὸ τῶν λόγων πλῆθος), so that a single name or formula must stand for many designata (ἀναγκαῖον οὖν πλείω τὸν αὐτὸν λόγον καὶ τοὔνομα τὸ ἓν σημαίνειν).[10]

Demonstrative syllogistic, a methodological sub-system of analytical syllogistic, employs a sub-language of the master language. This sub-language, however, is not a topical sub-language, as are the proper languages of the several sciences in which demonstration is employed. It is rather a topic-neutral but modally partisan language whose non-logical constants are limited to characters representing species, genera, differentiae, properties, and definitions. This set of characters also is finite.

It should be mentioned that there are no modally neutral premisses even in the so-called assertoric syllogistic. Descriptive predications are contingent, and definitory predications are necessary in varying degrees. Hence, 'assertoric syllogistic' is something of a misnomer, although Aristotle apparently is willing to allow predications to be considered prescinding from the modality conferred on them by their content words. Prescinding from modality is one end which his use of variables allows him to accomplish. The relation of this tacit modality to the expressed modality of the so-called modal syllogistic has yet to be fully explored.

What I propose in answer to Corcoran, then, is that the theory of propositional forms presupposed in the *Analytica* and used for analytical syllogistic is no more limited and no less complex than the theory elaborated elsewhere in the *corpus*. As I have said, I am not competent to deal with the question whether Aristotle's theory of propositional forms is adequate for geometry. But I would like to draw attention to a statement of Aristotle's whose full force has not been appreciated. Everyone has noted that in the first sentence of the *Analytica Priora* Aristotle announces that his inquiry is concerned with demonstration and belongs to demonstrative science. Now 'demonstrative science' (ἐπιστήμης ἀποδεικτικῆς) here could be taken either as a body of knowledge or as a mental activity. But whichever way this phrase is construed, Aristotle has laid double emphasis on the limited scope of his inquiry: he is concerned first of all with a certain method – demonstration, and, in addition, depending on how one takes 'demonstrative science', he is concerned with that method, either applied in its own proper field – demonstrative science – or applied through the exercise of its own proper intellectual virtue – demonstrative science.

According to Aristotle, some sciences, by reason of the exactitude and necessity of their subject matter, are appropriate fields for the method of demonstration. But this is not to say that all the theorems of any given science are susceptible of being demonstrated in the Aristotelian fashion. A science may be counted among the demonstrative sciences because demonstration is used to exhibit some of its propositions. More strictly speaking, however, 'demonstrative science' designates solely the necessary knowledge secured by demonstration. Thus, while the limitations set on propositional forms in demonstrative syllogistic may make them inadequate to the expression of all the theses of, say, Greek geometry, still, *ex hypothesi*, they are adequate to demonstration with its severely limited aims. Hence, if we allow Aristotle the intended interpretation of his demonstrative syllogistic, recognizing that demonstration is a method applied in a number of sciences but that it need not be the method which exhausts any science, then we should find it easier to grant that he played by the rules which he had himself laid down.

On Corcoran's view, Aristotle had no doctrine of logical truth. There can be no doubt that he is entirely correct in pointing out that Aristotle did not have a doctrine of logical truth like the doctrines developed in this century.[11] As Corcoran himself points out, however, and as Bocheński pointed out earlier ([2], pp. 92–93), Aristotle was conversant with the identity relation which plays such an important role in modern theories of logical truth. How could this be so?

The answer seems to be connected with the fact that, for Aristotle, identifications are not predications: on his view, there is no predication unless something is said of something else (*An. Pr.* 43a25–43; *De Int.* 16b6–11). As he says, predicates must be of a higher order than their arguments (*Cat.* 1b9; 2b15–21). Thus his syllogistic, which is concerned with predications, leaves little place for the identity formulae that figure so large in theories of logical truth.

Logically true statements evidently were employed in deductive contexts by some of Aristotle's contemporaries, but he seems to view the practice with disdain. For example, at *Analytica Posteriora* 73a6, Aristotle, in seeking to refute those who hold that demonstration is circular, remarks that their claim amounts to maintaining that "from *A* being so, *A* is so" (τοῦ A ὄντος τὸ A ἐστίν). From this tautology, he correctly observes, it is easy to prove anything. Hence it appears that Aristotle, although he recognized some of the features found in modern theories of logical truth, considered these features disadvantageous in the development of his own method of inquiry.

Corcoran also takes the view that Aristotle, because he had no theory of logical truth in which logically true statements are such in virtue of their form alone, had no system of logical axioms built up from logically true statements. Indeed, according to Corcoran, Aristotle did not distinguish logical from non-logical axioms because he had no idea of logical axioms. This, I think, is a little too strong. Of course, it is true to say that in Aristotle there is no close analogue of modern discussions of logical axioms *vis-à-vis* non-logical axioms. On the other hand, Aristotle does distinguish common principles of demonstrative science from the proper principles of the several demonstrative sciences (*An. Post.* 77a22 *sqq*; *Metaph.* 996b26 *sqq*). Now it must be admitted that Aristotle includes among common principles not only what might be acknowledged as logical laws – for instance, Excluded Middle and Contradiction, but also axioms which apply only to quantity – for instance, 'equals subtracted from equals have equal remainders'. These latter are reckoned among common principles because they are applied in the several fields of arithmetic, geometry, astronomy, optics, and so forth. These common principles do differ from proper principles, however, in that they are in the nature of rules which one might plausibly write as the justification of a step in a deduction. Proper principles, by contrast, are the definitions assumed by each science of its own peculiar subject matter. So, granted that Aristotle does not distinguish logical from non-logical axioms quite as we do, still I think it is too strong to say that he does not distinguish them at all.

Another feature of Aristotle's doctrine of truth which strikes one as anomalous in view both of modern practice and of his own apparent practice is his inclusion of a third variety of syllogistic premiss alongside universal and particular premisses. This third premiss variety – indefinite (ἀδιόριστος) – seems to resist extensional truth valuation and so to remain outside the set-theoretic interpretation which is usually given to syllogistic and which at least some statements of Aristotle's seem to indicate that he intended for it. Again, I shall not attempt to settle this question here, but I would like to offer a few suggestions concerning Aristotle's seemingly odd doctrine.

It has often been supposed that ἀδιόριστος indicates a premiss which might be universal or particular but whose quantity is left in doubt. It has also been proposed that for Aristotle ἀδιόριστος premisses are equivalent to particular premisses (Bocheński [1], p. 43). This last is not true: what Aristotle does say is that in a given syllogistic schema replacement of a particular affirmative premiss by an ἀδιόριστος premiss will yield the same result – either a valid syllogism or no syllogism, as the case may be (cf., for instance, *An. Pr.* 26b21; 27b36–38; 29a8 *sqq*; 29a27 *sqq*).

Further, there are at least two items of evidence which suggest that the first supposition – that ἀδιόριστος premisses as yet unquantified might still become quantified – is not true either. One item of evidence is drawn from Aristotle's examples of ἀδιόριστος premisses at 24a20–22: "contraries are studied by the same science" and "pleasure is not good". Now 'contraries' and 'pleasure' are universal since they can be predicates according to the rule stated at *De Int.* 17a39, but there seems to be some question whether the objects they designate are properly numerable. Ryle in *Dilemmas*, for example, points out that pleasure is not the sort of thing that one counts ([17], pp. 54–67, esp. p. 60).

The suspected innumerability of these objects brings us to the second item of evidence – the term ἀδιόριστος itself. This seems to be a technical term coined by Aristotle, since according to *LSJ* it does not occur before him and since the Greek word usually rendered 'indefinite' or 'indeterminate' – ἀόριστος – was available. Aristotle in fact does avail himself of ἀόριστος, not only in the *De Interpretatione* (16a32, b15), but right in the *Priora* (32b11 *sqq*), where modality, not quantity, is in question.

If ἀδιόριστος is an *ad hoc* coinage, what special force does Aristotle mean it to carry? A clue is provided at 26b24, where ἀδιόριστος is opposed to διωρισμένον. Translators customarily render these 'indefinite' and 'definite', but this seems to be a hedge. Aristotle uses διωρισμένον in the *Categoriae* in his discussion of the two kinds of quantity (4b20 *sqq*). Quantity, he says, is either discrete (διωρισμένον) or continuous (συνεχές), and among discrete quantities is number. So, if διωρισμένον means definite in the sense of 'discrete and numerable', I think we may take ἀδιόριστος as its opposite, not in the sense of 'continuous', because συνεχές already takes care of that, but simply in the sense of 'innumerable'.

It would appear, then, on this evidence, that ἀδιόριστος premisses are not so much quantifiable and as yet unquantified as they are in principle unquantifiable. Corcoran urges that they are extra-systematic with respect to syllogistic, and it is true that they are extra-systematic with respect to demonstrative syllogistic; but then so are particular premisses. On the other hand, ἀδιόριστος premisses are useful in non-demonstrative inquiries, and premisses of this sort do figure, for instance, in the argumentation of Books VII and X of the *Ethica Nicomachea.*

In his conclusion Corcoran raises the tantalizing question whether reasoning on Aristotle's view is natural or conventional. This would be a fit subject for a whole monograph, so I shall content myself here with suggesting the lines along which such a monograph might be composed.

First, one would have to point out that for Aristotle no hard lines divide natural from the conventional. Next, one might observe that for Aristotle the basis of reasoning, that is, the grasp of first principles, is immediate, intuitive, non-discursive, and non-linguistic. The derivation of other knowledge from the first principles is mediated, discursive, and linguistic. For Aristotle, I think it is safe to say that all logic is language and all language is conventional, but that not all conventions are arbitrary.

Swarthmore College

NOTES

[1] Łukasiewicz [8]. See McCall [10], p. 69, n. 1, for Łukasiewicz's remark concerning the date of his first proposals.

[2] Łukasiewicz [6], p. 44. For texts in *An. Pr.* and *An. Post.*, see Corcoran [3].

[3] Please note that I do not make the claim that Aristotle did develop a system of propositional logic. Such an Aristotelian system, if there is one, waits to be discovered. My claim is only the much more modest one which attributes the absence of a propositional logic not to Aristotle's ignorance nor to his inability but rather to his having other interests.

[4] For including astronomy in the Aristotelian division of sciences, see Merlan [11], p. 6; for excluding mathematics, see Merlan [12]; for including both mathematics and astronomy, see Mulhern [14].

[5] I follow Bocheński ([2], p. 88 *passim*) in recognizing analytical and non-analytical branches of syllogistic.

[6] Bocheński ([2], pp. 95–97) finds laws of the logic of relations at *Top.* 114a18 *sqq*; 119b3 *sqq*; 114b40–115a2; *An. Pr.* 48a40b9; 48b10–14, 14–19, 20–27; and *Top.* 114b38–115a14. It should be noted that C. S. Peirce claimed over and over an Aristotelian precedent for the logic of relatives. Cf. Peirce [15] 2.532, 2.552–553, 2.577, and 3.643.

[7] The occurrence of 'white', 'black', 'good', and 'inanimate' in syllogistic premisses has already been noted. There also occur in *An. Pr.* 'wild' (28b *ad fin.*), 'moves' (30a31), 'waking' and 'sleeping' (31b9), 'biped' (31b31), 'intelligent' (34b33), 'upright' (41b10), and so on. Aristotle also discusses the establishment or refutation, by syllogistic means, of accidents and properties, as well as genera (cf. 42b26 *sqq*).

[8] Cf. *An. Post.* 73a21–74a3. For a detailed discussion of this passage, see Mulhern [13].

[9] Differentiae, for Aristotle, are products, not conditions, of analysis; they do not answer to designata in any category. They are, however, assimilated to genera by Aristotle (*Top.* 101b19). They do not of themselves constitute definitory predicates, but their role in Aristotle's theory of predication is to express, in combination with the names of genera, analyses of species – that is to say, the definitions which are exhibited in demonstration.

[10] Cf. Aristotle's statement at *Metaph.* 1007a14–15 that accidents are infinite. Norman Kretzmann has correctly noted the importance of *Soph. El.* 165a5 *sqq* for the history of semantics. See Kretzmann [5], p. 362.

[11] For a useful précis of such systems, see Corcoran [4], (1) Logical Truth: Logistic Systems.

BIBLIOGRAPHY

[1] Bocheński, I. M., *Ancient Formal Logic*, North-Holland Publ. Comp., Amsterdam, 1951.

[2] Bocheński, I. M., *A History of Formal Logic*, (transl. and ed. by Ivo Thomas), Second ed. corrected, Chelsea Publ. Comp., New York, 1970.

[3] Corcoran, John, 'A Mathematical Model of Aristotle's Syllogistic', *Archiv für Geschichte der Philosophie* **55** (1973), 191–219.

[4] Corcoran, John, 'Three Logical Theories', *Philosophy of Science* **36** (1969), 153–157.

[5] Kretzmann, Norman, 'Semantics, History of', in *Encyclopedia of Philosophy* (ed. by Paul Edwards), Macmillan and Free Press, New York, 1967, Vol. 7, pp. 358–406.

[6] Łukasiewicz, Jan, *Aristotle's Syllogistic from the Standpoint of Modern Formal Logic*, Oxford Univ. Press, Oxford, 1951. Second ed. enlarged, 1957.

[7] Łukasiewicz, Jan, *Elementy logiky matematycznej* (ed. by M. Presburger), Publications of Students of Mathematics and Physics in Warsaw Univ.; Vol. 18, Warsaw, 1929. Second ed., Polish Scientific Publ., Warsaw, 1958. English transl. by Olgierd Wojtasiewicz, Pergamon Press, Oxford, 1963.

[8] Łukasiewicz, Jan, 'Philosophische Bemerkungen zu mehrwertigen Systemen des Aussagenkalküls', *Comptes rendus des séances de la Sociéte des Sciences et des Lettres de Varsovie* **23** (1930). English transl. by H. Weber in McCall [10] as 'Philosophical Remarks on Many-Valued Systems of Propositional Logic', pp. 40–65.

[9] Łukasiewicz, Jan, 'Z historii logiki zdań', *Przegląd Filozoficzny* **27** (1934), 417–437. German version 'Zur Geschichte der Aussagenlogik', *Erkenntnis* **5** (1935), 111–131. English transl. from German version by S. McCall in McCall [10], pp. 66–87.

[10] McCall, Storrs (ed.), *Polish Logic 1920–1939*, Oxford Univ. Press, Oxford, 1967.

[11] Merlan, Philip, 'Aristotle's Unmoved Movers', *Traditio* **4** (1946), 1–30.

[12] Merlan, Philip, "On the Terms 'Metaphysics' and 'Being-Qua-Being'", *The Monist* **52** (1968), 174–194.

[13] Mulhern, M. M., 'Aristotle on Universality and Necessity', *Logique et Analyse* **12** (1969), 288–299.

[14] Mulhern, M. M., 'Types of Process According to Aristotle', *The Monist* **52** (1968), 237–251.

[15] Peirce, C. S., *Collected Papers* (ed. by Charles Hartshorne and Paul Weiss), 8 Volumes, Harvard Univ. Press, Cambridge, 1960–1961.

[16] Ross, Sir David, *Aristotle's Prior and Posterior Analytics*; a revised text with introduction and commentary, Oxford Univ. Press, Oxford, 1965.

[17] Ryle, Gilbert, *Dilemmas*, Cambridge Univ. Press, Cambridge, 1966.

PART FOUR

STOIC LOGIC

JOSIAH GOULD

DEDUCTION IN STOIC LOGIC

In their logical theory Stoic philosophers made use of a simple but important distinction alleged to hold among valid arguments, a distinction to which Aristotle had first called attention.[1] They distinguished those arguments whose validity is evident from those whose validity is not evident and so needs to be demonstrated. The Stoics, having supposed that the distinction obtains, raise and answer the question, how does one demonstrate the validity of those arguments whose validity is not plain? The Stoics appear to have set forth both a discursive method of demonstration and a test for validity. In this paper I examine these two facets of Stoic logic.[2]

The paper is in three parts. The first is essentially terminological and taxonomic. There I record Stoic definitions of logical terms and I give three Stoic classifications of arguments, appending samples from the writings of Sextus Empiricus.[3] This provides and puts on exhibit an array of typically Stoic arguments to which I refer in the second part of the paper. There I examine Sextus' contention that the disagreement among the Stoics over the criterion of truth for a conditional proposition renders inefficacious the test that had been set forth as sufficient for judging the validity of an argument, and I argue that Sextus' charge has to be qualified. Even if an unqualified form of Sextus' accusation could be established, its importance, I maintain, would be diminished by the fact that the Stoics didn't make extensive use of this test anyhow. As I show in the third part of the paper, the Stoics ordinarily claim to prove the validity of all valid arguments[4] not by means of a test but by means of a calculus of propositions[5] having its base in a theory of deduction, which includes a language consisting of connectives and variables, axiomatic inference schemata, and rules of derivability. I conclude with a statement about the Stoic theory of deduction in relation to systems of logic developed in the 19th and 20th centuries and to Aristotelian syllogistic.

J. Corcoran (ed.), Ancient Logic and Its Modern Interpretations, 151–168.

I

In Sextus Empiricus' *Outlines of Pyrrhonism*[6] one finds the following Stoic definitions of the expressions 'premises', 'conclusion', and 'argument'

(i) 'premises': the propositions assumed for the establishment of the conclusion,

(ii) 'conclusion': the proposition which is established by the premises[7],

(iii) 'argument': a whole composed of premises and a conclusion.

In terms of these definitions the questions I shall be attempting to answer are: for the Stoics what are the conditions under which the premises in an argument logically imply its conclusion? And, if the premises of an argument in fact imply its conclusion but not evidently so, how according to the Stoics may this relation of logical consequence be made evident? Before dealing with these questions, however, I present several classifications of Stoic arguments (see the outline of these classifications below).

The first division of the first classification of arguments is into valid and invalid arguments. An argument is valid "when the conditional having as its antecedent the conjunction formed from the premises of the argument and as its consequent the conclusion of the argument is true" (*P.H.* ii.137). An example of a valid argument is

(1) If it is day, it is light.
It is day.
Therefore it is light.

Arguments which do not satisfy this condition are invalid.

Next valid arguments are divided into those which are true and those which are not true. A true valid argument is one of which both the conclusion and the premises are true (*P.H.* ii.138). An example of a true valid argument is (1) above when set forth during the day. Arguments which do not satisfy this condition are not true. An example of a not-true argument is the following when made during the day:

(2) If it is night, it is dark.
It is night.
Therefore it is dark.

Of true valid arguments some are demonstrative and some are not demonstrative. Demonstrative arguments are "those which conclude something non-evident through pre-evident premises".[8] An example of a demonstrative true valid argument, preserved by Sextus (*P.H.* ii.140), is

(3) If sweat flows through the surface of the skin, there exist imperceptible pores.
Sweat flows through the surface of the skin.
Therefore there exist imperceptible pores.

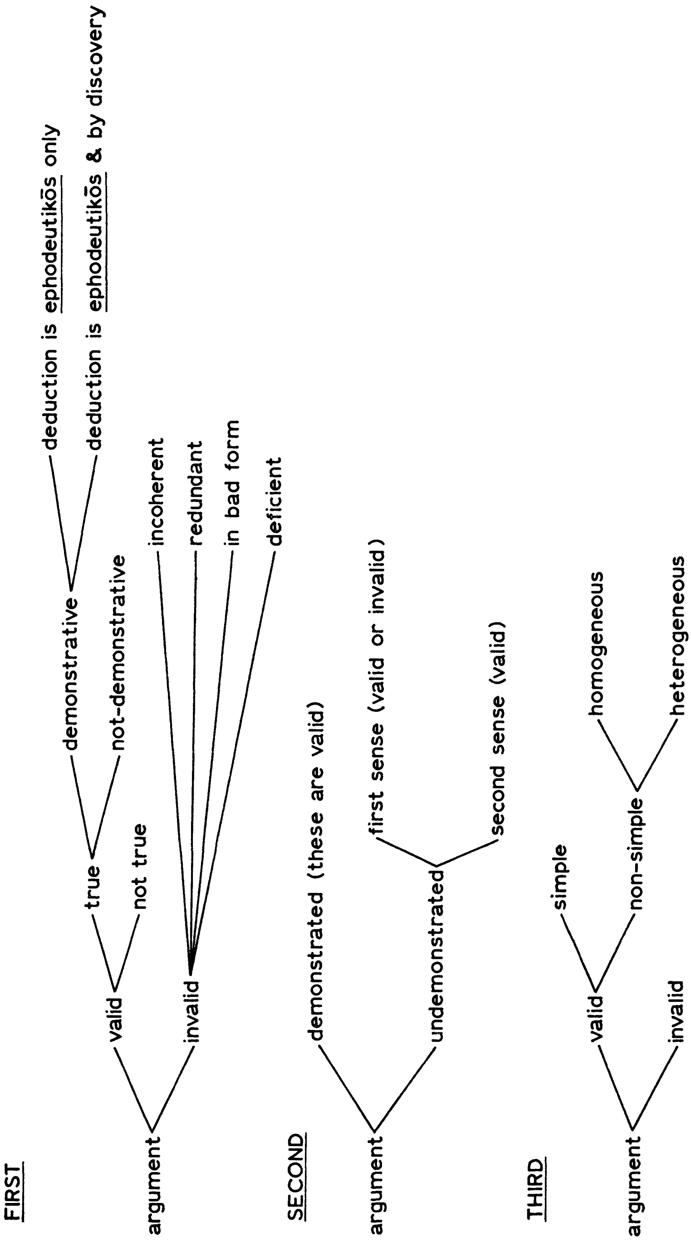
An argument not satisfying this condition is not demonstrative. Argument (1) is an example of an argument which is valid, true when set forth during the day, and not demonstrative. It will be shown subsequently that there was another kind of argument called undemonstrated, which provides an additional important category of arguments. It is not to be confused with a not-demonstrative argument.

Of demonstrative true valid arguments "some lead us through the premises to the conclusion *ephodeutikōs* only" (*P.H.* ii.141). I am not sure precisely what '*ephodeutikōs*' means. Etymologically the word suggests 'advancing over a path towards something' and when the expression attaches to the word 'argument' a reasonable candidate for the 'something' would be the conclusion of the argument. But 'advancing over a path towards a conclusion' is a metaphorical description of arguments generally and it fails to bring out what is peculiar to the type of argument to which the label is here attached. I simply transliterate the expression. A kind of this type argument is said to be one which "depends upon belief and memory". One might well ask, 'What kind of argument doesn't?' An example of an argument which depends on belief and memory is

(4) If someone said to you that this man would be wealthy, this man will be wealthy.
This god said to you that this man would be wealthy.
Therefore this man will be wealthy.

Sextus' comment on this argument is that we "assent to the conclusion not so much on account of the necessity of the premises as because we believe the assertion of the god" (*P.H.* ii.141–142).

SOME STOIC CLASSIFICATIONS OF ARGUMENTS



Contrasted with this type argument are those which "lead us to the conclusion not only *ephodeutikōs* but also by way of discovery" (*P.H* ii.142). An example of such an argument is (3). The element of discovery in this argument is the disclosure of the existence of pores through the fact that sweat flows through the surface of the skin. The element of belief in the argument, apparently sufficient to provide the *ephodeutikōs* component, is the "prior assumption that moisture cannot flow through a solid body" (*P.H.* ii.142).

The components of a 'demonstration' may be derived from one component of each division in this first classification, for a demonstration is a valid and true argument having a non-evident conclusion and disclosing that conclusion by the power of the premises (*P.H.* ii.143). I am uncertain as to the point of the last clause in Sextus' report. It appears to imply that the conclusion is obtained without the aid of assumptions external to the premises of the argument, although this would involve the existence of a class of demonstrative arguments different from those which are *ephodeutikōs*.

A second Stoic classification of arguments is also reported by Sextus, and it, too, ought to be kept in mind when thinking about deduction in Stoic logic. This classification begins from a division of arguments into demonstrated and undemonstrated. I take a demonstrated argument in this context to be one whose validity has been made evident. I say more subsequently about how the validity of arguments is made evident. An argument is undemonstrated in one of two senses. The first sense is the contradictory of that of 'demonstrated'. In this sense, then, an argument is undemonstrated if it has not been demonstrated (*Adv. Math.* viii.223), i.e., on my interpretation, if it has not been shown to be valid. In a second sense an argument is undemonstrated if it is immediately evident that it is valid (*ibid.*). This distinction may be brought out by noticing that the first sense is temporal inasmuch as an argument which is undemonstrated in that sense in 100 B.C. may be demonstrated in 50 B.C., while the second sense is non-temporal.[9] An argument is undemonstrated in this second sense if it exhibits one of five forms of argument which are referred to respectively as the first undemonstrated, the second undemonstrated, etc. These forms are also called *inference schemata*, and I have more to say about them below. For now I merely give the forms with illustrative examples (Gould, pp. 83–85):

The first undemonstrated

(5)	If the first, the second. The first. Therefore the second.	If it is day, there is light. It is day. Therefore there is light.

The second undemonstrated

(6)	If the first, the second. Not the second. Therefore not the first.	If it is day, there is light. There is not light. Therefore it is not day.

The third undemonstrated

(7)	Not both the first and the second. The first. Therefore not the second.	Not both it is day and it is night. It is day. Therefore it is not night.

The fourth undemonstrated

(8)	Either the first or the second. The first. Therefore not the second.	Either it is day or it is night. It is day. Therefore it is not night.

The fifth undemonstrated

(9)	Either the first or the second. Not the first. Therefore the second.	Either it is day or it is night. It is not day. Therefore it is night.

A third classification divides valid arguments first into simple and non-simple (*Adv. Math.* viii.228). A simple valid argument is one having the form of one of the five undemonstrated argument forms. A non-simple valid argument is one 'woven together' out of simple valid arguments in order that it may be known to be 'valid' (*Adv. Math.* viii.229). There are two kinds of non-simple arguments, one formed from two or more simple arguments all of the same form, and the other composed from two or more simple arguments not of the same form. The former is a homogeneous non-simple and the latter, a heterogeneous non-simple argument (*ibid.*). An example of a homogeneous non-simple argument is

(10) If it is day, then if it is day it is light.
It is day.
Therefore it is light.

For upon analysis it may be seen to have been compounded from two simple arguments having the form of the first undemonstrated. Analysis of this argument is carried out in accordance with the following 'dialectical theorem':

(11) Whenever we have premises from which a certain conclusion can be validly deduced, potentially we have also that conclusion among the premises, even if it is not stated explicitly.[10]

One analyzes (10) by drawing the conclusion from the first two premises in accordance with the first undemonstrated inference schema, thus getting

(12) If it is day, then if it is day it is light.
It is day.
Therefore if it is day, it is light.

Then by the theorem stated in (11) one gets as premises

(13) If it is day, then if it is day it is light.
It is day.
If it is day, it is light.

And by another application of the first inference schema one gets the conclusion in (10).

II

Now with this array of Stoic arguments on display I go on to consider how the Stoics talked about valid argument and valid inference. In three relatively extended accounts of Stoic logic from antiquity (Diogenes Laertius' *Vitae* vii. 42–83; Sextus Empiricus' *Outlines of Pyrrhonism* ii. and *Adversus Mathematicos* viii.) the talk about the validity of arguments is of two kinds. On the one hand, and this approach is found exclusively in the reports by Sextus, the validity of an argument is linked to the truth of its corresponding conditional proposition, i.e., to the conditional proposition having as its antecedent a conjunction of the propositions forming the premises of the argument and as its consequent the conclusion of the argument. Sextus is even more specific. He makes the truth of its corresponding conditional a sufficient condition for the validity of an argument. On the other hand, and this approach is found both in Sextus (*Adv. Math.* viii.228–229) and in Diogenes (*Vitae* vii. 79–81), the validity of some arguments is said to be evident and the validity of others, it is maintained, has to be shown by the analysis or resolution of them into those which are evidently valid.

I want first to consider Sextus' ascription to the Stoics of the view that a sufficient condition for the validity of an argument is the truth of its corresponding conditional (henceforth 'the conditionalization test'). One doesn't find in the logic fragments of the Stoics very many references to such a test or its use, but that's a historical point. It cannot, I think, be denied that Sextus is right in suspecting that the conditionalization test would yield ambiguous results as long as the disagreement over the criterion for the truth of a conditional proposition remained unsettled, but I shall argue that there is something to be said here in defense of the Stoics. Four different criteria are attested and three of them are identical with three kinds of 'implication' which have had advocates in the 19th and 20th centuries. I briefly treat these four criteria and then return to the conditionalization test.

The first criterion is that "the conditional proposition is true when it does not begin from the true and conclude with the false" (*Adv. Math.* viii.113). This form of implication, as the texts make abundantly clear, is what is now called 'material implication' (Mates, p. 44). After its author, Philo, I call a conditional true by this criterion a Philonian conditional. The second of the criteria is that "the conditional proposition is true which neither could nor can beginning from the true conclude with the false" (*Adv. Math.* viii.115). This criterion is attributed to Diodorus, and I call a conditional true by this criterion a Diodorean conditional. As Mates has convicingly shown (pp. 44–47), a Diodorean conditional is an always true Philonian conditional. This, then, appears to have been an ancient version of Whitehead's and Russell's formal implication.[11] The third and fourth criteria, which are not attributed to any individual, are authored by persons who seem to have interpreted conditional propositions as statements of necessary connection. As Martha Kneale writes, it "seems likely that they were formulated by philosophers who had in mind the use of conditionals in place of entailment statements" (p. 134). The third criterion of the truth of a conditional proposition is that such a proposition is true "whenever the contradictory of the consequent in it is incompatible with the antecedent in it" (*P.H.* ii.111). This looks very much like strict implication.[12] It is not explicitly ascribed to Chrysippus, but I think he is its author and have said why elsewhere (Gould, pp. 72–82). A conditional true by this criterion I call a Chrysippean conditional. The fourth criterion is that "the conditional proposition is true whose consequent is potentially included in its antecedent" (*P.H.* ii.112). This criterion is ascribed to "those who judge by way of signification" and it, according to its unnamed authors, explicitly excludes conditionals with duplicated propositions, such as 'if it is day, it is day', on the ground that every such duplicated conditional will be false (*ibid.*). I shall not recur to this type of true conditional, for I am not at all sure that I understand it, and anyhow the Stoics countenanced true duplicated conditionals (*Adv. Math.* viii. 108–110), and so it is probable that none of them adhered to the signification theory.

Now, returning to the conditionalization test, if the Stoics did invoke it to test the validity of arguments, it makes sense to raise the question, as Sextus did, whether the conditional corresponding to the argument being tested has to be a Philonian conditional, a Diodorean conditional, or a Chrysippean conditional? And I wish to consider that question, making use of the sample Stoic arguments presented in the first part of this paper. Consider first the conditional proposition corresponding to argument (1). It is

(14) If (it is day, and if it is day then it is light), then it is light.

For a Philonian a conditional is true when it does not begin from the true and conclude with the false. In particular for a Philonian, then, this proposition is true if (i) it is true that it is light and (ii) false that it is day and (iii) false that if it is day it is light. For in that case it begins from the false and concludes with the true. And so the corresponding argument, (1), is valid. But this is an incredibly weak test, for it would also yield the verdict valid on the following argument:

(15) If it is day, then it is day.
It is day.
Therefore, it is not day.

For the corresponding Philonian conditional is true if, say, it is true (i) that it is not day and false (ii) that it is day. Thus the test, using Philonian true conditionals, would pronounce valid an argument whose conclusion was the contradictory of one of its premises. Judging by examples of valid Stoic arguments which have survived in the literature, the Stoics did not use a Philonian conditional in the conditionalization test. And, as has been seen, there is a good reason why they shouldn't have. So, if a dispute broke out at all over this issue, it would have been over the remaining three types of conditionals as candidates for use in the conditionalization test.

Advocates of the Philonian conditional may have claimed that for purposes of a conditionalization test an always true Philonian conditional is required. And that would have been to concede that for purposes of the test a Diodorean true conditional is required. Now if one applies the conditionalization test and regards as true conditionals those which are Diodorean true, one will let pass all those conditionals for which it is *never* the case that while the antecedent is true the consequent is false. And arguments of the following sort immediately come to mind:

(16) If it is day and it is not day, then it is light.
It is day and it is not day.
Therefore it is light.

(17) If it is day, then day is day.
It is day.
Therefore day is day.

Leaving aside the fact that the Stoics may not have regarded (17) as being well-formed, its corresponding conditional will always be true, for it will never have a false consequent; and the corresponding conditional of (16) will always be true, for its antecedent will never be true. This brings out one of the consequences of regarding true conditionals as Diodorean true, and this is that it is not easy to see how one could ever conclude that such a conditional is true, unless it be stating a logical truth (Mates, p. 50; Hurst, p. 488). It is interesting to observe in this connection, however, that none of the sample arguments which have survived are degenerately valid arguments like (16) and (17) in form. A typical extant argument is (3), and its corresponding conditional is

(18) If (sweat flows through the surface of the skin, and if sweat flows through the surface of the skin then there exist imperceptible pores), then there exist imperceptible pores.

The Diodorean says that this conditional is true if it is never the case that it is true both (i) that sweat flows through the surface of the skin and (ii) that if sweat flows through the surface of the skin, there exist imperceptible pores, while it is false (iii) that there exist imperceptible pores. And by this criterion the conditional proposition is of course true. It is never the case that one meets with this combination of truth values, because one cannot logically have that combination. If the conjunction forming the antecedent is true, then the consequent must by logic be true, and so of course for any time you choose if at that time the antecedent is true, then the consequent is true. But, if this is so, the Diodorean truth of the conditional is being warranted by the incompatibility test, i.e., by the recognition that (18) is Chrysippean true. It is the case not just as a matter of fact that the antecedent is never true while the consequent is false, but rather it could not be the case that the antecedent is true while the consequent is false. And this is a 'could not' that derives not from a logically always false antecedent nor from a logically always true consequent, but one which derives from the logical incompatibility of the antecedent with the negation of the consequent. The conditional, (18), is tautologous, but that it is tautologous is guaranteed by the fact that its consequent is strictly implied by its antecedent. Judging by the shape of the surviving arguments the Stoics must have believed that a sufficient condition – and, given the limitations of the human understanding and restrictions on what was to be regarded as logically true (arguments which are degenerately valid apparently were not to be regarded as logically true), also a necessary condition – for concluding that a conditional is Diodorean true is first concluding that it is Chrysippean true.

I infer, then, that Sextus was right in thinking that the Stoic disagreement over the criterion of a true conditional proposition would be reflected in their doctrine that the truth of an argument's corresponding conditional is a sufficient condition for the validity of the argument. Philonians would have had to concede that Philonian true conditionals are far too weak a test, passing arguments which no one would regard as valid. But there is no reason why they couldn't have said that for purposes of the conditionalization test, an always true Philonian conditional (a Diodorean true conditional) is required. And, while in principle logical truth generally guarantees Diodorean truth, in practice, to judge by the extant argument samples, it was thought that the Diodorean truth of an argument's corresponding conditional had itself to be warranted by a strict implication between the statement in its antecedent and that in its consequent. In his criticisms of the Stoics Sextus was right in principle, but the Stoics perhaps felt the force of his remark less because of the circumstances I have described.

It is clear that the Stoics maintained that if an argument's corresponding conditional is true, then the argument is valid. It is not equally evident that the dispute over the criterion for the truth of a conditional made the conditionalization test inefficacious. Indeed, it is not even evident that the Stoics made much use of the test. As I suggested above, the evidence shows the Stoics talking more about the proof of the validity of arguments than about the application of a criterion for the validity of arguments, and it is to this side of their theory of deduction that I now turn.

III

The Stoics assumed as basic or axiomatic the five 'undemonstrated' inference schemata (Bocheński, p. 96). These five inference schemata were thought to be evidently valid and were called undemonstrated precisely because no demonstration was thought to be required to make their validity evident. Secondly, they maintained that the validity of all valid arguments in forms other than one of the five basic argument forms could be shown by analysis,[13] a procedure of reducing these other arguments by means of certain rules to a series of two or more arguments exhibiting one or more of the basic inference schemata. The variables in the schemata were the first two ordinal numerals, 'the first' and 'the second'; and the substituends for these variables were to be sentences expressing propositions and denoting truth values. As was suggested two sentences back, the Stoics apparently claimed that their propositional calculus was complete (*P.H.* ii. 156–157; Mates, pp. 81–82).

Galen (*SVF* II 248) refers to four rules in accordance with which the analysis of non-simple arguments was to be carried out. We know two and possibly three of these rules. They are:

(19) *First rule*: 'If from two propositions a third is deduced, then either of the two together with the denial of the conclusion yields the denial of the other.[14]

(20) *Third rule*: 'Whenever from two premises a third is deduced, and other propositions from which one of the premises is deducible are assumed, then from the other premise and those other propositions the same conclusion will be deducible'. (Alexander, *In Arist. An. Pr. Comment.* 278, 12–14).

Neither of the remaining two rules is given as such in the extant fragments, but Sextus has preserved (*Adv. Math.* viii 231) what he calls a dialectical theorem, and Mates has argued convincingly (p. 78, note 77) that this must have been the Stoics' second rule. It goes as follows:

(21) = (11) *Second rule*: 'Whenever we have premises from which a certain conclusion can be validly deduced, potentially we have also that conclusion among the premises, even if it is not stated explicitly'.

We don't know what the fourth rule was nor very much about how the Stoics applied these rules. Mates' discussion and examples are well-known. I give now two samples, preserved by Sextus, of heterogeneous non-simple arguments. Each makes use of the third rule for an analysis of the argument which makes its validity become evident. The second of them shows that the Stoics must have had some principle about the inter-definability of connectives. One such argument is the following (*Adv. Math.* viii.234):

(22) (i) If the phenomena appear in the same way to all those who are in a similar condition and signs are phenomena, then signs appear in the same way to all those who are in a similar condition.
(ii) Phenomena appear in the same way to all those who are in a similar condition.
(iii) Signs do not appear in the same way to all those who are in a similar condition.
(iv) Therefore signs are not phenomena.[15]

Putting (i) and (iii) of this argument together one can deduce the negation of the antecedent in (i) in accordance with the second inference schema. Next in accordance with the third rule one can put this negation together with (ii) and by the third inference schema obtain the conclusion (iv).

A second sample argument found in Cicero (*De Divinatione* xxxviii. 82–83), a more complex variety of the non-simple heterogeneous form, goes as follows:

(23) (i) If the gods exist and they do not declare to men beforehand what future events will be, then either they do not love men, they do not know what future events will be, they judge that it is of no importance to men to know what the future will be, they think it is not consonant with their dignity to preannounce what future events will be, or the gods cannot reveal what future events will be.

(ii) It is not the case that they do not love us, nor is it the case that they are ignorant of the things which they themselves form and design, nor is it of no importance for us to know those things which will happen in the future, nor does giving signs of the future comport badly with their dignity, nor is it the case that they cannot reveal what future events will be.

(iii) Therefore it is not true that there are gods and that they do not give signs of future events.

(iv) There are gods.

(v) Therefore the gods do give signs of future events.

In this argument (ii) could have been regarded as the negation of the consequent in (i) only if there had been some principle which permitted negated conjuncts in a conjunction to be defined in terms of a negated disjunction having as disjuncts those conjuncts unnegated. I assume that the Stoics had some such principle of the interdefinability of connectives (Bocheński, p. 92). Step (iii) is derivable from (i) and (ii) by the second undemonstrated inference schema and by Rule Two may appear together with the premises. From it together with (iv) the conclusion (v) is derivable by the third undemonstrated inference schema.[16]

In light of what the Stoics said about valid arguments, their classification and examples of invalid arguments (as reported by Sextus *P.H.* ii. 146–151) has some curious features. Briefly, they categorized invalid arguments as incoherent, redundant, in bad form, or deficient. An example (*P.H.* ii.146) of an incoherent argument is

(24) If it is day, it is light.
Wheat is being sold in the market.
Therefore Dion is walking.

An inchoherent argument, then, appears to be one in which the propositions forming premises and conclusion are all logically independent of one another. Notice that an incoherent argument could pass a conditionalization test, given the appropriate circumstances, if the conditional in the test were regarded as true because Philonian true, but would not pass that test if the conditional in it had to be either Diodorean true or Chrysippean true.

An example (*P.H.* ii.147) of a redundant argument is

(25) If it is day, it is light
It is day and Dion is walking.
Therefore it is light.

Such an 'invalid' argument appears to be one having an unused premise. Given a rule for conjunction elimination, the argument could be shown to exhibit the first undemonstrated argument form and to pass the conditionalization test.

The example of an argument in bad form preserved by Sextus (*P.H.* ii.147) is

(26) If it is day, it is light.
It is light.
Therefore it is day.

This argument, obviously an instance of the fallacy of affirming the consequent, could pass the conditionalization test if the conditional in question were required to be only Philonian true but not otherwise.

Finally, an example of an argument invalid because of deficiency is (*P.H.* ii.150)

(27) Wealth is either good or bad.
But wealth is not bad.
Therefore wealth is good.

The argument is deficient inasmuch as the first premise does not state an exhaustive disjunction of the possibilities, having left out that of wealth being indifferent. This extra-logical consideration was evidently thought to militate against the validity of the argument, an argument which passes the conditionalization test whether the conditional involved be regarded as Philonian, Diodorean, or Chrysippean true.

Development of a calculus of propositions in the wake of Aristotelian logic is a brilliant achievement, whether it be the achievement of the Megarians, the Stoics, or the Megarians and the Stoics (Bocheński, pp. 78–79). How precisely and rigorously the system was developed is difficult to say on the basis of the extant fragments. Probably it would be going too far to ascribe to the Stoics a logistic including a language of primitive symbols (logical connectives and variables), formation rules, rules of inference, and definitions. But in their theory of deduction one finds an astonishing number of anticipations of work in modern logical theory.

What is more striking and what has gone more unnoticed is the symmetry between the Stoic logic of propositions and Aristotle's syllogistic. Corresponding to Aristotle's four perfect syllogisms are the Stoics' five basic inference schemata. Corresponding to Aristotle's rules of conversion, *reductio*, and ecthesis are the Stoics' rules for the analysis of non-simple valid arguments. In fact, the first inference rule of the Stoics just is a version of *reductio ad absurdum* (Bocheński, p. 81). Each logic makes the claim that all valid arguments can be shown to be so on the basis of its axioms and rules of derivability. Finally a Stoic demonstrative argument is a species of valid argument having true premises just as for Aristotle a demonstrative syllogism is a species of valid syllogism having true premises. And, just as Aristotle had maintained that one cannot demonstrate all propositions, so the Stoics maintained that "one mustn't demand a demonstration of all propositions" (*Adv. Math.* viii.367).

In conclusion, then, I should say that the Stoics' logic of propositions has several structural similarities with Aristotle's syllogistic and that it also looks forward to the more sophisticated deductive systems of the 19th and 20th centuries.

State University of New York at Albany

NOTES

[1] *Prior Analytics* I.24b22–26, 27a16–18. The distinction between plainly valid syllogisms and non-evidently valid syllogisms is for Aristotle the distinction between 'perfect'

syllogisms, on the one hand, and 'imperfect' syllogisms, on the other. A perfect syllogism is one in which, as Aristotle frequently puts it, the necessity (of the conclusion if the premises be assumed) is evident. That the Stoics presupposed this distinction is made clear in Part III of this paper.

[2] I wish to thank my colleagues, James A. Thomas and Harold Morick, for helpful critical remarks on an earlier draft of this paper. I am also enormously indebted to John Corcoran for many incisive remarks and helpful suggestions on two later versions of the paper.

[3] Sextus is the richest source we have for a knowledge of Stoic logic. Being a Sceptic he is extremely critical of the Stoics. He also tends to be tediously repetitious. He appears to have quoted and paraphrased with care, though there aren't always non-circular ways of checking this. As Mates has observed (p. 9), "any parts of Stoic logic which he found either too difficult or too good to refute will be absent from his account", but even so there is enough material in Sextus to extract a fairly good account of the elements of Stoic logic.

[4] Mates refers in several places (pp. 4, 58, 82) to and gives evidence for the Stoics' claim that their propositional logic was complete.

[5] The Stoics didn't call their logic a calculus of propositions (Diogenes Laertius groups Chrysippus' books dealing with the subject under the heading 'Logic in Relation to Arguments and Moods', *Vitae* vii. 193); but Stoic logic shares so many similarities with modern propositional logic, calling their logic 'a calculus of propositions' while anachronistic is at least not baneful, and it is, in fact, in my view illuminating to use this expression to refer to Stoic logic.

[6] ii.135–136. This work will be referred to in the remainder of the paper as *P.H.*

[7] Thomas has rightly pointed out that the intent here must have been something like "the proposition which is allegedly established by the premises". Otherwise every conclusion would be the conclusion of a valid argument.

[8] *P.H.* ii.140. Sextus reports (*P.H.* ii.97–98) that the 'dogmatists' distinguished three kinds of non-evident objects. Some are absolutely non-evident; these are those which are not of the sort to fall under our apprehension, e.g., that the stars are even in number. Some are on occasion non-evident; these are of a sort to be evident but are made non-evident on occasion by external circumstances, e.g., as a city in which I am not present now is to me. Finally, some are naturally non-evident; these are naturally incapable of falling under our clear apprehension, e.g., that there are imperceptible pores.

[9] I am indebted to John Corcoran for having suggested to me this feature of the distinction.

[10] *Adv. Math.* viii.231. I discuss this theorem below (p. 18) in conjunction with other Stoic rules of inference.

[11] 'When an implication, say $\varphi x . \supset . \psi x$, is said to hold always, i.e. when $(x) : \varphi x . \supset . \psi x$, we shall say that φx *formally implies* ψx; and propositions of the form '$(x) : \varphi x . \supset . x \psi x$' will be said to state *formal implications*.' Alfred N. Whitehead and Bertrand Russell, *Principia Mathematica to *56* (2nd ed., Cambridge Univ. Press, 1964), p. 20. I am not altogether certain that Diodorean implication is a species of formal implication, but it seems to me to wear that aspect, for, as Mates says (p. 45), "'If [Diodorean] it is day, then it is light' holds if and only if, 'If [Philonian] it is day at t, then it is light at t' holds for every value of t". And this appears to me to mean that if a Philonian conditional (material implication) holds always, it is Diodorean true, which is very like Russell's and Whitehead's characterization of formal implication as given above. Since Diodorus holds that what is always true is necessarily true, one might also feel

some temptation to say that Diodorean implication is an ancient version of C.I. Lewis' strict implication (see following note and Mates' remark on this point, p. 47).

[12] "Thus 'p implies q' or 'p strictly implies q' is to mean 'It is false that it is possible that p should be true and q false' or 'The statement 'p is true and q is false' is not self-consistent'. When q is deducible from p, to say 'p is true and q is false' is to assert, implicitly, a contradiction." C.I. Lewis and C.H. Langford, *Symbolic Logic*, 2nd ed., Dover Publ., Inc., New York, 1959, p. 124.

[13] The substantive term used here is 'analysis' (ἡ ἀνά λυσις), 'Sextus Empiricus', *Adv. Math.* viii.229. The verbal term is 'to analyze' (ἀναλύειν), *SVF* II 248.

[14] This is Mates' translation (p. 77) of the passage from Apuleius, *In De Interp.*, 277–278.

[15] This argument is ascribed by Sextus to Aenesidemus (*Adv. Math.* viii.234, 215), but when Sextus goes on to say (*Ibid.* 235), "An argument such as this is composed from a second and third undemonstrated argument, as it is possible to learn from analysis..." we may infer that the analysis he applies is a Stoic analysis.

[16] For other passages in which some of the Stoic argument forms are exhibited, see *SVF* II 952, 1011, and 1012.

BIBLIOGRAPHY

Alexander, *In Aristotelis Analyticorum Priorum Librum I Commentarium*, G. Reimer, Berlin, 1883.

Aristotelis, *Analytica Priora et Posteriora* (ed. by W. D. Ross with Preface and Appendix by L. Minio-Paluello), Clarendon Press, Oxford, 1964.

Arnim, Hans von, *Stoicorum Veterum Fragmenta*, Vol. II., B. G. Teubner, Leipzig, 1903.

Bocheński, I. M., *Ancient Formal Logic*, North-Holland Publ. Co., Amsterdam, 1963.

Cicero, *De Divinatione*, Harvard Univ. Press, Cambridge, Mass., 1959.

Diogenes Laertius, *Vitae*, Book vii, Harvard Univ. Press, Cambridge, Mass., 1950.

Gould, Josiah B., *The Philosophy of Chrysippus*, E. J. Brill (reprinted), Leiden, 1971.

Hurst, Martha, 'Implication in the Fourth Century B.C.', *Mind* **44** (1935), 484–495.

Kneale, William and Kneale, Martha, *The Development of Logic*, The Clarendon Press, Oxford, 1962.

Lewis, C. I. and Langford, C. H., *Symbolic Logic*. 2nd ed. Dover Publications, New York, 1959.

Mates, Benson, *Stoic Logic*, Univ. of California Press, Berkeley, Calif., 1961.

Sextus Empiricus, *Outlines of Pyrrhonism*, Book ii, Harvard Univ. Press, Cambridge, Mass., 1955.

Sextus Empiricus, *Adversus Mathematicos*, Book viii, Harvard Univ. Press, Cambridge, Mass., 1957.

Whitehead, Alfred N. and Russell, Bertrand, *Principia Mathematica to *56*, 2nd ed., Cambridge Univ. Press, 1964.

JOHN CORCORAN

REMARKS ON STOIC DEDUCTION

The purpose of this note is to raise and clarify certain questions concerning deduction in Stoic logic. Despite the fact that the extant corpus of relevant texts is limited, it may nevertheless be possible to answer some of these questions with a considerable degree of certainty. Moreover, with the answers obtained one might be able to narrow the range of possible solutions to other problems concerning Stoic theories of meaning and inference.

The content of this note goes somewhat beyond the comments I made during the discussion of Professor Gould's paper [8], 'Deduction in Stoic Logic', in the symposium. I am grateful to Professors Gould and Kretzmann for pointing out the implications of those comments as well as for encouraging me to prepare them for this volume.

One of the obstacles to a careful discussion of Stoic logic is obscurity of terminology. Clarification of terminology may catalyze recognition of important historical facts. For example, in 1956 a modern logician suggested (incorrectly) in a historical note [4, fn. 529] that the distinction between implication and deduction *could* not have been made before the work of Tarski and Carnap. But once historians had clarified their own terminology it became obvious that this distinction played an important role in logic from the very beginning. Aristotle's distinction between imperfect and perfect syllogisms is a variant of the implication-deduction distinction and Gould [8] suggests the existence of a parallel distinction in Stoic logic.

1. Implication and Inference

Let us clarify our terminology. We use the two-placed verb 'to imply' (*P* implies *c*) to indicate the converse of the logical consequence relation. For us, its subject is always a set of sentences and its object is always a single sentence. For example, we might say that Euclid's Postulates *imply*

J. Corcoran (ed.), Ancient Logic and Its Modern Interpretations, 169–181.

Playfair's Postulate. As is common in ordinary English, we use the three-placed verb 'to infer' to indicate a certain rational action. Thus, we might say that Playfair *inferred* his postulate *from* Euclid's postulates. The subject is always human, the direct object is always a single sentence and the prepositional object is always a set of sentences (but it is sometimes omitted by ellipsis). 'To deduce' is a synonym for 'to infer'.

The more common English usage of 'implies' presupposes that the subject contains only truths. Occasionally a logician has adopted this convention, e.g., Frege [7; pp. 82, 105, 107] and Łukasiewicz [10, p. 55]. When it is not known whether the presupposition obtains, the common usage requires the verb to be put in the subjunctive in order to 'cancel' the presupposition. Thus Frege might say something like the following: the axiom of choice, if true, *would imply* Zorn's lemma. However, in this article the verb 'implies' never carries the presupposition. Our usage reflects Aristotle's fundamental discovery that the logical consequence relation is separable from issues of the material truth of premises. In effect, Aristotle saw that the so-called ground-consequence relation can be analyzed into a property (being 'grounds') and a relation ('implication').

Likewise, 'to infer' is often used with the presupposition that the subject knows that the prepositional object is true. According to this usage we might assert, "if Zorn inferred his lemma from the axiom of choice, he must have known that the axiom of choice is true *and* he must have discovered that the axiom of choice implies his lemma." However, in this article our use of 'to infer' never carries the presupposition. To infer c from P is simply to deduce c from P, i.e., to discover by logical reasoning that P implies c. (Warning: according to this usage 'incorrect inference' is not inference, just as 'false pregnancy' is not pregnancy.)

My opinion, stemming in part from reading Mates' *Stoic Logic* [12], Bury's translation of Sextus' writings [3], Gould [8] and other works, is that the Stoics did use the distinction between implication and inference. Here we come to the first problem.

Problem 1: (a) To explicate the Stoic analogue of the implication-inference distinction. (b) To determine whether the Stoic usage involved presuppositions. (c) To determine whether the Stoics articulated the distinction (which is much more than simply using it). (d) To develop extensive textual support for the answers to the above.

2. Arguments, their Deductions and their Counterinterpretations

According to Gould [8] and others [e.g., 12, p. 58], the Stoics had a technical term (*logos*) which translates exactly into our technical term 'argument' in the sense of a set P of sentences together with c, a single sentence (P is the premise set and c is the conclusion). Our technical term does not agree with common usage in several respects, the most noteworthy of which is that one can produce an argument (technical sense) without engaging in any argumentation (reasoning, inference). To do this one simply specifies a set of sentences together with a single sentence. In the technical sense, arguments never *express* reasoning. In fact, one must engage in reasoning in order to determine the validity of an argument; therefore, the reasoning is not already expressed *in* the argument. An argument (P, c) is *valid* if and only if P implies c, otherwise *invalid*. Another confusion results from the fact that the terms 'premises' and 'conclusion' suggest that someone took the premises as 'his premises' and inferred the conclusion. Resnik [14] and Copi [5] define the term 'argument' in such a way that to call (P, c) an argument is to *presuppose* that someone took the premises as his premises and inferred the conclusion; but, of course, their subsequent usage accords with the definition, which does not make that logically irrelevant presupposition. Another confusion results from the inclination to regard 'argument' as an honorific term and to refuse to count as arguments certain 'bad' arguments (those which are invalid or which have contradictory premises or which include the conclusion among the premises). This confusion is encouraged to some extent by translating Aristotle's term *syllogismos* as 'argument' because for Aristotle all 'syllogisms' are valid; an invalid argument cannot be a 'syllogism' at all (not even an imperfect one). These reflections bring up the second problem.

Problem 2: (a) What were the non-technical uses of the Stoic terms for 'argument', 'premise', 'conclusion' and 'valid'? (b) What were the common connotations of these words? (c) What kinds of confusions were likely to arise in technical usage because of the non-technical connotations? (d) Which of these confusions actually occurred?

To proceed we need to review the well-known asymmetry between the normal mode of establishing validity and the normal mode of establishing invalidity. For example, in *Prior Analytics* (I, 4, 5, 6) in order to establish that an argument (P, c) is *valid*, Aristotle produces a deduction, a list of easy logical steps leading (although not necessarily directly) from P to c and "making clear that the conclusion follows". On the other hand, in order to establish that an argument (P, c) is *invalid*, Aristotle produces a counter interpretation, i.e., he interprets the non-logical terms in such a way as to verify the premises and falsify the conclusion. It's the same in more complicated cases. To establish that Euclid's postulates (and axioms) imply the Pythagorean Theorem, one produces a step-by-step deduction of the latter from the former. To establish that the fifth postulate does not follow from the others one produces a counterinterpretation making the others true and the fifth false.

The asymmetry between Aristotle's method of establishing validity and his method of establishing invalidity is more than just echoed by modern logicians. Tarski, for example, relegates the two methods to separate (but adjacent) sections of his *Introduction to Logic* [16, §36, §37]. After a brief discussion of deduction within an axiomatic framework, Tarski adds [*op. cit.*, p. 119]

More generally, if within logic or mathematics we establish one statement on the basis of others, we refer to this process as a *derivation* or *deduction*...

A few pages later [p. 124], he takes up the problem of showing that a certain sentence does not follow from certain premises. Here he discusses a reinterpretation of the basic terms in a manner that will leave the premises true while making the conclusion false.

Because the dichotomy of methods may not have been emphasized sufficiently in recent literature, it may appear to persist only in a somewhat muted form. However, I think that a case can be made for the historical thesis that what we now call 'proof theory' has its roots in the method of establishing validity whereas what we now call 'model theory' is rooted in the method for establishing invalidity.

Our main concern here is with the Stoic method for establishing validity, but we can still wonder about the Stoic method for establishing invalidity. As far as I have been able to determine, very little has been written about the latter and it may well be the case that the Aristotelian dichotomy was *not* preserved by the Stoics. They may have been concerned only with establishing validity. If this conjecture seems strange we may note that there is nothing about establishing invalidity either in *The Port-Royal Logic* or in Boole's *The Laws of Thought*. Moreover, long before the method of counterinterpretations was *used to establish* the invalidity of the argument from the other postulates of geometry to the parallel postulate, the argument was *widely assumed* to be invalid [cf. 4, p. 328].

Notice that a deduction is a piece of extended discourse consisting of several sentences over and above the premises and conclusion. As an aside we might point out that our term 'lemma' which usually indicates an especially important intermediate line in a long deduction was used by the Stoics to indicate a premise [*loc. cit.*]. As another aside which may be relevant to avoiding confusion we might note that some recent writers have used the terms 'a deduction' and 'an implication' interchangeably, sometimes using 'an implication' to indicate a valid argument (but, of course, for some older writers an implication is just an if-then sentence!).

It is useful to imagine that the deductions and the counterinterpretations all exist prior to being 'produced' so that 'production' is really only exhibition. If this is too much, just imagine that all deductions and all counterinterpretations potentially exist. In any case think of both classes of 'objects' as 'there'. Now we can ask interesting questions about the completeness of the method for establishing validity and about the completeness of the method of establishing invalidity. First, does every valid argument have a corresponding deduction? Second, does every invalid argument have a corresponding counterinterpretation?

Notice that only one of these questions can be trivial. *If* valid *means* having a deduction *then* the first question is trivial but the second is significant. On the other hand, *if valid means* having no counterinterpretations *then* the second question is trivial while the first is significant. Standard practice seems to be to take the latter point of view, i.e., to assume that valid means having no counterinterpretations. The significant question, then, is whether to every argument lacking counter interpretations there corresponds a deduction (to establish its validity). If not, then there are valid arguments whose validity cannot be established.

In any case we are led to consider three large classes: the class of arguments, the class of deductions and the class of interpretations. In the balance of this note we focus on the class of deductions; but, of course, the class of arguments and the class of interpretations are both continually in the background.

3. Aims of theories of deduction

It is unlikely that God gave men language and left it to Aristotle or to the Stoics to invent deductions. When Aristotle began his work there was an extant corpus of deductive discourses and a well-established activity of producing deductions. In fact, historians believe that there were at least two axiomatizations of geometry which existed prior to Aristotle's time.

This situation leaves Aristotle with three options as far as the aim of his theory of deduction is concerned. He could have had a *descriptive* aim or a *prescriptive* aim or a *conventionalistic* aim. That is, roughly, he could have set himself the task of *describing* the class of deductions (by cataloging the rules according to which they had been produced) or he could have *prescribed* the rules which *should* be used to produce ideally 'correct' deductions or he could have *devised* rules which would produce discourses which would serve the same purpose that ordinary deductions serve (viz. establishing that conclusions follow from premise-sets). There seems to be a tendency among mathematicians to assume that the descriptive approach is the dominant one not only in Aristotle but even in modern logic. Bourbaki [2, p. 1], whose foundational writings have been influential, has said,

> Proofs had to exist before the structure of a proof could be logically analyzed; and this analysis, carried out by Aristotle, and again and more deeply by the modern logicians, must have rested then, as it does now, on a large body of mathematical writing.

Indeed, on reading the *Analytics*, it is hard to escape the conclusion that Aristotle's *aim* was descriptive. However, as Mueller [13] has shown, Aristotle's final product fell far short of success *as a descriptive effort* because even the most elementary deductions in Euclid cannot be produced by Aristotle's rules. Here we come to another problem.

Problem 3: (a) To decide whether the Stoic logicians had set themselves descriptive or prescriptive or conventionalistic aims. (b) If the first, to decide whether their 'data' included the mathematical and scientific deductions available to them or whether they restricted their data so as to include only 'philosophical' discourse. If the second, to discover the criterion of correctness used to ground the 'should' of the prescriptions. If the third, to discover the reason they abandoned (or overlooked) the first two goals. (c) In any case to adduce persuasive philological arguments for the above.

4. Sentential and Argumental Systems of Deduction

There are many different *styles* of systems of deduction and it is historically important to know the exact style that the Stoic system exemplified. Here we will characterize two styles which seem relevant to discussion of Stoic deduction. In order to determine the style of the latter it may be necessary for the historian to first construct an exhaustive survey of the extant styles and even then there is no reason to think that the Stoic system will necessarily conform to one of them.

When a person first starts to think about deductions he often conceives of a deduction of c from P as a list of *sentences* beginning with those of P, having intermediate sentences added according to rules and ending with c. A deduction whose 'lines' are all sentences is called a *sentential deduction*. A *direct, linear* sentential deduction is one of the sort described above – one goes from the premises step-by-step directly to the conclusion.

As I have suggested, I think that there is an inclination to think at first that all deductions are direct, linear and sentential. But this would be to overlook the indirect, linear, sentential deductions which proceed from P to c by assuming sentences in P, supposing also 'the denial' of c and then adding immediate inferences until one arrives at a sentence and its own denial. Aristotle's deductive system is a linear sentential system with direct and indirect deductions.

In regard to style the systems of Boole and Hilbert are more primitive than that of Aristotle because their deductions are *all direct and linear*. Systems of direct, linear, sentential deductions can have binary rules (which proceed from two local premises to a local conclusion, e.g. *modus ponens*) unary rules (which proceed from a single local premise to a local conclusion, e.g. universal instantiation) and nullary rules (which need no local premises and produce a local conclusion *ab initio*). Nullary rules are commonly referred to as logical axiom schemes.

In addition to linear rules which proceed from finitely many local premises to a local conclusion, a sentential system can also have suppositional rules which correspond to inference of a local conclusion (not *from* local premises but) *on the basis of* a 'pattern' of reasoning. For example conditionalization can be stated as a suppositional rule which proceeds to a conditional on the basis of a pattern of reasoning from the antecedent to the consequent. Thus the class of sentential deductive systems is quite diverse. It includes systems of direct linear deductions (Boole and Hilbert), systems of direct and indirect linear deductions (Aristotle) and systems of suppositional deductions (Jaskowski, Fitch, etc.). Many (but by no means all) of the so-called natural-deduction systems are sentential (cf. [6, III]).

Opposed to the sentential deductions (which are lists of sentences) there are those which are lists of arguments. Systems which consist entirely of lists of arguments are called *argumental deductive systems*. The systems of Lemmon [9], Suppes [15] and Mates [11] are in this style. In creating an argumental deduction one does not start with premises and proceed to a conclusion but rather one takes *ab initio* certain simple arguments and constructs from them, line-by-line, increasingly complex arguments until the argument with desired premises and conclusion is reached. In argumental systems the rules produce arguments from arguments (not sentences from sentences).

Given a certain minimal clear-headedness about the notion of a deduction, the problem of determining the exact nature of the Stoic deductive system (or systems) emerges. Let us put this down with a little care.

Problem 4: (a) To describe the class(es) of discourses which the Stoic logicians regarded as deductions, i.e., which were taken to establish the validity of arguments. (b) For the (each) Stoic deductive system we need both an exact description of the rules and also an account of how the rules were used to produce extended discourses (deductions).

5. The Stoic Fragments

The main purpose of this section is to review and interpret some of the available information concerning Stoic deduction in order to contribute toward a solution of the problem of discovering the style of the Stoic system.

It has been suggested that the theory of deduction may have been of minor importance in Stoic logic because, since the Stoics had truth-tables, they could establish the validity of arguments by a computational rather than discursive means. Two points are relevant here. First, Mates claims that there is no evidence that the Stoics *used* any computational means for establishing validity. Apparently the fact that truth-functional validity admits of a computational decision procedure, as embarrassingly trivial as it is, had to wait until 1920 to be noticed. Second, the existence of truth table methods should not disguise the fact that validity is always established by a deduction – to compute a truth-table for a truth-functionally valid argument is nothing more (or less) than writing a deduction-by-cases in tabular form.

Incidentally, I find it very difficult to understand how anyone could believe that the Stoics *knew* that their deductive system was complete when there is no evidence that they availed themselves of truth-table methods for establishing validity. Indeed, as has been pointed out elsewhere, if the Stoics had demonstrated completeness then surely they must have worked on the problem and, yet, there seem to be no fragments which admit of interpretation either as deliberation on the problem of demonstrating completeness or as alluding to such deliberation. In my opinion, it is not even clear that the Stoics *believed* their system complete (cf. [12, pp. 81–82]).

(A) *Language*: The Stoics analyzed sentences as truth-functional combinations of atomic sentences using as connectives: the conditional, conjunction, exclusive disjunction, and negation. Here we use $\supset$, &, v and $\sim$.

(B) *Sentential rules*: There were evidently five rules which 'produced' a single sentence from a pair of sentences and it is clear in each case that whenever the operands are true the resultant is true. Thus these five rules *could* serve as immediate *sentential-inference rules* (SIR, plural: SIRs) These can be written as follows:

(SIR1) $p \supset q, p/q,$
(SIR2) $p \supset q, \sim q/\sim p,$
(SIR3) $\sim (p \,\&\, q), p/\sim q,$
(SIR4) $p\text{v}q, p/\sim q,$
(SIR5) $p\text{v}q, \sim q/p.$

(C) *Argumental rules*: There were evidently four rules which produced an argument from a pair of arguments or (in at least one case) from a single argument. It is clear in the three known cases that whenever the operands are valid the resultant is also valid. Thus these rules could serve as immediate *argumental-inference rules* (AIR, plural AIRs). This concept will be discussed below but, for the present, we will write these rules using a symbolic notation. For later reference we will quote the rule before symbolizing it. In symbolizing the argumental rules we use the arrow to separate premises from conclusion and we use the double slant line to separate operands from resultant (just as we used the single slant line to separate operands from resultant in the sentential rules).

(AIR1) *If from two propositions a third is deduced, then either of the two together with the denial of the conclusion yields the denial of the other.*

This evidently gives two subrules.

(AIR1.1) $p, q \to r \,//\, p, \sim r \to \sim q,$
(AIR1.2) $p, q \to r \,//\, \sim r, q \to \sim p.$

Here it should be noted that the Stoics could have been using the term 'the denial' ambiguously to indicate *either* the result of adding a negation to a sentence *or* the result of deleting a negation from a sentence (which stands with a negation). If this is so, one would get sixteen subrules (when r is a negation, when p is a negation and when q is a negation).

(AIR2) *Whenever we have premises from which a certain conclusion can be validly deduced, potentially we have also that conclusion among the premises, even if it is not stated explicitly.*

To symbolize this let S be a set of premises and let $S+p$ be the result of adding p to S.

(AIR2) $S \to p;\ S+p \to r \,//\, S \to r.$

Today this rule is sometimes called 'the cut rule'; but there are other 'cut' rules as well.

(AIR3) *Whenever from two premises a third is deduced and other propositions from which one of the premises is deducible are assumed, then from the other premise and those other propositions the same conclusion will be deducible.*

(AIR3) $p, q \to r;\ S \to p \,//\, q+S \to r.$

This is another 'cut' rule. A modern logician might be baffled by the presence of two cut rules. That the 'force' of (AIR2) is so close to that of (AIR3) causes some speculation concerning the accuracy of the sources.

It is not known what the fourth rule is but it has been alleged that the Stoics 'had conditionalization'. One AIR version of conditionalization can be written as follows.

(AIR4) $S+p \rightarrow q\,//\,S \rightarrow (p \supset q)$.

Incidentally, it is important to distinguish between having a rule of conditionalization and knowing the principle of the corresponding conditional (which is semantic). The latter can be stated: an argument is valid if and only if the corresponding conditional (if 'conjunction-of-premises', then 'conclusion') is logically true. A rule of conditionalization is a rule for constructing deductions whereas the principle of the corresponding conditional is a semantic metatheorem. Obviously one could have either without the other. As far as I have been able to tell the Stoics knew the principle of the corresponding conditional but there is no evidence to indicate that they employed a deductive rule of conditionalization. (Note that the rule of conditionalization does not mention the conjunction connective.)

Another possibility for the fourth rule is one which would permit something like indirect deductions. One way of putting this is as follows.

(AIR5) *A set of premises implies a conclusion if the premises together with the denial of the conclusion imply a contradiction.*

(AIR5) $S+p \rightarrow q, \quad S+p \rightarrow {\sim} q\,//\,S \rightarrow {\sim} p$.

On grounds of common sense one would be inclined to accept the hypothesis that the Stoics had a rule for constructing 'indirect deductions'. However, there seems to be no textual evidence to corroborate that hypothesis.

(D) *The Stoic System*: Because of the existence of the argumental rules it is impossible that the Stoics had a sentential system. On the other hand, a sentential rule can easily be adapted for use as an *ab initio* (nullary) argumental rule. For example, *modus ponens* can be adapted to the following nullary argumental rule.

(AIR5) $//\,p,\ p \supset q \rightarrow q$.

Thus it seems possible that the Stoic system was an argumental system. Taste for simplicity tends toward this conclusion. However, it may have been the case that the argumental rules were thought of as rules for producing sentential rules from sentential rules so that the Stoics had a double-tiered sentential system: a kind of argumental system for producing sentential rules which were then incorporated into a sentential system for producing sentential deductions.

To exemplify the idea of producing sentential rules from sentential rules by means of argumental rules we offer the following.

(SIR6) $p, \sim q / \sim (p \supset q)$ (from (SIR1) by (AIR1.1)),
(SIR7) $\sim \sim q, p / \sim (p \vee q)$ (from (SIR4) by (AIR1.2)).

In order to settle these questions it is necessary to review the extant corpus and isolate all passages which are expressions of deductions. One must then try to discover the kind of rules which would best account for each passage. As far as I can see we still do not know exactly what the rules are because one cannot know what a rule is unless one knows how it is used.

There is a final consideration which may be important. Imagine that a deductive system emerges from a kind of operational conception. For example if we think of a logical consequence of a set of sentences as being somehow 'contained in' the set then we are inclined to view deduction as an operation of 'analyzing' a set of sentences to find out what is 'contained in' it. From this conception the linear, direct, sentential systems emerge (logical axioms will have to be thought of as catalysts which may be added in an analytic process without adding to the 'content' of the set of sentences being analyzed). An argumental system, especially those of Lemmon [9], Mates [11] and Suppes [15], may be seen as emerging from a constructional or synthetic conception; one starts with trivially valid arguments and uses them to synthesize increasingly complex arguments.

According to Mates [12, pp. 64, 77] the Stoics spoke of *analyzing* complex arguments and of *reducing* complex arguments to simple arguments. If this is to be taken literally then we can assume that the Stoics thought of complex arguments as some how 'composed of' simple arguments and that they used the argumental rules backward, so to speak, i.e. that they established the validity of a given argument by first finding simpler arguments which could be synthesized to yield the given argument, then doing the same thing to the simpler arguments, and so on until a set of 'simple arguments' was reached. If this is so then the Stoic 'deductions' were actually tree diagrams fanning out to simpler arguments from the given argument and having simple arguments at the extremities.

This conclusion seems to be compatible (at least) with the evidence that Mates cites but it goes counter to Mates' own conclusion. However, Mates' own account of the Stoic deductive process [12, p. 78] does not involve the argumental rules at all.

State University of New York at Buffalo

BIBLIOGRAPHY

[1] Aristotle, *Prior Analytics.*
[2] Bourbaki, N., 'Foundations of Mathematics for the Working Mathematician', *Journal of Symbolic Logic* **14** (1949), 1–8.
[3] Bury, R. G. (transl.), *Sextus Empiricus*, Vols. 1 and 2, Cambridge, Mass. 1933.
[4] Church, A., *Introduction to Mathematical Logic*, Princeton 1956.
[5] Copi, I., *Symbolic Logic* (2nd ed.), New York 1965.
[6] Corcoran, J., 'Discourse Grammars and the Structure of Mathematical Reasoning, I, II, III', *Journal of Structural Learning* **3** (1972).
[7] Frege, G., *On the Foundations of Geometry and Formal Theories of Arithmetic* (transl. by E. W. Kluge), New Haven 1971.
[8] Gould, J., 'Deduction in Stoic Logic', this volume, p. 151.
[9] Lemmon, E. J., *Beginning Logic*, London 1965.
[10] Łukasiewicz, J., *Aristotle's Syllogistic* (2nd ed.), Oxford 1957.
[11] Mates, B., *Elementary Logic*, New York 1965.
[12] Mates, B., *Stoic Logic*, Berkeley 1961.
[13] Mueller, I., 'Greek Mathematics and Logic', this volume, p. 35.
[14] Resnik, M., *Elementary Logic*, New York 1970.
[15] Suppes, P., *Introduction to Logic*, Princeton 1957.
[16] Tarski, A., *Introduction to Logic* (3rd ed.), New York 1965.

PART FIVE

FINAL SESSION OF THE SYMPOSIUM

JOHN CORCORAN

FUTURE RESEARCH ON ANCIENT THEORIES OF COMMUNICATION AND REASONING

In *Die Meistersinger* one finds some advice which to some extent expresses the general attitude of this symposium. It reads as follows: "If you by rules would measure what doth not with your rules agree, forgetting all your learning, seek ye first what *its* rules may be". It is interesting to reflect on some possible explanations of why it is now possible for us to 'forget all our learning' and seek 'the rules that the ancients had purposed'. Perhaps the most relevant fact is that we now possess a framework rich enough to encompass and categorize many diverse theories of language and reasoning. In the second place, as a result of what must have appeared as 75 years of game-playing, we now have, in reasonably developed form, literally hundreds of possible abstract languages and logics. Consequently, we can now afford to look with an unjaundiced and objective eye at the writings of the ancients. The danger of forcing an ancient theory into a procrustean bed is considerably diminished.

Even though many of us have opinions concerning 'the truth' in some of these matters, many possible interpretations of ancient logic are now so obvious that even the most enthusiastic zealot can see the issues which must be objectively settled in order to establish one interpretation as more plausible than another. For example, prior to the 1950's the idea of a comprehensive logic devoid of anything resembling truth-functions was practically inconceivable. But since then Tarski, Scott, and Kalicki investigated what are now called equational logics, inspired no doubt by the fact that truth-functions play a decidedly minor role in many elementary developments in algebra. In high school, we learned to solve equations without using truth-functions in our schematic discourses. In any case, theoretically possible logics devoid of truth-functions were studied in some detail and found to be sufficiently rich to form underlying logics for a fair amount of scientific activity. In a sense this development made it possible to look at Aristotle without assuming in advance that he must have smuggled truth-functions in somewhere.

Examples like this can be repeated. I do not want to overemphasize

J. Corcoran (ed.), Ancient Logic and Its Modern Interpretations, 185–187.

logic here, but another logical example is too telling to pass over. Prior to 1934, all published logics were developed in the so-called axiomatic framework which was devised by Frege and aped by all informed logicians. In 1934, however, both Gentzen and Jaskowski published logics which were as rich in deductive power as the axiomatic logics, but which had radically different structures. Thereafter it was no longer plausible to assume that if a person had developed a logic then he necessarily had an axiom system. This, of course, opened the path to a new assessment of Aristotle's logic and, predict, to a new assessment of Stoic logic.

Before discussing some of the open problems in the understanding of ancient theories, I would like to undermine an overly narrow construal of our work today. Notice that, in almost all of our expositions of ancient doctrines, the emphasis was on placing those doctrines accurately and objectively within modern settings. To be more specific, most of us were concerned to say, of the things that we know, which of them were already known by the ancients. This, of course, is of great importance, not only for our own understanding of the historical development of our own technical fields, but also because, in order to be part of the cultures of subsequent generations, ancient texts must be reinterpreted from the standpoint of each subsequent generation. The Renaissance interpretation of classical antiquity is hardly relevant to *our* understanding of it. If classical antiquity is now of importance to us, then we must try to relate it to the categories and issues of our own times. To assume that the Renaissance humanists were more accurate than modern classicists because the former were temporally closer to antiquity would be preposterous *and* irrelevant.

However, the above approach to ancient theories overlooks one crucial and potentially valuable possibility: namely, that the ancients had insights, perhaps even fairly well developed theories, which are substantially better than our own views on the same topics. Notice that if some Renaissance figures had understood Aristotle's theory of perfecting syllogisms, then some areas of modern logic could have been developed earlier. (I have in mind natural deduction systems. We could have had them in the late 1800's if people in the Renaissance had already understood that Aristotle had one.) I think that we have a responsibility to make it impossible for future generations to say of us that, for example, had we understood the *Categories*, we would have been able to develop theories of semantics far superior to those that we are now developing. In other words, I think that we must look at the ancients with the hope of finding in them doctrines and ideas which would be substantial contributions to modern linguistics and logic. Perhaps the resolution of the current chaos in modern modal logic will turn on recapturing the meaning of some of the convoluted passages in the Aristotelian corpus. Incidentally, although I can point to no one item, I do feel that my own grasp of logic has been enhanced and broadened by my studies of Aristotle. But I do not recommend aspiring logicians to start there.

In addition to the technical insights which may emerge through modernistic interpretations of ancient logic, we also search for philosophic insights. Attempts to understand ancient theories seem to force us to reconsider the fundamental and enduring questions concerning logic and language. As we all sadly know, successful technical advances have a tendency to engender trains of imitative variations which cloud fundamental issues. Investigation of ancient theories tends to force us to get clearer about what is really important in modern technical developments. It challenges us to clarify the philosophic value of modern achievements. We are invited to ask of modern developments what they can provide, *vis-à-vis* the fundamental questions, that the ancient views could not provide. In the light of modern developments, one is surely refreshed by discovering in Aristotle's works that logic is about reasoning and that linguistics is about the system of communication which seems to distinguish us from animals and make science and history possible. Again we are refreshed to discover that Aristotle saw deduction as objective and natural rather than subjective and contrived. When most logic texts fail to consider that it is humans that produce deductions, and that humans engage in such activity for a reason, Aristotle offers us his modest observation that perfect syllogisms "make plain that the conclusion follows". And of course the refreshment is twofold. We are reminded of the basic motivation for studying logic and linguistics and we are moved to rejoin the centuries-old dialogues on the fundamental issues.

State University of New York of Buffalo

A PANEL DISCUSSION ON FUTURE RESEARCH IN ANCIENT LOGICAL THEORY

Participants in order of appearance:

LYNN ROSE, *SUNY Buffalo* – JOHN CORCORAN, *SUNY Buffalo* – JOSIAH GOULD, *SUNY Albany* – IAN MUELLER, *University of Chicago* – MARY MULHERN, *Swarthmore College* – NEWTON GARVER, *SUNY Buffalo* – WILLIAM PARRY, *SUNY Buffalo* – JOHN GLANVILLE, *California State University (San Francisco)* – JOHN MULHERN, *Bryn Mawr College* – JOHN SWINIARSKI, *Buffalo, New York* – KEITH ICKES, *University of Indiana* – NORMAN KRETZMANN, *Cornell University* – JOHN RICHARDS, *University of Georgia.*

INTRODUCTION

What follows is very nearly a word-for-word transcription of tape recordings of a discussion which took place in the final session of the symposium. The reader will notice a certain spontaneity and liveliness not usually found in scholarly writings. Some of the speakers would want to revise their remarks were they to be published as scholarly *dicta*. Therefore, the reader should take this as a record of free conversation and not as part of the research archives of the history of logic. J.C.

Lynn Rose: One of the topics that has intrigued me for some time is the relationship between Plato and the *Prior Analytics*. I would like to state a view that seems to diverge from some of the things that have been said in the sessions. Prof. Mueller mentioned that the syllogistic logic seems to have been developed independently of Greek mathematics. If my notes are right, I think he also mentioned in passing that it was developed more or less independently of Plato and his School, who had almost no interest in logic. Then later on John Corcoran said that the assertoric logic is extensional where modal logic is somewhat more intensional. I think he connected the intensional aspect of the modal logic with Plato and regarded that approach as an error. It seems to me that the assertoric logic is closer to Plato than the modal logic is. I would agree that Plato's forms are very much intensional rather than extensional, but it seems to me that the project of taking a Platonic position about forms *and* seeking a

J. Corcoran (ed.), Ancient Logic and Its Modern Interpretations, 189–208.

formal logic that is extensional is quite a consistent one and that in modern times people like Russell and Goodman have done these two things at once without any real inconsistency. They sound very much like Plato when they are talking about forms and qualities and universals or whatever and yet their logical position puts a lot of emphasis on an extensional viewpoint.

What I am suggesting is that, in spite of Plato's position about the forms, he was moving toward a formal logic, that the stage had been set for the *Prior Analytics* by Plato. Briefly, I see the *Parmenides* as a first and abortive search for something like a formal logic (which is why the *Parmenides* is a mess) and the method of division as an approach which was somewhat more successful from Plato's own point of view. Whether anything in the *Prior Analytics* is attributable to Plato I'm not sure, but I think he at least set the stage for the *Prior Analytics*. I can't see the *Prior Analytics* springing fully elaborated from the brow of Aristotle or the brow of anyone else. It must have had a history. There must have been a lot of work going on before it got set up in that form. So my suggestion is that maybe it is the other way around. Maybe the first work in logic by Aristotle was on the assertoric syllogistic and that was under Platonic influence. Then the modal logic, which presumably came later, would be more likely to be Aristotle's own work. So, I see the bad part of the *Prior Analytics* as Aristotle's independent work.

John Corcoran: I really see it the other way around, although I don't know Plato as well as you do. You may very well be right, but I think the flavor that one gets out of the two men is that Aristotle takes concrete individuals as being of much more fundamental ontological nature than the universals, and for Plato it's the other way around. In the assertoric logic it's the concrete individuals that seem to be what's important. In the modal logic it's the universals that seem to be the more important. But in connection with the *Parmenides* you may very well be correct.

Josiah Gould: There is a curious passage in the *Phaedo*, the one where it appears that the arguments which had been quite good no longer appear to be good and Socrates warns everybody against misology. And you may remember in that passage he says that just as when a couple of friends let you down you may come to hate all men, so when a couple of arguments let you down you come to hate all arguments. The corrective that is needed is an art of logic (*techne logike*). What's odd is that, I think, it is the only reference to such an art in the whole Platonic corpus. It seems to me that your point is well taken in so far as that kind of remark falling on the mind of a person like Aristotle took seed and grew. I don't see that Plato himself contributed to the development of such an art. What's odd about it is that it was he himself who saw the need.

Lynn Rose: Well, where I'm not at all confident is what the exact relationship between Plato and the *Prior Analytics* is. I am confident that he was moving toward the *Prior Analytics* and that the *Parmenides* represents one effort, the method of division another effort. I see the method of division as leading naturally to the *Prior Analytics*, which is better than division and, of course, better than the *Parmenides*. Almost anything would be.

Ian Mueller: It seems to me that the most crucial thing missing in Plato's notion of a *techne logike* is the concept of form. That seems to be the breakthrough of the *Prior Analytics*. You get some cases in Plato where Socrates says that from 'All *A* are *B*' you can't necessarily infer 'All *B* are *A*'. It's clear that Plato is trying to make a general point. But there is very little in Plato which suggests what John (Corcoran) called a revolutionary idea: that the validity of an argument depends on its form.

John Corcoran: Incidentally, this revolutionary idea isn't *explicitly* stated in Aristotle.

Ian Mueller: No.

John Corcoran: He just uses it over and over again. As I said in my reply to (and, as it turned out, my agreement with) Mary Mulhern, just because someone uses the principle is no grounds for saying that it was part of his theory.

Ian Mueller: Maybe I am changing the subject, but the crux of your discussion with Mary Mulhern seems to me to be the distinction between having a theory and having an isolated insight. To say that Aristole has a propositional logic of some kind just because he states some propositional laws seems improper to me.

Mary Mulhern: Let me point out that I didn't say that he did actually have a logic of propositions. What I wished to point out was that I thought there was evidence for enough insights so that he could have gone on to elaborate a propositional logic if he had been interested to do so. My position was that he was not interested in doing so. He did not elaborate such a logic but elaborated the other one instead.

Ian Mueller: I'd still maintain that the issue arises when you say he could have if he had wanted to. Are you implying that he in some sense had the idea of stating fundamental propositional arguments or axioms and deriving others from them? Are you making that strong a claim? Or is your claim just that there are some rudimentary things in Aristotle which, if he had had a propositional logic, you would have called its roots? Or something weaker?

Mary Mulhern: Well, it seems to me that he is able to handle propositions as units in arguments. He can work with them. He has propositional variables and what not. He can work with that sort of basic unit. That unit didn't have sufficient explanatory power for him so he had no interest in pursuing it further. Although I think it can be shown that he did know how to deal with them, he didn't give us a theory concerning them because he had other fish to fry.

Ian Mueller: Could one draw an analogy with a mathematician who says, "I see I could go into algebra, but algebra is boring, so I'll do something else."? In this case the discipline is already there (I don't mean that it pre-exists) and the mathematician sees that he could develop it, but it doesn't look interesting to him. That seems like an awfully strong claim to make about Aristotle and propositional logic.

John Corcoran: I think that what Mary Mulhern is saying has a firm kernel of truth in it. But it is going to take a lot of pages and a lot of delicate writing to say it in such a way as to be more true than false. I think there are no grounds at all for saying that Aristotle envisaged the possibility of a theory of truth-functional logic on a par with the theory of syllogistic.

Mary Mulhern: How could it be? As you have pointed out there are important differences between them. I don't know that you could begin to treat them as being on a par.

Newton Garver: As I understand your point, Mary (Mulhern), it's a fairly restricted one. In order to combat the criticism that Aristotle's logic is deficient for not having taken account of propositional interests, you claim: well, he makes enough mention of specific propositional inferences that we can suppose that he would have seen this as a deficiency in his system, were it really a deficiency.

John Corcoran: Oh. This seems to support my point.

Newton Garver: No. You're adding a certain amount of sophistication to his logical insight. I take it that Mary's point was that he in fact made use of a certain type of argument. Now that *kind* of argument was needed to *complete* the arguments that he deals with in the syllogistic, so he had enough familiarity with it .So we can now say that he didn't *just* overlook this because he wasn't even familiar with what a propositional inference is at all.

Mary Mulhern: That is what I mean. Łukasiewicz had made the claim that Aristotelian syllogistic presupposed propositional logic as an underlying logic. As an interpretive procedure this was completely cockeyed. He made the further claim that, although it was true that propositional logic was the underlying logic of syllogistic, Aristotle had no idea in the world of this kind of system and that it wasn't invented or thought of or talked about or discussed, no one had an inkling of it until it was developed by the Stoics. Now what John Corcoran showed was that syllogistic is itself a fundamental logical system; it does not presuppose propositional logic as an underlying logic, nor does it presuppose any other logical system as an underlying logic. And he suggested in his paper that it was then gratuitous to speak of Aristotle's ignorance here. But he didn't go on to say what we might speak of. I just followed it up by pointing out what I thought were Aristotle's motives in the matter.

John Corcoran: So your point is mainly that if it were needed he would have easily seen it. Okay. That's a very different kind of a point.

Ian Mueller: A question just occurred to me. Don't you need transitivity of implication for what you do?

John Corcoran: It comes out in the wash. It's not presupposed there. It's there, but not in the form of a law of propositional logic.

William Parry: Actually sometimes Aristotle is much more correct than many modern logicians in refraining from hasty generalizations. Now you see, for instance, the principle of direct reduction is perfectly sound in the syllogism. I mean these arguments (reducing one syllogism to another by direct reduction) are valid.But logicians infer in general that when the conjunction P and Q entails R, the conjuction P and not-R will entail not-Q. They made a hasty generalization and this gives them the paradoxes of strict implication, which, of course, you may swallow if you want to, but if you don't want to you don't have to. Aristotle didn't have to. Direct reduction works perfectly in a syllogism because there any two propositions have all the terms and you never get any novelties. But when you make a general rule out of it then you go *from* 'P and Q entail P' to 'P and not-P entail not-Q'. So you can get the conclusion which is completely irrelevant to the premises. But you can never get that in the use of reduction in the syllogism. So Aristotle, if he thought of it, would have been smart enough not to generalize the propositional logic too hastily from the syllogisms.

I want to make a different point going back to what Lynn Rose was getting at there. Now it seems to me that beginning with assertoric syllogistic is quite consistent with Aristotle's first stages of syllogism being more Platonic, *because* if you are dealing only with forms (and remember, of course, only by way of exception does he use singular terms) you want general terms, of course. And if your general terms are, e.g., men, animals, and stones – the kind of terms he usually uses when he gives counterexamples – then here it would be redundant to bring in questions of necessity or possibility, because this is all in the realm of necessary matter, as the medievals would say. To say that all men are necessarily animals is redundant, if you are talking about the relation of men to animals.

So, starting with forms, Platonic forms shall we say, then there is no necessity for bringing in modality. Everything is either necessary or impossible. If you do bring in real contingencies, and talk about contingent matters, it is then that the distinction becomes pertinent. So, I think that assertoric logic properly comes first in order. Remember for a proposition to be assertoric doesn't mean it isn't necessarily true, of course. I think it is important to distinguish apodictic and necessary. 'All men are animals' is necessary but not apodictic. So I think it is quite natural if he is thinking in Platonic forms or something analogous to them at any rate – his own version of them – that he would begin with the assertoric and only later go on to the modal. When you want to bring both the contingent and necessary into the same system, only then is it necessary to make this distinction.

John Glanville: Contingency makes modal logic necessary, not the Platonic forms.

William Parry: Yes, that's right.

John Corcoran: I think that is going to clear up a lot of problems that I have in interpreting the *Analytics* as a whole. Because the *Posterior Analytics* is obviously a treatise on axiomatic science and in it there is practically no reference (or maybe literally no reference) to modalities.

Your idea would explain why it's not needed even though necessity is an essential aspect of scientific knowledge. That's very interesting.

Josiah Gould: Except I think there is the problem that the demonstrative syllogism is supposed to apply in all the natural sciences. When we start talking about the natural world it turns out that the sentences we use to talk about that world are, for the most part, themselves for-the-most-part sentences. That is, when you say so-and-so is the case for the most part, it is already by definition the kind of sentence that can't be plugged into a demonstrative syllogism.

John Corcoran: I guess I could take this time to say some things about future research. I think one thing that has emerged from this conference is Prof. Mueller's observation that for Aristotle *modus ponens* is not a valid rule of inference. He says that any if-then must be established syllogistically first. So, taking an if-then as a premise is an illegitimate move. He says these things aren't really arguments. I think that in that passage the conclusion that Aristotle is using if-then in the sense of logical consequence is unavoidable. It would be interesting to look at the rest of the corpus and see whether there are any grounds whatever for thinking that Aristotle was aware of the truth-functional use of if-then. It may very well be the case that if-then for him *did* express logical consequence and that only. That would shed more light on why no propositional logic got developed. You can't get off the ground without truth tables. That's one possible piece of future research.

Ian Mueller: I think you can really. The opposite assumption seems to me to have played too great a role in some interpretations of Stoic logic. One could have just five unproved arguments and a few ways for manipulating them without going into the questions of the interpretation of the connectives involved. One could have a logic without interpreting the connectives.

John Corcoran: Just a deductive system without semantics. I think that the only reason that you can think that's possible is because we have just gone through a wave of formalism where people took seriously the idea of having a logic that didn't have any semantics in it. It was just pure manipulation of symbols.

Ian Mueller: The wave itself shows it to be possible.

John Corcoran: Okay. That's a hypothesis that could be investigated.

Maybe the Stoics were really formalists and it was foot-dragging reactionaries that put the truth-tables in.

Ian Mueller: All I mean is that you don't have to have a semantics to have a logic.

John Corcoran: I have something written up here about some problems with the *Categories* and some problems with the *Analytics* and some problems in Stoic logic.

The first thing I want to talk about is a problem in *Categories.* In the *Categories* there are two prominent vertical hierarchies. Namely, the one involving individuals – this man, this plant, and so on – and substantial universals, man, plant and so on. And the other involving instances of qualities – this shape, this color, and so on. And qualitative universals – spherical, green, and so on. These correspond roughly to nouns and adjectives. Alongside these two hierarchies of *Categories* there is another hierarchy which is not in *Categories*; namely, the one involving what we call in our native untutored tongue *substance, mass, matter* or better perhaps, *stuff.* Words for stuff (cheese, water, earth, metal, meat, so on) are called *mass words* by linguists. They behave in some respects like nouns and in other respects like adjectives. In any case, we have an ontologically different category. In their primary senses these words, like adjectives, do not take numerical modifiers. In the primary sense of cheese, we don't talk about several cheeses or one cheese or two cheeses. We always have to say *a piece* of cheese. In other ways, they behave more like nouns. Someone called them mass nouns rather than mass words, while referring to nouns themselves as count nouns, thereby letting on that the former do not admit numerical modifiers as the latter do. To a modern linguist the absence of hierarchy of stuff constitutes an obvious gap in *Categories.* The questions that this situation suggests are many. Does this indicate a *lacuna* in the text? That is, could there have been a category of mass nouns that was completely omitted, one that Aristotle had worked on? Now assuming that it isn't a gap in the text, did Aristotle have some doctrine which 'eliminated' mass or which reduced it to primary being or to quality or to something else? Could you be a reductionist and reduce mass to one of the other categories? How does Aristotle's account of change compare with an account which encompasses the flow of stuff? Now, as you recall, in *Categories* Aristotle's theory of change is that change always occurs in concrete individuals and the way it occurs is by instances of qualities coming into being and passing away *in* the thing. Now this account of change doesn't allow for the flow of stuff through a concrete individual. And as we all know from our own private experiences, mass *does* flow through us. And the scientists tell us that what is our mass today is no part of us in 13 years or something, that our entire bodily substance is replaced by different substance after 13 years. So this common sense observation about change, and also the scientific observation, isn't accounted for by the theory of change in *Categories*. At least the one that's still there. To add that new category gives you a new theory of change and gives you lots of other new things. I'm just suggesting that as future research this be looked into.

Mary Mulhern: I'll send you an off-print. [See Mary Mulhern, 'Types of Process According to Aristotle', *The Monist* **52** (1968), 288–299 (*Editor's note*).]

John Cocoran: You've done this!

Mary Mulhern: Five years ago.

John Mulhern: Also there are problems for you in the Second Book of *Physica*.

John Corcoran: This comes in *Physics*?

John Mulhern: The reduction of substance to matter, which he is not favorably inclined to, in individuals.

John Corcoran: If this is in *Physics*, and the standard chronology is right, then this indicates a change in viewpoint.

John Mulhern: I don't think so.

John Swiniarski: The medievals run across a problem in a slightly different way. Suppose I promise you five pounds of rice out of this barrel of rice. The nominalist would like to analyze it into some definite five pounds of rice in that barrel, but then they have to think of permuting all the grains of rice in the barrel into all possible five pound packets that I might be promising you. But if I promise you five quarts of wine out of my barrel of wine, it's a little more mysterious how I could permute all the molecules of water. What exactly am I promising you? The mass factor there causes a problem in terms of their having a simple analysis. If I promised you one out of ten books, well, it's easy. But if I promise you a certain amount of a mass item, it becomes tricky.

John Corcoran: It's interesting that I can promise you *a* book. Suppose you say, "I'll go to the store for you if you give me a book out of your bookshelf". I say, "Okay, go to the store and I'll give you a book out of my bookshelf". That doesn't imply that there exists a book on the bookshelf that I promised you, does it?

John Swiniarski: There might not be any books on the bookshelf when I leave for the store. There might be many when I get back or there might be one.

John Corcoran: There are apparently two uses of the word 'some', that people who wrote dictionaries noticed but logicians haven't. We need to do something with that too to bring logic up to date with the dictionary.

Keith Ickes: What are those two uses?

John Corcoran: One is what the dictionary calls the indefinite use, and the other is the definite, where 'some book' is a kind of proper name. If I say, "some book is on the desk", I may be saying: "exists x, x is a book and x is on the desk". I could be saying that – I probably wouldn't be. I'm probably referring to that book by the phrase 'some book'. If you were handy I'd say *this* book is on the desk. Some people have speculated that the difference between those two uses of *some* is all the difference between classical logic and intuitionism. Where the intuitionists always use the definite sense. The intuitionists never say *some* unless they can come up with one, whereas in classical logic you can say *some* and you don't have to come up with one. It's getting way off the mass-word problem.

John Swiniarski: I don't remember enough about Greek grammar and syntax. There might be some features of Greek grammar or syntax itself that might obscure the problem of mass nouns or somehow absorb it into the structure.

Mary Mulhern: There's no indefinite article, for instance, in Greek – which gives you a problem with your count nouns. There are some nouns in Greek which can be either count nouns or mass nouns depending on how you use them.

John Corcoran: We have those in English, you know. Like beer and beers.

Lynn Rose: Wire, string, rope. All the lengths.

John Corcoran: In any case, this is an example where something that has been made a big deal of in modern linguistics may be worth using as a category to go back and look at Aristotle.

Mary Mulhern: I think it's from a different analysis though. It wouldn't fit in his scheme of categories. Now certainly an analysis like that is useful, but you couldn't add it in an eleventh category.

John Corcoran: It's interesting that the Aristotelian framework is really aped by Wittgenstein in *Tractatus*. I mean mass nouns can hardly fit in the *Tractatus* either.

Newton Garver: That's not clear. Why not?

John Corcoran: Because facts are just individuals in certain relations.

Newton Garver: So whenever you get a mass noun occurring in a sentence, this has to be built into an understanding that there are certain individuals, a limited selection of them, standing in limited relations. But the objects are entirely abstract. Objects in the *Tractatus* are something of which there are no examples, hence no limitations on what you consider the objects to be or the concatenations of the objects to represent in the way of ordinary sentences.

John Corcoran: I'm embarrassed. I'm just going on flavor. I think that's something we could get into though. How *does* mass fit into the *Tractatus*?

The second set of problems with the *Categories* comes up when you notice that relations don't seem to form a separate vertical hierarchy in the same sense that quality and substance do, but that the relations themselves divide into substantial, qualitative, and massive. What do I mean by that? Well, what's a substantial relation? It's a relation that relates individual substances, like brother, sister, parallel. They are on a par with substantial predicates at a secondary level. So you have individual substances, then you have secondary substances and then you have, in a different direction, substantial relations. You also have qualitative relations – darker, brighter, smoother, more rough, and so on. So over the individual instances of qualities, you have ordinary qualities and then you have qualitative relations. Then you have relations which relate masses – heavier and lighter.

Ian Mueller: It's not clear that these three are distinguished by Aristotle. Besides heavier and lighter could be thought of as relations between two objects: e.g. this object is heavier than that object.

John Corcoran: Okay. How about denser? I'm just saying that such relations might enter the *Categories*.

John Swiniarski: Could you set up a ten point grid and take a look at each category relative to each other category?

John Corcoran: That's another thing I didn't even bring up. But, in addition to the relations that are clearly within a category, you can have cross categorical relations. For example, the relation of being-in between an instance of a quality and an individual would be a relation that wouldn't be in either category but it would be cross categorical. It would be a relation between instances of qualities and primary substances. There are others too, but the others would get too far away from the categories. In fact, the division of the Aristotelian category of relation into substance and quality may be the beginning of a viable doctrine of internal and external relations. In any case, observations along these lines at the very least provide motivation for taking a fresh look at the *Categories.*

Ian Mueller: I would like to add a remark on relations. I haven't found a satisfactory discussion of just what a relation is for Plato or Aristotle. Scheibe's article [*Phronesis* XII (1967)] is a start. It seems to me that this is an open problem which an industrious person with a knowledge of logic could attack and get important results.

Newton Garver: Certainly the chapter on relations is one that strikes a modern reader as most difficult.

John Glanville: Well the distinction that later on is called the distinction between the *secundum esse* and *secundum dici* by the Scholastics, I think, really does come out of Aristotle. The relation 'according to be' is taken to be the relation in one of the categories and the other relation is 'according to be said'. Think back into the Greek what that must represent. It's the 'to be said toward'. When you say, potency is said toward act. This, to me, has always seemed to be the antecedent of what later in British logic gets called the internal relation. But I think that's there already in Aristotle's logic. There's not sufficient reflection on that. What he is primarily reflecting on is the adventitious sort of relation which is external and which he treats in one of the chapters of the *Categories.* But the other thing is there all over the map and it's part of what holds the system together. So that in talking about an accident being in a substance, you wouldn't have to multiply entities here and say that the 'in' is another relation in between the accident and the substance. It's part of what it is to be an accident, to be in a substance. And this would be a feature internal to accident as such.

John Corcoran: I don't know what you mean by another.

John Glanville: The other is substance. Of course, in order for accident to be it has to be in a substance.

John Corcoran: You don't think I'm saying anything like ... well, imagine that God created the world in stages. Could he have put down the individuals and the instances of qualities without putting the relation of 'is in' in? No, I'm not saying that. No, after God created the individuals and the instances of qualities, it was of the nature of the latter to be in the former.

John Glanville: Is that in Aristotle? I think that's your question.

John Corcoran: What I just said I put in the terms of the myth of creation, but the idea behind it, that it's pointing to, I think is in Aristotle. All I was doing is pointing out that the relation is there and that it's a cross-categorical relationship. I wasn't making any ontological hay out of it all, which is what you were thinking of me as doing. Is that right?

John Glanville: No. I was saying that the relation 'according to be said' (or, as it later on gets called, 'the transcendental relation') is there and it doesn't multiply entities. I wasn't supposing that you were multiplying entities either by pointing it out. As a matter of fact, you protected yourself from that by using the modern notion of internal relation. So I didn't misunderstand you.

Newton Garver: The question of whether Aristotle would allow this as a relation is something that needs to be worked out.

John Glanville: It's older than Aristotle. It's in Plato. The original *pros ti* is an internal relation. What's new in Aristotle is a category of relation.

Newton Garver: Yes, but he talks about a bird having a wing and says that we shouldn't consider the wing as something that's related to the bird, because that would be to misspeak, that the relation is not between the wing and the bird but rather between the wing and winged-thing or something like that. So what he does is to insist that for every relation you have to have a correlative. Exactly this point about not allowing the bird to be the correlative of the wing is not entirely clear. This is something that needs more research.

Ian Mueller: I'd like to ask another question about relations. Galen talks about relational arguments, relational syllogisms. Most of them do involve relations, but one that he incudes is this argument from the Stoics: 'It is day; you say that it is day; therefore you speak the truth', or 'You say that it is day; you speak the truth; therefore it is day'. I was wondering if anyone has any idea why such an argument is classed as relational. My view is that the classification is accidental. Galen coined the word 'relational' to cover a whole class of arguments which originally had another name. All the other arguments he calls relational do turn on relations like double or equal, but this one doesn't. From our point of view it turns on the semantic notion of truth.

Norman Kretzmann: The one is between what it is you say and the way the world is. The other is between the speaker and what he says. Certainly both of these are picked out in Aristotle as relations. Certainly the relation, between what is said and the way the world is, is picked out, but I don't know if it is categorized. It is discussed. A terminology is built up for the thing that is said, but I can't recall any place where there's a discussion of a relation between the sayer and what is said. It looks as if it's easy to import enough stuff to make that relational in one of two ways, but whether those are Aristotelian relations or not, I don't know.

John Corcoran: I have two classes of problems with the *Analytics* that I would like to mention. The most obvious open problem in the *Analytics* is to give the exact nature of the theory of perfecting of modal syllogisms. Assuming that my interpretation is correct, the general framework of doing this is already down. That is, we have the general outline of what a perfect syllogism is. It's going to be a generalization of what I've done, if I'm right. The problem is to add the rules of perfecting the modal syllogisms. The other Aristotle scholars here can correct me if there is disagreement, but I think there is wide agreement that there are at least two, if not maybe as many as five, *different* modal systems there, all incompatible on a superficial level. So that there are going to be different kinds of necessity. So perhaps the most fruitul approach is to try to ferret out as many different semantic notions of necessity as possible and then to concoct systems of perfect syllogisms according to those semantic ideas. And then to go back and see how they fit with the text, try to develop these things to cover as much of the text as possible. You may say "Aha, that's all very nice but one problem is that if what you have already said is right, it's going to be a natural deduction system, but all the modal systems that have been so far worked out are either axiomatic deduction systems or else are Gentzen-type systems, neither of which fits the Aristotelian framework". That's not exactly true. There is a modal logic which was worked out by Weaver and me in *Notre Dame Journal of Formal Logic,* June, 1969, that has a natural deduction version of S5 which could easily be carried over to the Aristotelian framework. One of the main rules is that if all your premises are modal and you get a conclusion then you can add as the next conclusion the necessity of that conclusion. That rule is almost certainly one of the rules in Aristotle. So the framework for doing this investigation of the modal logic is already there and it's a question of doing the dirty work.

Ian Mueller: I wonder how much really turns on the difference between natural deduction and axiomatics. It seems to me that if somebody carried out the investigation in terms of a regular axiomatic deductive system, the problem of translating the result into the natural deductive system might not be so great.

John Corcoran: Well for Aristotle there weren't any connectives. There's no way of translating it.

Ian Mueller: But just think of the relation between your work and Łukasiewicz's. Your seeing the incorrectness of Łukasiewicz's interpretation of Aristotle's syllogistic is an important insight. But given this insight, the adjustments of Łukasiewicz's work required to get a correct interpretation are largely technical. If you have a lot of modal apparatus in an axiomatic system it might be preferable to use the system to attack Aristotle's modal logic. I don't know. For getting the basics right I'm not sure that the difference between natural deduction and an axiomatic system is going to be crucial.

John Richards: A large part of it is intent. More, I think, than the final result is the intent that was originally there.

Ian Mueller: Ultimately you want to get it exactly right. But using hammers seems to me a good way to get at things. Later one can start chopping away with lighter mallets.

John Corcoran: The natural thing really is the lighter mallet, if you work with it.

Ian Mueller: Perhaps it makes a difference. I was suggesting that I don't see the differences between natural deduction and axiomatics in broad structure but in finer points.

John Corcoran: To get this you have to write on the blackboard a lot. It's a fact that we do reason, and it's also fact that we don't reason axiomatically. The natural deduction systems are called natural because they jibe more with our normal way of doing business than the axiomatic systems do. So the defter tool is going to be the natural deduction approach.

Ian Mueller: Well, that's what seems to me not to follow. It doesn't follow from the fact that we naturally reason in a certain way that for a certain purpose it doesn't help to represent reasoning in another way.

John Corcoran: Okay. Here the purpose is understanding what the Aristotelian system is and, if our general *modus operandi* is closer to Aristotle to begin with, it will be easier to say what the differences are than if we have a *modus operandi* that's very far away. Then we will always have all kinds of fiddlings to do to move back and forth.

Ian Mueller: Let me make one more analogy. Then we'll drop the topic, or you can reply to me. It seems to me that the distance between what one understands after reading Łukasiewicz and what one understands after reading Maier is a much greater and a much more important gap to close than the distance between what one understands after reading Corcoran and after reading Łukasiewicz. I would be willing to say that if someone has the axiomatic apparatus he should use it rather than develop an alternative apparatus.

Lynn Rose: Storrs McCall has several different systems of modal logic which he says can all be found in the *Prior Analytics*, but they're not *consistent*. I was wondering if that could be just what you want.

John Corcoran: He could have the key ideas.

John Swiniarski: There seems to me to be one approach that in a weird way correlates with what Dr. Parry was saying earlier. When you gave the brief summary of your system, you seem to put all the predicates on a par, so to speak. But if it's a science we are talking about, one of those predicates that enters must be the supreme genus of the science and also some of them must be special insofar as they are divisions away from that supreme genus in accordance with the proper rules of divisions. So the predicates that are going to enter into your whole machinery already have a certain ordering among them. Now it might be the case that, once you go through the ordering and use Aristotle's rules of definition and proper division and organize your predicates, then you can make any distinction between which propositions have to do with necessary matter and which propositions have to do with contingent matter. Of course, you still presuppose almost that you do have all the data of the science in. You might be able to somehow work at it from that angle.

John Corcoran: That kind of thing can be another step to take after my thing. Something that can be incorporated in it. You can take a complete theory and then extract out of it a hierarchy of this sort by looking at the forms of the true sentences of the theory. Those true sentences will induce a hierachy of predicates. That outlook may explain some of the chapters in the Book II of *Prior Analytics.*

Okay, so that's the problem with doing the modal logic. There's another batch of problems too which comes from the fact that Aristotle actually was the first proof-theorist. He set down several metatheorems about the system and the ones I've been able to figure out are all true. They were not only true but one was important in getting the metatheoretic results about the system that I got. In one place where I got myself in a bind, I was trying to prove a certain theorem and I couldn't. I worked backwards from the theorem and got to a lemma that I had to get. Then I worked forward and got to a lemma that was very close to the one that I needed. Then I showed it to another guy and he pulled out a line from Aristotle which he used to link up the two and fill the gap. So Aristotle was doing some very heavy proof-theoretic thinking. The problem is to go through Book A and Book B of *Prior Analytics,* to figure out what those metatheorems are and to figure out what Aristotle's proofs of them were. So that's another batch of problems. I should mention Smiley in this regard. He has gotten some of them out already. His work is very interesting, but there is still a lot left to be done. (See Timothy Smiley's 'What is a Syllogism?', *Journal of Philosophical Logic* **2** (1973), ed.)

Ian Mueller: Is there a proof of a metatheorem in Aristotle?

John Corcoran: Yes. There's the proof that the direct proofs can be thrown out.

Ian Mueller: But that's the one you said doesn't really hold together.

John Corcoran: That one does hold together. The one that shows, in effect, that you can use just the universal rules to perfect all of the two-premise syllogisms, that holds also. The completeness proof doesn't. The general theorem that the whole system is equivalent to the one with just the universal rules, that one doesn't go through either. But I imagne there is a lot to learn about my version of Aristotle's system that can be gotten out of Aristotle. That's another batch of problems.

Norman Kretzmann: I don't see the line in Aristotle that links your two lemmas as evidence that Aristotle was deeply into proof-theoretic work. I'm not sure that I understand the situation described very clearly. The fact that something that Aristotle says enabled you to hook these two up doesn't suggest to me that there's anything like the same approach to this juncture in Aristotle as you were taking when you arrived at that point and your friend luckily supplied the link.

John Corcoran: Yes, I don't expect that the few vague things that I've said should be convincing on that at all.

Norman Kretzmann: But you have evidence of a different sort.

John Corcoran: Yes, you can read the details in my article and see the kinds of arguments I have.

Lynn Rose: How can these metatheorems that you mention fail?

John Corcoran: One problem is that Aristotle doesn't allow nested *reductio* strategy. It's very subtle. If you wrote the proof down it wouldn't be at all obvious that you needed to presuppose that you need nested *reductio* strategy to make it go through. But then when you try to put down the details then you see they do presuppose it. For example, he patches perfect syllogisms together. He knows if you can get from here to here and you can get from here to here then you can get a perfect syllogism that goes all the way down, but it could be that you used *reductio* in both of them. If you used it in one or the other then you could put the one that you used it in first and have the whole thing be an indirect proof. But you can't patch two indirect proofs together and still get a perfect syllogism, because you may have only one *reductio* in the whole thing. He lays down at several places that you can have only one. So that's one of the main problems. There is also a certain vagueness in Aristotle about what you really have to show in order to prove that these things work. That vagueness may be partly or largely in my reading of Aristotle. It may also be partly or largely in the translators who were completely oblivious to these possiblities. The things I'm saying I by no means regard as definite or established.

Well, the last problem that I have is the one I already raised after Josiah Gould's talk: what style deductive system did the Stoics have? (Cf. John Corcoran, 'Remarks on Stoic Deduction', this volume, p. 169, ed.)

Ian Mueller: I have another kind of hard-working problem for an industrious person with a knowledge of logic and Greek, or for another kind of industrious person. Actually there are two ways to make Alexander of Aphrodisias's commentary on the *Prior Analytics* accessible to students of the history of logic. One is to translate the work and perhaps produce a study of it as a whole. The other is to go to libraries and destroy its indices; someone else will then do the translation because he can no longer use the indices to find the passages he needs.

John Swiniarski: There was one general point that came to my mind in the discussion earlier today. I remember Wittgenstein made a comment someplace that introduction of a symbol into mathematical logic is a momentous event. I don't remember the context, but in that context it expressed an important point. But that kind of speech, I think, sometimes leads a person to have a too respectful attitude toward the notational devices that do exist. Someone who's deeply involved with these notational devices seems to get to a point where there is an almost playful attitude toward the different notations and different devices. Do you think logic is taught to philosophy students and undergraduates with that kind of an attitude? I know it's dangerous to try to teach that kind of attitude at too low a level, but it seems to me sometimes that maybe a person remains chained to the particular types of symbolism and conventions that they happen to learn when they go through their logic courses.

John Corcoran: The same problem of having to teach virtue. If you do good acts in front of the learner maybe he can figure out what the principle is. You can teach a logical system to the students, and if what they learn is a whole bunch of symbol manipulations, then they will be worse off for it. If they learn what the person who devised the whole thing was up to and what he was trying to do with it, then maybe they will be able to adapt those purposes to other problems and not be wedded to the particular formalism that they got started with. But, *how* you teach that, I have no idea. I think one way of doing it is to teach a couple different symbolisms which are to some extent incompatible, that give genuinely different analyses of the same material. To try to inculcate a respectful attitude toward the problem of whether one is more correct than the other.

Ian Mueller: Have you ever tried that? My experience has been that teaching different symbolisms has the opposite effect. The student comes to think that notation is essential and that logic is nothing but notation. I almost agree with what you said before. I don't think a person is worse off for having learned only symbolic manipulation but that he is hardly any better off.

Newton Garver: It's very difficult. I noticed that one of the big stumbling blocks in theoretical physics is that you have different notations for statistical mechanics, using one to deal with certain problems and using another with other problems.

Norman Kretzmann: Well, that was a feature of a logic book I did and it seemed to me to work pretty well. At any rate, when I taught from it I did it with the intention of cutting the language loose from notation and cutting the operations loose from the notation and every time I switched notation I brought the previous one in and showed how it could be adapted to do the next job, but then dropped it and went on to a new one. They seemed to come out much more sophisticated with regard to the marks than they do with a single straight-line development.

Ian Mueller: But was that just Polish and *Principia* notation?

Norman Kretzmann: No, it was also an adaption of Łukasiewicz notation for traditional logic and, well, it attempted, in every one of the different branches that I dealt with in that text, to show how there was a choice between notations and what differences could result in the choice.

John Glanville: There was one thing about where John Mulhern started that I'd like to mention. Of course, he was talking about the application of modern symbolic techniques. But he really did, in effect, start his pre-Aristotelian logic by telling about Plato, although he referred in a word or two, I suppose, back to the possibilities of something before that. There is continental work on Zeno and Parmenides and other pre-Platonic philosophy that seems to me to need whatever light we can throw on it. Perhaps, symbolic techniques would help.

Ian Mueller: I want to push the post-Aristotelians. I have the feeling that modern logic is too heavy an apparatus to get a great deal more out of Aristotle, Chrysippus, and their predecessors in terms of logical theory. I think one can use modern logic very nicely to analyze particular arguments, e.g. in the Platonic dialogues, but such analyses do not yield the conclusion that the author of the arguments was a logician. On the other hand, Alexander's commentary on the *Prior Analytics* may well be a goldmine for the history of logic. Somebody who is less lazy than I ought to go to work on it.

INDEX OF NAMES

SYNTHESE HISTORICAL LIBRARY

Texts and Studies
in the History of Logic and Philosophy

1. M. T. BEONIO-BROCCHIERI FUMAGALLI, *The Logic of Abelard.* Translated from the Italian. 1969, IX + 101 pp.
2. GOTTFRIED WILHELM LEIBNITZ, *Philosophical Papers and Letters.* A selection translated and edited, with an introduction, by Leroy E. Loemker. 1969, XII + 736 pp.
3. ERNST MALLY, *Logische Schriften,* ed. by Karl Wolf and Paul Weingartner. 1971, X + 340 pp.
4. LEWIS WHITE BECK (ed.), *Proceedings of the Third International Kant Congress.* 1972, XI + 718 pp.
5. BERNARD BOLZANO, *Theory of Science,* ed. by Jan Berg. 1973, XV + 398 pp.
6. J. M. E. MORAVCSIK (ed.), *Patterns in Plato's Thought. Papers arising out of the 1971 West Coast Greek Philosophy Conference.* 1973, VIII + 212 pp.
7. NABIL SHEHABY, *The Propositional Logic of Avicenna: A Translation from al-Shifā': al-Qiyās,* with Introduction, Commentary and Glossary. 1973, XIII + 296 pp.
8. DESMOND PAUL HENRY, *Commentary on De Grammatico: The Historical-Logical Dimensions of a Dialogue of St. Anselm's.* 1974, IX + 345 pp.
9. JOHN CORCORAN, *Ancient Logic and Its Modern Interpretations.* 1974, X + 208 pp,

SYNTHESE LIBRARY

Monographs on Epistemology, Logic, Methodology,
Philosophy of Science, Sociology of Science and of Knowledge, and on the
Mathematical Methods of Social and Behavioral Sciences

1. J. M. BOCHEŃSKI, *A Precis of Mathematical Logic*. 1959, X + 100 pp.
2. P. L. GUIRAUD, *Problèmes et méthodes de la statistique linguistique*. 1960, VI + 146 pp.
3. HANS FREUDENTHAL (ed.), *The Concept and the Role of the Model in Mathematics and Natural and Social Sciences. Proceedings of a Colloquium held at Utrecht, The Netherlands, January 1960*. 1961, VI + 194 pp.
4. EVERT W. BETH, *Formal Methods. An Introduction to Symbolic Logic and the Study of Effective Operations in Arithmetic and Logic*. 1962, XIV + 170 pp.
5. B. H. KAZEMIER and D. VUYSJE (eds.), *Logic and Language. Studies dedicated to Professor Rudolf Carnap on the Occasion of his Seventieth Birthday*. 1962, VI + 256 pp.
6. MARX W. WARTOFSKY (ed.), *Proceedings of the Boston Colloquium for the Philosophy of Science, 1961–1962*, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and Marx W. Wartofsky), Volume I. 1963, VIII + 212 pp.
7. A. A. ZINOV'EV, *Philosophical Problems of Many-Valued Logic*. 1963, XIV + 155 pp.
8. GEORGES GURVITCH, *The Spectrum of Social Time*. 1964, XXVI + 152 pp.
9. PAUL LORENZEN, *Formal Logic*. 1965, VIII + 123 pp.
10. ROBERT S. COHEN and MARX W. WARTOFSKY (eds.), *In Honor of Philipp Frank*, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and Marx W. Wartofsky), Volume II. 1965, XXXIV + 475 pp.
11. EVERT W. BETH, *Mathematical Thought. An Introduction to the Philosophy of Mathematics*. 1965, XII + 208 pp.
12. EVERT W. BETH and JEAN PIAGET, *Mathematical Epistemology and Psychology*. 1966, XXII + 326 pp.
13. GUIDO KÜNG, *Ontology and the Logistic Analysis of Language. An Enquiry into the Contemporary Views on Universals*. 1967, XI + 210 pp.
14. ROBERT S. COHEN and MARX W. WARTOFSKY (eds.), *Proceedings of the Boston Colloquium for the Philosophy of Science 1964–1966, in Memory of Norwood Russell Hanson*, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and Marx W. Wartofsky), Volume III. 1967, XLIX + 489 pp.
15. C. D. BROAD, *Induction, Probability, and Causation. Selected Papers*. 1968, XI + 296 pp.

16. Günther Patzig, *Aristotle's Theory of the Syllogism. A Logical-Philosophical Study of Book A of the Prior Analytics.* 1968, XVII + 215 pp.
17. Nicholas Rescher, *Topics in Philosophical Logic.* 1968, XIV + 347 pp.
18. Robert S. Cohen and Marx W. Wartofsky (eds.), *Proceedings of the Boston Colloquium for the Philosophy of Science 1966–1968*, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and Marx W. Wartofsky), Volume IV. 1969, VIII + 537 pp.
19. Robert S. Cohen and Marx W. Wartofsky (eds.), *Proceedings of the Boston Colloquium for the Philosophy of Science 1966–1968*, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and Marx W. Wartofsky), Volume V. 1969, VIII + 482 pp.
20. J. W. Davis, D. J. Hockney, and W. K. Wilson (eds.), *Philosophical Logic.* 1969, VIII + 277 pp.
21. D. Davidson and J. Hintikka (eds.), *Words and Objections: Essays on the Work of W. V. Quine.* 1969, VIII + 366 pp.
22. Patrick Suppes, *Studies in the Methodology and Foundations of Science. Selected Papers from 1911 to 1969.* 1969, XII + 473 pp.
23. Jaakko Hintikka, *Models for Modalities. Selected Essays.* 1969, IX + 220 pp.
24. Nicholas Rescher *et al.* (eds.). *Essay in Honor of Carl G. Hempel. A Tribute on the Occasion of his Sixty-Fifth Birthday.* 1969, VII + 272 pp.
25. P. V. Tavanec (ed.), *Problems of the Logic of Scientific Knowledge.* 1969, XII + 429 pp.
26. Marshall Swain (ed.), *Induction, Acceptance, and Rational Belief.* 1970, VII + 232 pp.
27. Robert S. Cohen and Raymond J. Seeger (eds.), *Ernst Mach; Physicist and Philosopher*, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and Marx W. Wartofsky), Volume VI. 1970, VIII + 295 pp.
28. Jaakko Hintikka and Patrick Suppes, *Information and Inference.* 1970, X + 336 pp.
29. Karel Lambert, *Philosophical Problems in Logic. Some Recent Developments.* 1970, VII + 176 pp.
30. Rolf A. Eberle, *Nominalistic Systems.* 1970, IX + 217 pp.
31. Paul Weingartner and Gerhard Zecha (eds.), *Induction, Physics, and Ethics, Proceedings and Discussions of the 1968 Salzburg Colloquium in the Philosophy of Science.* 1970, X + 382 pp.
32. Evert W. Beth, *Aspects of Modern Logic.* 1970, XI + 176 pp.
33. Risto Hilpinen (ed.), *Deontic Logic: Introductory and Systematic Readings.* 1971, VII + 182 pp.
34. Jean-Louis Krivine, *Introduction to Axiomatic Set Theory.* 1971, VII + 98 pp.
35. Joseph D. Sneed, *The Logical Structure of Mathematical Physics.* 1971, XV + 311 pp.
36. Carl R. Kordig, *The Justification of Scientific Change.* 1971, XIV + 119 pp.
37. Milič Čapek, *Bergson and Modern Physics*, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and Marx W. Wartofsky), Volume VII. 1971, XV + 414 pp.
38. Norwood Russell Hanson, *What I do not Believe, and other Essays*, ed. by Stephen Toulmin and Harry Woolf. 1971, XII + 390 pp.
39. Roger C. Buck and Robert S. Cohen (eds.), *PSA 1970. In Memory of Rudolf Carnap*, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and

Marx W. Wartofsky), Volume VIII. 1971, LXVI + 615 pp. Also available as a paperback.

40. DONALD DAVIDSON and GILBERT HARMAN (eds.), *Semantics of Natural Language*. 1972, X + 769 pp. Also available as a paperback.
41. YEHOSUA BAR-HILLEL (ed.), *Pragmatics of Natural Languages*. 1971, VII + 231 pp.
42. SÖREN STENLUND, *Combinators, λ-Terms and Proof Theory*. 1972, 184 pp.
43. MARTIN STRAUSS, *Modern Physics and Its Philosophy. Selected Papers in the Logic, History, and Philosophy of Science*. 1972, X + 297 pp.
44. MARIO BUNGE, *Method, Model and Matter*. 1973, VII + 196 pp.
45. MARIO BUNGE, *Philosophy of Physics*. 1973, IX + 248 pp.
46. A. A. ZINOV'EV, *Foundations of the Logical Theory of Scientific Knowledge (Complex Logic)*, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and Marx W. Wartofsky), Volume IX. Revised and enlarged English edition with an appendix, by G. A. Smirnov, E. A. Sidorenka, A. M. Fedina, and L. A. Bobrova. 1973, XXII + 301 pp. Also available as a paperback.
47. LADISLAV TONDL, *Scientific Procedures*, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and Marx W. Wartofsky), Volume X. 1973, XII + 268 pp. Also available as a paperback.
48. NORWOOD RUSSELL HANSON, *Constellations and Conjectures*, ed. by Willard C. Humphreys, Jr. 1973, X + 282 pp.
49. K. J. J. HINTIKKA, J. M. E. MORAVCSIK, and P. SUPPES (eds.), *Approaches to Natural Language. Proceedings of the 1970 Stanford Workshop on Grammar and Semantics*. 1973, VIII + 526 pp. Also available as a paperback.
50. MARIO BUNGE (ed.), *Exact Philosophy – Problems, Tools, and Goals*. 1973, X + 214 pp.
51. RADU J. BOGDAN and ILKKA NIINILUOTO (eds.), *Logic, Language, and Probability*. A selection of papers contributed to Sections IV, VI, and XI of the Fourth International Congress for Logic, Methodology, and Philosophy of Science, Bucharest, September 1971. 1973, X + 323 pp.
52. GLENN PEARCE and PATRICK MAYNARD (eds.), *Conceptual Change*. 1973, XII + 282 pp.
53. ILKKA NIINILUOTO and RAIMO TUOMELA, *Theoretical Concepts and Hypothetico-Inductive Inference*. 1973, VII + 264 pp.
54. ROLAND FRAÏSSÉ, *Course of Mathematical Logic* – Volume I: *Relation and Logical Formula*. 1973, XVI + 186 pp. Also available as a paperback.
55. ADOLF GRÜNBAUM, *Philosophical Problems of Space and Time*. Second, enlarged edition, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and Marx W. Wartofsky), Volume XII. 1973, XXIII + 884 pp. Also available as a paperback.
56. PATRICK SUPPES (ed.), *Space, Time, and Geometry*. 1973, XI + 424 pp.
57. HANS KELSEN, *Essays in Legal and Moral Philosophy*, selected and introduced by Ota Weinberger. 1973, XXVIII + 300 pp.
59. ROBERT S. COHEN and MARX W. WARTOFSKY (eds.), *Logical and Epistemological Studies in Contemporary Physics*, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and Marx W. Wartofsky), Volume XIII. 1973, VIII + 462 pp. Also available as a paperback.

In Preparation

58. R. J. SEEGER and ROBERT S. COHEN (eds.), *Philosophical Foundations of*

Science, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and Marx W. Wartofsky), Volume XI.

60. ROBERT S. COHEN and MARX W. WARTOFSKY (eds.), *Methodological and Historical Essays in the Natural and Social Sciences. Proceedings of the Boston Colloquium for the Philosophy of Science, 1969–1972*, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and Marx W. Wartofsky), Volume XIV.
61. ROBERT S. COHEN and MARX W. WARTOFSKY (eds.), *Scientific, Historical, and Political Essays in Honor of Dirk J. Struik*, Boston Studies in the Philosophy of Science (ed. by Robert S. Cohen and Marx W. Wartofsky), Volume XV.
62. KAZIMIERZ AJDUKIEWICZ, *Pragmatic Logic*, transl. from the Polish by Olgierd Wojtasiewicz.
63. SÖREN STENLUND (ed.), *Logical Theory and Semantic Analysis. Essays Dedicated to Stig Kanger on his Fiftieth Birthday.*