CONTRIBUTIONS TO LOGIC AND METHODOLOGY IN HONOR OF J. M. BOCHEŃSKI

EDITOR

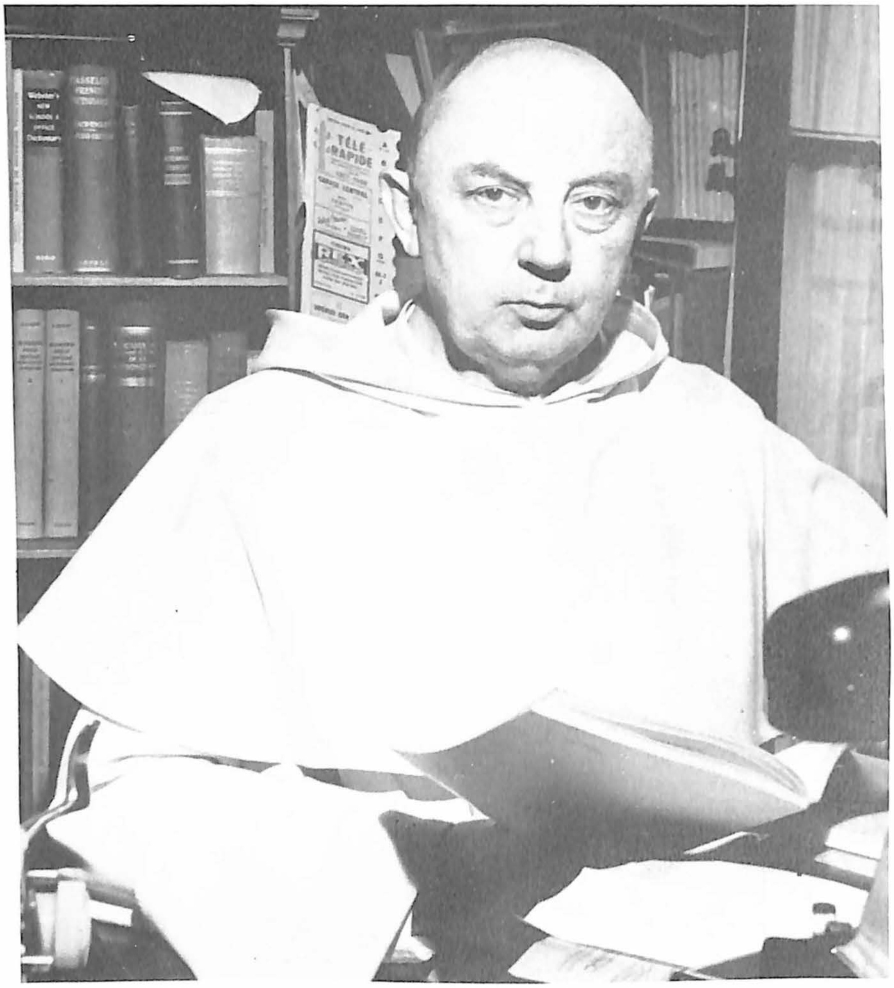
A.-T. TYMIENIECKA

IN COLLABORATION WITH

CHARLES PARSONS

NORTH-HOLLAND PUBLISHING COMPANY

CONTRIBUTIONS TO LOGIC AND METHODOLOGY
IN HONOR OF
J. M. BOCHEŃSKI



J. M. Bochenski

CONTRIBUTIONS TO LOGIC AND METHODOLOGY IN HONOR OF J. M. BOCHEŃSKI

edited by

ANNA-TERESA TYMIENIECKA

in collaboration with

CHARLES PARSONS

1965

NORTH-HOLLAND PUBLISHING COMPANY – AMSTERDAM

PRINTED IN THE NETHERLANDS

PREFACE

As colleagues or former students we wish to show our esteem and friendship for J. M. Bocheński on the occasion of his 60th birthday. We cannot enumerate here what we owe him in several fields of philosophical scholarship, or for his pioneering work in new areas of research, or for his inspiring teaching or for his leadership in scholarly enterprise.

A bibliography will be found on page IX of this book, but it should, in any case, be incomplete for we hope he will be able to give us the benefits of his fertile mind for many years to come. And while our tokens of appreciation have the cold impersonality of learned articles we honor him not only as a scholar, but also as a man of character and a warm-hearted friend.

The editor takes this opportunity to thank those whose cooperation made the publication of this book possible, especially the Committee which raised the necessary funds. The kind assistance of the late Professor Evert W. Beth in introducing the volume to its publisher is remembered with gratitude.

A.-T. T.

ACKNOWLEDGEMENTS

Publication of this volume was made possible by the generous support of the following individual and organizations:

Mr. Julian Godlewski
Direction de l'Instruction Publique du Canton de Fribourg, Suisse
Conseil de l'Université de Fribourg, Suisse
Faculté des Lettres de l'Université de Fribourg, Suisse
Académie Internationale de Philosophie des Sciences
Société Suisse de Philosophie
Société Philosophique de Fribourg, Suisse

The undersigned, constituting the Committee of Organization, hereby express their gratitude for this support.

NORBERT LUYTEN, O.P., *chairman*
ANNA-TERESA TYMIENIECKA
HENDRIK S. HOUTHAKKER
ALPHONSE BRONARSKI

CONTENTS

SCIENTIFIC PUBLICATIONS

of

J. M. BOCHEŃSKI, O.P.

1932 1. *Die Lehre vom Ding an sich bei Straszewski* (Studia Gnesnensia III), Lwów, Ks. Sw. Wojciecha w Poznaniu, 1932, 106 pp. 8°. (I)

2. *Z psychologii różańca* (= Sur la psychologie du Rosaire), Szkoła Chrystusowa **4** (1932) 228–239.

3. *Comptes rendus*, Bulletin Thomiste **9** (1932):
N. 304, p. 397: Kowalski, Fil. Cieszkowskiego.
N. 315, p. 401–5: Salamucha, Pojęcie dedukcji.
N. 321, p. 408: Wais, Kosmologia szczegółowa.
N. 342, p. 418: Czyrnek, Wpływ duszy na ciało.
N. 425, p. 455: Chechelski, Noetyka św. Tomasza.
N. 426, p. 455: Rubczyński, Zróżnicowanie.

1933 1. *La métaphysique et la logique moderne*, ex: Sbornik Mézinarodnich tomistickych konferenci w Praze 1932, ed. M. Haban O.P. (Olomouc 1933) 153–159.

2. *Kongres filozofii tomistycznej w Pradze*, (= Le Congrès de philosophie thomiste à Prague). Aten. Kapł. **185** (1933) 80–87, 183–189.

3. *Z ruchu tomistycznego* (= Chronique du mouvement thomiste), Aten. Kapł. **32** (1933) 313–318.

4. *Filosofija u Polskoj* (= La philosophie en Pologne) Hrvatska Prosvjeta. **20** (1933).

1934 1. *Z ruchu tomistycznego* (= Chronique du mouvement thomiste) (2: princ. de causalité), Aten. Kapł. **33** (1934) 75–79.

2. id. (3. Congrès de Poznań), ibid. 414–424.
3. id. (4. épistémologie) ibid. 494–50.
4. *Studie o pojetie a zasade pricinosti*, Filosoficka Revue **6** (1934) 19–24, 55–60.
5. *Logistique et logique classique*, Bulletin Thomiste **10** (1934) 240–248.
6. *Comptes rendus*, Bull. Thomiste **4** (1934) NN. 424–426, pp. 303–304 (Chojnacki).

1935 1. *Zagadnienie przyczynowości u neoscholastyków* (= Le problème de la causalité chez les néoscolastiques), Przeglad Filozoficzny **38** (1935) 125–134 et tiré à p. Warszawa 1935.
2. *Duae consequentiae Stephani de Monte*, Angelicum **13** (1935) 397–399.
3. *Z ruchu tomistycznego*, (= Chronique du mouvement thomiste) (5. La philos. chrétienne), Aten. Kapł. **36** (1935) 211–217
4. *Comptes rendus*, Bulletin Thomiste **4** (1935) N. 935, p. 601–603; J. Salamucha, Dowód ex motu.
5. *Comptes-rendus*, Angelicum **12** (1935):
   p. 428: Tymczak, Nauka Sw. Tomasza o wierze.
   p. 436: Menger, Moral, Wille u. Weltgestalt.
   p. 437: Krise u. Neuaufb. d. exakt. Naturwissen.
   p. 439 s.: Erdey, Synops. phil. schol. I. Logica.
6. *Dowód przyczynowy na istnienie Boga w świetle Wiary katolickiej* (= La démonstration causale de l'existence de Dieu à la lumière de la Foi catholique).

1936 1. *De cognitione existentiae Dei per viam causalitatis relate ad fidem catholicam* (Studia Gnesnensia 14, Dział teologiczny 7) Posnaniae 1936, 8°, XVI+244.
2. *Notiones historiae logicae formalis*, Angelicum **13** (1936).
3. *W sprawie uprzystępnienia Św. Tomasza* (= Pour rendre S. Thomas accessible).
4. *Elementa Logicae Graecae*, Romae, Anonima Libraria Cattolica Italiana (1937) 122 p.

1937 1. *Notes historiques sur les propositions modales,* Rev. d. sciences Philos. et Théol. **36** (1937), 674–692.

2. *Tradycja myśli katolickiej a ścisłość* (=La tradition de la pensée catholique et l'exactitude) ex: Myśl katolicka wobec logiki współczesnej (Studia Gnesnensiana 15), Poznań 1937, 27–34.

3. *O "relatywiźmie" logistycznym,* (= Du "relativisme" logistique), ibid. 87–111.

1938 1. *De consequentiis scholasticorum earumque origine,* Angelicum **15** (1938) 92–109.

2. *Z historii logiki zdań modalnych,* (Etudes d'histoire de la logique modale), Lwów, O.O. Dominikanie, 1938, 47 pp.

3. *Nove Lezioni di Logica Simbolica,* Roma Angelicum, 1938, 184 pp.

4. *Powszechniki jako treści cech w filozofii św. Tomasza z. Akwinu* (= Les universaux comme contenus des prédicats dans la philosophie de S. Thomas d'Aquin), Przegl. Filozoficzny **41** (1938) 136–149.

5. *Compte-rendu,* Ateneum Kapl. **41** (1938) 85–94 (Studia Philosophica), Lwów 1935.

1939 1. *La logique de Théophraste,* Collectanea Logica **1** (1939) 195–304. (Le ms. et le tirage brulé en 1939 par les bombes allemandes).
Rec.: H. Scholz, Ztschr. f. Mathem. u. ihre Grenzgeb. **22** (1940) 290 s.: Jb. üb. die Fortschritte d. Mathem. **65** (1939) (ed. 1941) 22s.

2. *Założenia etyki wojskowej,* (= Les fondements de l'éthique militaire) Rozkaz wewnętrzny Biskupa Polowego W.P. 5/39 (1939) 77–106.

3. *Teoria męstwa* (= Théorie de la force), ibid. 7/39, 121–148.

4. *Teoria prawości żołnierskiej,* (= Théorie de la justice militaire) ibid.

5. *Pius XI,* Polski Przegląd Tomistyczny **1** (1939) 97–103.

6. *Zespół definicji do metalogicznego wykładu sylogistyki tradycyjnej* (= Ensemble de définitions pour un exposé

métalogique de la syllogistique traditionnelle), Polski Przegląd Tomist. **1** (1939).

7. *Compte-rendu*: Polski Przegląd Tomistyczny **1** (1939) 187–192: K. Ajdukiewicz, Propedeutyka (Rédacteur avec le P. A. Gurowski O.P.).
8. *Polski Przegląd Tomistyczny* **1** (1939) trois fascicules parus (Revue Thomiste Polonaise).

1940 1. *Sancti Thomae Aquinatis de modalibus opusculum et doctrina*, Angelicum **17** (1940) 180–218 et tiré à p. Romae 1945, 39 pp.

1946 1. *Textus logicales selecti... editi a.Dni MCMXXXV, quos textus notis et proemio auctos typis mandavit Fr. Thomas M. Romero Gross...* Quiti, Typographia Sti Dominici, 1946, 18 × 13 cm, 133 pp. (Res olim pro manuscripto edita, nunc sine editoris prioris licentia a P. Romero typis data).

1947 1. *La philosophie*, ex: Pologne 1919-1939, Neuchatel, Ed. de la Baconnière, III (1947) 229–260.

2. *La logique de Théophraste* (Collectanea Friburgensia XXXII), Fribourg, Librairie de l'Université, 1947, 27 × 17½ cm, 139 + 1 pp.
3. *Petri Hispani Summulae Logicales quas e codice manu scripto Reg. Lat.* 1205 *e.d I. M. Bochenski O.P.*, ... Taurini, Marietti, 1947, 22 × 15½ cm, XXIV + 144 pp.
4. *Europäische Philosophie der Gegenwart*, Bern, A. Francke A.G. (Sammlung Dalp 50), 1947, 17 × 11 cm, 304 pp.
5. *Besprechung* von: J. P. Mullaly, The Summulae Logicales of Peter of Spain, 1946, in: Divus Thomas (CH) **25** (1947) 361–363.
6. *Le déchainement des forces laïques* (avec G. de Plinval) ex: Histoire illustrée de l'Eglise, ed. G. de Plinval et R. Pittet, vol. II. Fasc. XVI Genève 1947 (pp. 213–260 de I.M.B.).

1948 1. *On the categorical syllogism*, in: Dominican Studies, Oxford **1** (1948) 35–57.

2. *On Analogy*, in: The Thomist **11** (1948) 424–447.
3. *Philosophy in Switzerland during and after the War*, in: The New Scholasticism **22** (1948) 440–443.
4. *X Congresso Internazionale di Filosofia di Amsterdam*, in: Sapienza **1** (1948) 470–472.

1949 1. *Allgemeine Philosophische Bibliographie*, *ex*: Bibliographische Einführungen (1949.2) I. 1. Bern 1948, 42 SS. (mit F. Monteleone).
2. *Bibliographische Einführungen in das Studium der Philosophie* (unter Mitwirkung von R. B. Winn, E. W. Beth, R. Jolivet, O. Gigon, J. de Menasce, M. F. Sciacca, M. D. Philippe und K. Dürr) hrsg. von I.M.B. Band I, Bern 1948, 11 Hefte, 411 SS.
3. *Réflexions sur l'évolution de la philosophie*, in: Studia Philosophica (Basel) **7** (1948) 12–24.
4. *L'état et les besoins de l'histoire de la logique formelle*, ex: Proceedings of the Xth International Congress of Philosophy, I, (Amsterdam 1949) (fasc. II), 1062–4.
5. *ABC Tomizmu*, London 1949/50, 83 str.
6. *Précis de logique mathématique*, Collection Synthèse 2 (Bussum/Pays-Bas 1949), 90 pp.
7. *O metodzie teologii w świetle logiki formalnej* (Sur la méthode de la théologie à la lumière de la logique formelle). Collectanea theologica, (Varsoviae) **21** (1949) 171–192.
8. *Logical remarks on A-propositions*, in: Dominican Studies **2** (1949) 249–254.

1950 1. *Der sowjetrussische dialektische Materialismus.* (Bern 1950), 213 SS.
2. *Bibliographische Einführungen in das Studium der Philosophie*, hrsg. (unter Mitwirkung von O. Gigon, P. Wyser, F. van Steenberghen, O. Perler, G. Vajda und C. Regamey) von I.M.B., Bd. II. 10 Hefte, 452 SS. Bern 1950.

1951 1. *Non-analytical laws and rules in Aristotle*, in: Methodos **3** (1951) 70–80.

2. *Europäische Philosophie der Gegenwart*, 2. umg. A. (Bern 1951) 323 SS.
3. *Ancient Formal Logic*. (Amsterdam 1951), X + 122 pp.
4. *Rev. of*: Arthur Pap. Elements of Analytic Philosophy. New York 1949, in: The New Scholasticism **25** (1951) 4, 472–476.
5. *Filosofia actual* (trad. ex. 1947.4). 2 ed. (Breviarios No. 16) Mexico C., Fondo de Cultura Economica, 1951, 340 pp.

1952 1. *Bóg we filozofii J. P. Sartre'a*. In: Duszpasterz Polski Zagranicą (Rzym) **3** (1952) 4, (13), 470–479.
2. *Rev. of*. D. H. H. Ingalls: Materials for the Study of Navya Nyāya Logic. In: The Journal of Symbolic Logic **17** (1952) 117–119.
3. *Geschiedenis der Hedendaagse Europese Wijsbegeerte*. trad. (ex 1947.4) M. Duchateau. Bruges, Desclée De Brouwer 1952, 330 pp.

1953 1. *Philosophie et Sciences*. In: Studia Philosophica (Basel) **13** (1953) 11–18 Idem ut: *Relations de la philosophie aux sciences* in: Atti del IX Convegno di studi filosofici tra professori universitari, Gallarate 1953. Brescia, Morelliana, 1953 102–103.

1954 1. *Die zeitgenössischen Denkmethoden*. (Dalp. Taschenbücher, Band 304), Bern, Francke Verlag (1954), 151 SS.
2. *Spitzfindigkeit*. Ex: Festgabe an die Schweizerkatholiken. Fribourg, 1954, 334-352.
3. *Rez. von:* L. Wittgenstein: Tractatus Logico-Philosophicus...a cura di G.C. Colombo S.J., Milano Roma 1954, In: Freiburger Zeitschrift for Philosophie und Theologie **1** (1954) 316-317.

1955 1. *Grundriss der Logistik*. Aus dem Französischen übersetzt, neu bearbeitet und erweitert von Dr. A. Menne, Paderborn, Ferdinand Schöningh, 1954, 124 SS.
2. *Lettre*. Ex: Proceedings of the Royal Irish Academy. 57 A 6 (November 1955) p. 65.

3. *Die Entwicklung der formalen Logik.* Ex: Wissenschaft und Kultursystem, hrsg. v. G. Eisermann (Festschr. f. Alexander Rüstow). Erlenbach-Zürich u. Stuttgart, 1955. 61–75.

1956 1. *Gedanken zur mathematisch-logischen Analyse der Analogie.* In: Studium Generale **9** (1956) 3, 121–125.
2. *Die kommunistische Ideologie und die Würde, Freiheit und Gleichheit der Menschen im Sinne des Grundgesetzes für die Bundesrepublik Deutschland vom* 13.5.1949. In: Aus Politik und Zeitgeschichte. Beilage zur Wochenzeitung "Das Parlament", B. VI/56,/8.II.1956/, 77–95.
3. *idem* (Schriftenreihe der Bundeszentrale f. Heimatdienst) Bonn, 1956, 75 SS. (Dasselbe in: KPD-Prozess Dokumentarwerk, 3 Bd. hrsg. v. G. Pfeiffer u. H. G. Strickert, Karlsruhe, Verl. C. F. Müller 1956, 509–549, als: Gutachten des Professors Dr. Dr. I. M. Bochenski, Freiburg, Schweiz).
4. *Śp. Jan Łukasiewicz.* In: Duszpasterz Polski Zagranica **7** (1956) 3, 28, 200–202. Przedr. in: "Życie" (Londyn) nr 33/477, 1956.
5. 1956.2 mit neuer Vorrede. Ebda.
6. *Formale Logik.* (Orbis Academicus. Problemgeschichten der Wissenschaften in Dokumenten und Darstellungen), Freiburg in Br., Verl. Karl Alber, 1956, XV + 640 SS.
7. *Scholastic and Aristotelian Logic.* In: Proceedings of the American Catholic Philosophical Association **30** (1956) 112–117.
8. *Gendai no yoroppatetsugaku* (= trad. ex 1947.4). Tokyo, Iwanami Gendai Sōsho (1955) 314 + 66 + 5 pp.

1957 1. *Der sowjetrussische dialektische Materialismus. (Diamat)* Zweite, umgearbeitete Auflage. L. Lehman Verl. München, A. Francke Verl. Bern, 1956 (ersch. 1957) (Dalp Taschenbücher, Band 325) 151 SS.
2. *The Problem of Universals.* Ex: The Problem of Universals. A symposium. I.M.B. Alonzo Church, Nelson

Goodman, University of Notre Dame Press. 1956, 33–54 (publ. 1957).

3. *Los metodos actuales del pensamento.* Madrid, Ediciones Rialp SA. 1957, 266 + 5 pp. (trad. 1954.1).
4. *Handbuch des Weltkommunismus.* Hersg. v. J. M. B. u. G. Niemeyer. In: Aus Politik u. Zeitgeschichte B XXV/57 (3.VII.1957) bis XXXX/57 (16.X.1957) SS. 383–660.
5. *Die formale Struktur des Kommunismus. ibid.* SS. 385–388.
6. *Philosophische, soziologische und wirtschaftstheoretische Grundlagen* (mit G. Niemeyer u. E. Walter). *ibid.* SS. 389–404.
7. *Die Religion. ibid.* SS. 629–644.
8. *Zur Kritik des Kommunismus. ibid.* SS. 660–664.

1958
1. *Handbuch des Weltkommunismus.* (Buchausgabe mit Bibliogr. von 1957.4). Freiburg/München 1958, XVI + 762 SS.
2. *On Communist Ethics.* In: The Columbia Review (Fribourg) 43, (April) 1958, pp. 17–22.
3. *El materialismo dialettico* (Spanische Ueb. von 1957.1) mit n. Vorrede). Madrid, Realp, (s.a. 1958), 267 pp.
4. *Probleme der gegenwärtigen Logik.* In: Deutsche Universitätszeitung. **13** 1958 (2) S. 74–79.

1959
1. *Logisch-philosophische Studien.* Mit Aufsätzen von P. Banks, A. Menne u. I. Thomas. Ueb. u. hrsg. v. Albert Menne. Freiburg/München, Alber, 1959, 152 SS.
2. *Bibliographie der sowjetischen Philosophie* I. Die "Voprosy Filosofii" 1947–1956. (Nr.) 1–906. Freiburg/Schw. Osteuropa Institut. 1959. VII + 73 SS.
3. *Einführung in die sowjetische Philosophie der Gegenwart.* In: Aus Politik u. Zeitgeschichte. B. 45/59(4. XI 1959) SS. 593–617.
4. *Die dogmatischen Grundlagen der sowjetischen Philosophie (Stand 1958).* Dordrecht, Reidel, 1959. XII + 84 SS.
5. *A precis of mathematical logic.* Transl. ... by O. Bird (from 1949.6). Dordrecht, Reidel, 1959. X + 100 pp.

6. *Kommunismus.* Ex: Staatslexikon. Freiburg, Herder. Bd. 6, 1959, Koll. 1170–1199.
7. *Bibliographie der sowjetischen Philosophie 2. Bücher 1947-1956. Bücher und Aufsätze 1957-1958. Namenverzeichnis 1946-1958.* (Nr. 1000–2303.) Dordrecht, Reidel, 1959. III + 109 pp.
8. *Wege zum philosophischen Denken. Einführung in die Grundbegriffe.* Basel (Freiburg) Wien. Herder, 1959. 125 SS.
9. *Histoire de la logique formelle.* Ex: Philosophy in Mid-Century. ed R. Klibansky. Firenze, Nuova Italia, 1959. pp. 3–11.

1960 1. *Vorrede.* Ex: S. Müller-Markus: Einstein und die Sowjetphilosophie I. Dordrecht, Reidel, 1960. SS. XI–XIII.

1961 1. (Edit. with Th. J. Blakeley:) *Studies in Soviet Thought* I. Dordrecht, Reidel, 1961, IX + 141 pp.
2. *On Soviet Studies.* In 1961.1, pp. 1–11.
3. *Soviet Logic.* In 1961.1, pp. 29–38.
4. *A History of Formal Logic.* Transl. (of 1956.6) and edited by I. Thomas. Notre Dame Un. Press 1961, XXII + 557 pp.
5. *Diretrizes de pensamento filosofico.* Trad. Prof. Alfred Simon. Sao Paolo, Herder, 1961, 119 pp.
6. *Logik.* Ex: Lexikon f. Theologie und Kirche Freiburg, Herder. Bd. 1961, Koll. 1116–1118.
7. *Logistik.* ib. Koll. 1118f.

1962 1. *Preface.* Ex: G. Küng: Bibliography of Soviet Work in the Field of Logic and the Foundations of Mathematics, from 1917 to 1957. Notre Dame Journal of Formal Logic **3** (1962) pp. 1–4.
2. *Handbook on Communism* Engl. Translation of 1958.1). New York, Prager, 1962. XII + 686.
3. *The Three Components of Communist Ideology.* In: Studies in Soviet Thought, 2, 1962, 7–11.
4. (Review of) A. Buchholz: Kampf um die bessere Welt.

Stuttgart 1961. In: Studies in Soviet Thought 2, 1962, pp. 139–142.

5. *Preface* (to:) A.-T. Tymieniecka: *Phenomenology and Science in Contemporary European Thought*, New York Noonday, 1962, pp. VII–XI.

CHAPTER I

BETRACHTUNGEN ZUM SEQUENZEN-KALKUL

P. BERNAYS

1 Von den Folgerungsaxiomen zum Sequenzen-Kalkul

Unter den Formen der mathematischen Behandlung der deduktiven Aussagenlogik ist der Sequenzen-Kalkul dadurch gekennzeichnet, dass der Gesichtspunkt der Folgerung nicht nur die axiomatische Anlage bestimmt, sondern dass die abzuleitenden Ausdrücke selbst Darstellungen von Folgerungsbeziehungen sind. Der Sequenzen-Kalkul kann so eingeführt werden, dass man ausgeht von den Axiomen der *Folgerungsmenge*, welche von ALFRED TARSKI aufgestellt wurden,[1] und an diesen eine Elementarisierung vornimmt. Jene Axiome beziehen sich auf einen Gesamtbereich S von Aussagen, welche abstrakt als Individuen genommen werden. Einer Teilmenge X von S wird ihre Folgerungsmenge $F(X)$ zugeordnet (die Menge der aus X nach einem gewissen Regelsystem zu gewinnenden Folgerungen).

Die Axiome lassen sich in drei Arten gliedern:

(1) Axiome, welche Mächtigkeiten betreffen; sie besagen:
- a) dass der Bereich S höchstens abzählbar ist,
- b) dass jede Folgerung aus einer Menge X, d.h. jedes Element von $F(X)$, zur Folgerungsmenge mindestens einer *endlichen* Teilmenge von X gehört;

(2) Axiome, welche die Operation der Bildung der Folgerungsmenge als eine Art der ergänzenden Abschliessung, eine Art Hüllen-Operation kennzeichnen;

[1] Vgl. die Abhandlung: Ueber einige fundamentalen Begriffe der Metamathematik. Comptes Rendus de Varsovie **23** (1930) Classe III, S. 2. Siehe auch: Fundamentale Begriffe der Methodologie der deduktiven Wissenschaften I. Monatsh. Math. Phys. **37** (1930).

(3) ein Axiom, das die Existenz eines Elements postuliert, aus welchem jede Aussage von S gefolgert werden kann (im Sinne der Regel "ex falso quodlibet").

Wir wollen hier direkt nur die *Axiome der zweiten Art* heranziehen, des Axiom 1.b) aber implicite in dem Sinne, dass wir speziell Folgerungen aus endlich vielen Prämissen betrachten. Lassen wir zunächst diese Beschränkung weg, so besteht der erste Schritt der Elementarisierung darin, dass wir anstelle der Funktion F die Beziehung $a \varepsilon F(X)$ ("a ist Element der Folgerungsmenge von X") betrachten, die wir in der Form $X \to a$ darstellen.

Die Axiome der zweiten Art für die Funktion F besagen, dass für beliebige Teilmengen X, Y von S gilt:

a) X ist Teilmenge von $F(X)$ und $F(X)$ Teilmenge von S;

b) wenn X Teilmenge von Y ist, so ist $F(X)$ Teilmenge von $F(Y)$;

c) $F(F(X))$ ist Teilmenge von $F(X)$.[2]

Diese Axiome sprechen sich mittels der Beziehung $X \to a$, – wenn von vornherein die Dingvariablen a, b auf Elemente von S, die Mengenvariablen X, Y auf Teilmengen von S bezogen werden – folgendermassen aus:

(1) Wenn a Element von X ist, so $X \to a$.

(2) Wenn $X \to a$ und X Teilmenge von Y ist, so $Y \to a$.

(3) Wenn $Y \to a$ und wenn Y die Menge der b, ist, für welche $X \to b$, dann $X \to a$.

(2) und (3) lassen sich, aufgrund von (1), durch folgende Forderung ersetzen:

(4) Wenn $Y \to a$ und wenn für jedes b von Y gilt $X \to b$, dann $X \to a$.

Die Forderungen (1), (4) sind also gleichwertig mit (1), (2), (3).

Hiermit ist der erste Schritt zur Elementarisierung vollzogen. Der zweite Schritt besteht darin, dass wir speziell Folgerungen aus *endlichen* Teilmengen X von S betrachten, die durch Aufzählung

[2] Die Fassung dieser Axiome entspricht derjenigen bei KARL SCHRÖTER. Vgl. Ein allgemeiner Kalkülbegriff. Forschungen zur Logik ... Neue Folge, Heft 6 (Leipzig 1941), S. 31–33. Bei Tarski wird das zweite dieser Axiome mit der Forderung 1.b in ein Axiom zusammengefasst, welches besagt: $F(X)$ ist die Vereinigung der Mengen $F(U)$, die den endlichen Teilmengen U von X zugehören.

ihrer Elemente gegeben sind. Die aus den Elementen $a_1, \ldots, a_n$ bestehende Menge werde mit $\{a_1, \ldots, a_n\}$ bezeichnet. Die betrachteten Folgerungsbeziehungen stellen sich dann dar in der Form

$$\{a_1, \ldots, a_n\} \rightarrow b,$$

und die Forderungen (1), (4) ergeben die Regeln:

(R₁) Es gilt stets $\{a_1, \ldots, a_n\} \rightarrow a_i \qquad (i = 1, \ldots, n)$.

(R₂) Gelten die Beziehungen $\{a_1, \ldots, a_n\} \rightarrow b_1, \ldots, \{a_1, \ldots, a_n\} \rightarrow b_m$, sowie auch $\{b_1, \ldots, b_m\} \rightarrow c$, so gilt auch $\{a_1, \ldots, a_n\} \rightarrow c$.

Wir können uns hier von den geschweiften Klammern befreien und entsprechend inhaltlich uns eine Menge $a_1, \ldots, a_n$ durch die *Folge* der aufgezählten Elemente gegeben denken. Dabei ergibt sich durch Anwendung der (nun ohne geschweiften Klammern zu benutzenden) Regeln (R_1), (R_2) von selbst, dass für das Bestehen einer Beziehung $a_1, \ldots, a_n \rightarrow b$ die Reihenfolge der Elemente $a_1, \ldots, a_n$ sowie auch die wiederholte Aufzählung ein und desselben Elementes von keinem Einfluss ist. (D.h. von zwei Sequenzen die sich nur in solcher Weise voneinander unterscheiden, ist die eine aus der anderen mittels der Regeln (R_1), (R_2) ableitbar.)

Hiermit kommen wir auf eine Art von Kalkul, wie er wohl zuerst von PAUL HERTZ in seiner Theorie der "Satzsysteme" behandelt worden ist,[3] und für welchen ausser der folgerungstheoretischen Deutung auch verschiedene andere Interpretationen in Betracht kommen. Z.B. kann die Beziehung $a_1, \ldots, a_n \rightarrow b$ gedeutet werden als die Behauptung: "Wenn die Umstände $a_1, \ldots, a_n$ vorliegen, so liegt auch der Umstand b vor", oder auch als die folgende: "Alle Dinge welche die Eigenschaften $a_1, \ldots, a_n$ besitzen, besitzen auch die Eigenschaft b". Auch für diese beiden Deutungen ist die Gültigkeit der Regeln (R_1), (R_2) ersichtlich.

Der abstrakte Rahmen der Theorie ist der folgende. Man hat einen Elementen-Bereich E. Es werden "Sätze" der Form $a_1, \ldots, a_n \rightarrow b$ betrachtet, worin $a_1, \ldots, a_n, b$ Elemente aus E sind. Die Folge

[3] Siehe: Ueber Axiomensysteme für beliebige Satz-Systeme. Math. Annalen **89** (1923) und **101** (1929).

$a_1, \ldots, a_n$ heisst das "Antecedens", b das "Succedens". Eine Menge von Sätzen der betrachteten Form heisst ein *Satz-System*, und ein Satz-System heisst *abgeschlossen*, wenn es durch Anwendung der Regeln (R_1), (R_2) nicht erweitert werden kann, d.h. wenn es erstens alle solchen Sätze enthält, bei dem das Succedens mit einem Element des Antecedens übereinstimmt ("trivale Sätze"), und wenn es stets, sofern es die Sätze $a_1, \ldots, a_n \to b_1, \ldots, a_1 \ldots a_n \to b_m$ und $b_1, \ldots b_m \to c$ enthält, auch den Satz $a_1 \ldots a_n \to c$ enthält.

Eine Teilmenge T eines abgeschlossenen Satz-Systems S heisst ein Axiomen-System für S wenn jeder Satz von S sich aus Sätzen von T durch Anwendung der Regeln (R_1), (R_2) gewinnen lässt.

Für die Definition der Abgeschlossenheit und des Axiomen-Systems können die Regeln (R_1), (R_2) durch andere, ihnen gleichwertige Regel-Systeme ersetzt werden.[4] Tatsächlich wurden von Paul Hertz nicht diese Regeln gewählt, vielmehr ist das von ihm benutzte Begriffs- und Regel-System das folgende: Unter den trivialen Sätzen werden die "tautologischen" Sätze, d.h. die von der Form $a \to a$ als Ausgangssätze ausgezeichnet. Als "unmittelbarer Schluss" wird der Uebergang von einem Satz zu einem solchen bezeichnet, der aus ihm durch Hinzufügung von Elementen zum Antecedens entsteht. Und als "Syllogismus" wird die folgende Schlussfigur (R_2^*) erklärt:

$$\frac{\begin{array}{l} a_{11} \ldots a_{1n_1} \to b_1 \\ a_{21} \ldots a_{2n_2} \to b_2 \\ \vdots \\ a_{m1} \ldots a_{mn_m} \to b_m \\ \qquad\qquad b_1 \ldots b_m, a_1 \ldots a_k \to c \end{array}}{a_{11} \ldots a_{1n_1}, a_{21} \ldots a_{2n_2}, \ldots a_{m1} \ldots a_{mn_m}, a_1 \ldots a_k \to c}$$

Nunmehr heisst ein Satz-System abgeschlossen, wenn es erstens alle tautologischen Sätze enthält und wenn es ferner durch Anwendung von unmittelbaren Schlüssen und Syllogismen nicht erweitert werden kann. Und ein Teilsystem T eines abgeschlossenen

[4] Die Auszeichnung der Regeln (R_1), (R_2) findet sich in der Abhandlung von KARL POPPER, New Foundations for Logic, Mind **56** (1947) N.S. No. 223, sowie bei PAUL LORENZEN, *Einführung in die operative Logik und Mathematik* (1955) § 6, S. 41–42.

Satz-Systems S heisst ein Axiomen-System für S, falls jeder Satz aus S, sofern er nicht schon zu T gehört oder tautologisch ist, aus Sätzen von T und tautologischen Sätzen mittels unmittelbarer Schlüsse und Syllogismen gewonnen werden kann. Eine solche Ableitung des Satzes heisst ein "Beweis" des Satzes aus dem Axiomensystem T.

Man überlegt sich ohne Schwierigkeit, dass diese Definitionen der Abgeschlossenheit und des Axiomen-Systems mit den vorherigen gleichwertig sind, insbesondere, dass der unmittelbare Schluss und der Syllogismus sich auf Anwendungen der Regeln (R_1), (R_2) zurückführen lassen. Im üblichen Sinne heisst ein Axiomen System unabhängig, wenn keiner seiner Sätze aus den übrigen beweisbar ist.

Die Begriffsbildungen, Fragestellungen und Ergebnisse aus der Hertz'schen Untersuchung der Satz-Systeme sollen hier nicht weiter verfolgt werden.[5] Im vorliegenden Zusammengang betrachten wir diese Theorie der Satz-Systeme unter dem Gesichtspunkt, dass aus ihr der *Sequenzen-Kalkul* erwachsen ist. GERHARD GENTZEN wurde auf den Sequenzen-Kalkul, jedenfalls zu einem Teil, durch seine Beschäftigung mit der Theorie der Satz-Systeme geführt. Er brachte hier die vordem noch offene Frage, ob es nicht nur zu jedem endlichen, sondern auch zu jedem unendlichem Satz-System immer ein unabhängiges Axiomen-System gebe, in verneinendem Sinne, durch Aufweisung eines Gegenbeispiels, zur Entscheidung. Zugleich zeigte er andrerseits, dass ein abgeschlossenes Satz-System stets dann ein unabhängiges Axiomen-System besitzt, falls es ein Axiomen-System besitzt welches nur aus Sätzen der Form $a \rightarrow b$, d.h. mit einem *eingliedrigem* Antecedens, besteht.[6] Anlässlich dieser Untersuchung gab Gentzen den Schlussregeln eine vereinfachte Form, indem er bemerkte, dass das Schema des Syllogismus sich auf den Spezialfall $m = 1$ zurückführen lässt. Diesen Spezialfall bezeichnet er als "Schnitt".

[5] Immerhin mag bei dieser Gelegenheit darauf hingewiesen werden, dass in der Hertz'schen Theorie der Satz-Systeme ein gewiss bei weitem noch nicht hinsichtlich der möglichen Fragestellungen und Erkenntnisse ausgeschöpftes Forschungs-Gebiet der Axiomatik und Logik vorliegt.

[6] Siehe die Abhandlung: Ueber die Existenz unabhängiger Axiomen-Systeme zu unendlichen Satz-Systemen. Math. Annalen **107** (1932).

Mit seiner Angabe der Sätze in der Form $A \rightarrow b$, wobei A das als *Menge* aufgefasste Antecedens ist, stellt sich der Kalkul der Satz-Systeme folgendermassen dar: Die Ausgangssätze sind die tautologischen Sätze und die Axiome. Als Schemata des Schliessens hat man den unmittelbaren Schluss, oder nach Gentzen's Bezeichnung, das "Verdünnungs-Schema" $\dfrac{B \rightarrow c}{AB \rightarrow c}$ und das "Schnittschema" $\dfrac{A \rightarrow c, \quad Bc \rightarrow d}{AB \rightarrow d}$. Dabei bedeutet AB die Vereinigung der Mengen A, B und Bc die aus B durch Hinzufügung des Elementes c hervorgehende Menge.

Von hier aus gelangen wir zu einem *Sequenzen-Kalkul*, indem wir 1. die Elemente als Aussagenformeln einer formalisierten Theorie annehmen 2. ein Antecedens nicht als Menge, sondern wieder als *Folge* von Aussagen-Formeln auffassen. Bezeichnen A, B solche Folgen und c eine Aussagen-Formel so möge "A, B" die Folge bedeuten, die entsteht, indem die Folge B hinter die Folge A gesetzt wird, "A, c" und "c, A" mögen die Folgen bedeuten welche entstehen, indem c rechts, bezw. links, als Glied an die Folge A angefügt wird. Die Gesamt-Ausdrücke der Form $A \rightarrow b$ heissen jetzt, zur deutlichen Unterscheidung von den Sätzen der formalisierten Theorie, nicht mehr "Sätze", sondern "Sequenzen".

Es werden auch *leere* Formel-Folgen zugelassen und eine Sequenz $\rightarrow c$ mit leerem Antecedens soll gleichbedeutend sein mit der Succedens-Formel c.

In Entsprechung zu dem beschriebenen Kalkul der Satz-Systeme ergibt sich nun der folgende Sequenzen-Kalkul: Als "Grund-Sequenzen" haben wir die tautologischen Sequenzen der Form $a \rightarrow a$. Ausserdem können gewisse Sequenzen axiomatisch als Grund-Sequenzen postuliert sein. Als Schemata des formalen Schliessens werden genommen:

Verdünnung: $$\frac{B \rightarrow c}{a, B \rightarrow c}$$

Zusammenziehung: $$\frac{a, a, B \rightarrow c}{a, B \rightarrow c}$$

Vertauschung: $\dfrac{C, a, b, D \to c}{C, b, a, D \to c}$

Schnitt: $\dfrac{A \to c; \quad B, c \to d}{A, B \to d}$.

Die mit B, C, D, A angegebenen Formel-Folgen können eventuell leer sein. Die Schemata der Zusammenziehung und der Vertauschung bringen zum Ausdruck, dass es für die Gültigkeit einer Sequenz nicht auf die Vielfachheit des Auftretens des gleichen Gliedes im Antecedens noch auch auf die Reihenfolge der Glieder ankommt.

Hier besteht eine Möglichkeit der Vereinfachung. Als Spezialfall eines Schnittes haben wir $\dfrac{B \to c; \quad B, c \to d}{B, B \to d}$, und aus der Sequenz $B, B \to d$ erhalten wir mittels der Schemata der Vertauschung und der Zusammenziehung $B \to d$. Als abgeleitetes Schema ergibt sich somit:

$$(S) \qquad \frac{A \to b; \quad A, b \to c}{A \to c}.$$

Legen wir andrerseits dieses Schema anstelle des vorherigen Schnittschemas zugrunde und erweitern wir das Verdünnungs-Schema, indem wir neben dem Uebergang von $B \to c$ zu $a, B \to c$ auch denjenigen zu $B, a \to c$ – (der nach dem Vorherigen durch Verdünnung und Vertauschung bewirkt wird) – als Regel nehmen, so werden die Schemata der Zusammenziehung und der Vertauschung sowie das vorherige Schema des Schnittes zu *ableitbaren* Uebergängen.

Es wird genügen, die Ableitbarkeit der Vertauschung an einem Beispiel zu zeigen. Der Uebergang von einer Sequenz $u, a, b, v \to c$ zu $u, b, a, v \to c$ kann folgendermassen mittels der tautologischen Grundsequenzen, des erweiterten Verdünnungs-Schemas (Verdünnung "nach links" und "nach rechts") und des vereinfachten Schnittes (S) vollzogen werden: Aus der Sequenz $u, a, b, v \to c$ erhält man durch mehrmalige Verdünnung nach links

$$((1)) \qquad u, b, a, v, u, a, b, v \to c.$$

Aus der Grund-Sequenz $v \to v$ erhält man durch Verdünnungen nach links und nach rechts:

((2)) $u, b, a, v, u, a, b \rightarrow v.$

Aus ((2)) und ((1)) liefert das Schnitt-Schema (S) die Sequenz

((3)) $u, b, a, v, u, a, b \rightarrow c.$

In ganz entsprechender Weise, wie man von ((1)) zu ((3)) gelangt,

gewinnt man aus ((3)) die Sequenz $u, b, a, v, u, a \rightarrow c,$
entsprechend aus dieser $u, b, a, v, u \rightarrow c,$
und schliesslich aus dieser $u, b, a, v \rightarrow c.$

Es sind somit als Schemata des formalen Schliessens ausreichend: die Verdünnung nach links und nach rechts und das einfachere Schnitt-Schema (S).

2. Sequenzen-Schemata für die intuitionistische Aussagenlogik

Der beschriebene Sequenzen-Kalkul bildet nur den Anfangsteil eines logischen Kalkuls mit Sequenzen. Man kann ihn als den "reinen" Sequenzen-Kalkul bezeichnen, da in seinen Regeln nur diejenigen Gesetzlichkeiten ihren Ausdruck finden, welche das Folgern aus endlich vielen Prämissen betreffen, noch nicht aber die auf die aussagenlogischen und prädikatenlogischen Operationen bezüglichen Gesetze. Wir haben die Anknüpfung des reinen Sequenzen-Kalkuls an den Kalkul der Satz-Systeme betrachtet. Diese bildete für Gentzen nur die eine Seite der Motivierung seines Sequenzen-Kalkuls. Die andere Art der Motivierung, welche in seiner Abhandlung "Untersuchungen über das logische Schliessen"[7] (worin er zuerst den Sequenzen-Kalkul entwickelte) explicite in Erscheinung tritt, besteht in dem Zusammenhang mit dem Kalkul des "natürlichen Schliessens", der eine Art des Annahmen-Kalkuls ist (d.h. ein Kalkul, bei dem Annahmen von Sätzen eingeführt

[7] Mathematische Zeitschrift **39** (1934) siehe insbesondere S. 183–196. Eine Übersetzung dieser Gentzen'schen Abhandlung ins Französische mit Erläuterungen, Zusätzen und Kommentaren wurde von ROBERT FEYS und JEAN LADRIÈRE publiziert, in der Sammlung: *Philosophie de la Matière* (Paris 1955). Eine Übersetzung ins Englische, von MANFRED SZABO, soll demnächst im American Philosophical Quarterly erscheinen.

werden, die hernach gemäss bestimmten Regeln wieder ausgeschaltet werden). In der Tat führt die Aufgabestellung, den logischen Kalkul möglichst weitgehend dem gebräuchlichen Schliessen anzupassen, auf einen Annahmen-Kalkul.[8]

Der Annahmen-Kalkul hat jedoch in seiner Handhabung, gewisse Komplikationen durch die Superposition der Annahmen (d.h. weil die unter Annahmen erfolgenden Beweise wiederum Annahmen einführen können). Hier lässt sich nun mit Vorteil der Sequenzen-Formalismus zur Anwendung bringen, in welchem das Gelten einer Aussage *unter bestimmten Annahmen* die logische Grundform bildet. Die betreffende Aussage steht dann im Succedens, die Annahmen bilden die Glieder des Antecedens.

Die allgemeinen Regeln für das Umgehen mit Sequenzen werden durch die Schemata des reinen Sequenzen-Kalkuls geliefert. Dazu treten die Schemata, durch welche die Rolle der logischen Operatoren bestimmt wird. Diese Schemata können auf sehr verschiedene Art gewählt werden. Wir wollen hier zunächst diejenige Form der Schemata betrachten, die sich ergibt, wenn wir die Gentzen'schen Schemata des natürlichen Schliessens in Sequenzen-Schemata übersetzen. Dabei wollen wir die Betrachtung auf die aussagenlogischen Operatoren beschränken. Gentzen betrachtet von diesen: Konjunktion ("und"), Alternative ("oder", im Sinne von "vel"), Implikation ("wenn-so") und Negation. Die Negation, die eine gewisse Sonderstellung hat, möge vorerst noch beiseite gelassen werden. Die Uebersetzung der Schemata für Konjunktion, Alternative und Implikation aus dem Kalkul des natürlichen Schliessens ergibt – wenn wir als Symbole für die drei Operationen: &, $\vee$, $\supset$, und, wie bisher, als Zeichen für Aussagen, bezw. Aussagenformeln, kleine lateinische Buchstaben,[9] als Zeichen für Folgen von Aussagen grosse lateinische Buchstaben nehmen:

[8] Ein Annahmen-Kalkul wurde auch etwa zur gleichen Zeit wie der Gentzen'sche von Stanisław Jaśkowski begründet, der damit einer Anregung von Jan Łukasiewicz folgte, die sich ebenfalls auf die Anlehnung an das natürliche Schliessen bezog. Vgl. S. Jaśkowski, On the Rules of Suppositions in Formal Logic. Studia Logica, Nr. 1 (Warschau 1934).

[9] Wir können hier so verfahren, weil wir die kleinen lateinischen Buchstaben nicht für Individuen-Variabeln brauchen werden.

$$a, b \rightarrow a \,\&\, b \qquad a \,\&\, b \rightarrow a, \qquad a \,\&\, b \rightarrow b$$

$$a \rightarrow a \vee b, \qquad b \rightarrow a \vee b \qquad \frac{A, b \rightarrow d; \quad A, c \rightarrow d}{A, b \vee c \rightarrow d}$$

$$\frac{A, b \rightarrow c}{A \rightarrow b \supset c} \qquad a, a \supset b \rightarrow b.$$

Wir haben hier 8 Schemata; 6 davon sind Schemata für Grund-Sequenzen, 2 sind Schemata des formalen Schliessens. Die ersteren entsprechen direkt den Gentzen'schen Schematen, z.B. die Grundsequenz $a, b \rightarrow a \,\&\, b$ dem Schema $\frac{a, b}{a \,\&\, b}$. Dagegen ist bei den Schematen des Schliessens jeweils im Antecedens der Sequenzen eine Folge A hinzugefügt. Z.B. das Schema $\frac{A, b \rightarrow c}{A \rightarrow b \supset c}$ tritt an die Stelle der Gentzen'schen Regel: "Wenn unter, der Annahme b die Aussage c gefolgert werden kann, so gilt $b \supset c$." Dieser Regel würde äusserlich das Schema $\frac{b \rightarrow c}{\rightarrow b \supset c}$ entsprechen, welches in dem betrachteten Schema als Spezialfall (für eine leere Folge A) enthalten ist. Das Erfordernis der Hinzufügung von A im Antecedens ergibt sich daraus, dass die Gentzen'sche Regel auch im Rahmen solcher Beweisführungen anwendbar sein muss, die ihrerseits unter gewissen Annahmen erfolgen.[10]

Zur Erläuterung des Systems unserer 8 Schemata – es werde kurz mit "[&, v, ⊃]" bezeichnet – sei folgendes bemerkt.

1. Die Anwendung der Schemata ist so zu verstehen, dass für die Buchstaben $a, b, c, \ldots$ Aussagenformeln zu setzen sind, die entweder "Primformeln" oder aus solchen mittels der Verknüpfungen &, v, ⊃ zusammengesetzt sind, wobei in der üblichen Weise die Reihenfolge der Operationen durch Klammern oder Trennungs-

[10] Tatsächlich lässt sich auch anhand eines verbandstheoretischen Modelles zeigen, dass, wenn wir das Schema $\frac{A, b \rightarrow c}{A \rightarrow b \supset c}$ durch das speziellere $\frac{b \rightarrow c}{\rightarrow b \supset c}$ ersetzen, der Uebergang von einer Sequenz $a, b \rightarrow c$ zu $a \rightarrow b \supset c$ mit unseren Schematen nicht vollzogen werden kann.

punkte anzugeben ist. Was für Aussagen als Primformeln zu nehmen sind, hängt von der jeweiligen Anwendung des Kalkuls ab. Beim reinen Aussagenkalkul fungieren als Primformeln wiederum Buchstaben. Die Sequenzen sollen nicht zu den Aussagenformeln gerechnet werden.

2. Die Schemata [&, v, ⊃] sind, wie schon erwähnt, *in Verbindung mit dem reinen Sequenzen-Kalkul* anzuwenden, für welchen, wie wir feststellten, das Schema der tautologischen Grundsequenzen, die Schemata der Verdünnung nach rechts und nach links und das Schnittschema (S) ausreichen. Die Gewinnung einer Sequenz mittels des reinen Sequenzenkalkuls und der Schemata [&, v, ⊃] bezeichnen wir als Herleitung der Sequenz; und die Herleitung einer Sequenz $\rightarrow c$, mit leerem Antecedens, welche ja mit der Formel c gleichgesetzt wird, gilt darum auch als Herleitung dieser Formel.

3. In dem Gentzen'schen Kalkul des natürlichen Schliessens sind die "Schlussfiguren-Schemata" so gewählt, dass sie entweder die "Einführung" oder die "Beseitigung "(Elimination) eines Verknüpfungssymbols bewirken. Eine entsprechende Unterscheidung können wir auch bei den Schematen [&, v, ⊃] vornehmen. Nämlich in jedem dieser Schemata tritt nur eines der Symbole &, v, ⊃ und auch nur einmal, und zwar bei den Schematen des Schliessens in der unteren Formel auf. Bei den Schematen, die einer Einführung entsprechen, findet das Auftreten im Succedens, bei denen, die einer Beseitigung entsprechen, im Antecedens statt.

Wir können den Gegensatz von Einführung und Beseitigung durch eine andere Wahl der Schemata prägnanter zum Ausdruck bringen, indem wir zur sequenztheoretischen Kennzeichnung der Verknüpfungen &, v, ⊃ anstatt der Schemata [&, v, ⊃] folgende direkt mt ihrer Umkehrung versehene Sequenzen-Schemata nehmen: [11]

$$\langle \& \rangle: \quad \frac{A, b, c \rightarrow d}{\overline{A, b \,\&\, c \rightarrow d}} \qquad\qquad \langle \vee \rangle: \quad \frac{A, b \rightarrow d; \quad A, c \rightarrow d}{\overline{A, b \vee c \rightarrow d}}$$

[11] Der Gedanke, die logischen Operatoren durch solche, ihre Umkehrung einbegreifende Sequenzen-Schemata zu kennzeichnen, wurde von Karl Popper in seiner bereits erwähnten Abhandlung: New Foundations for Logic (vgl. Fussnote 4) angeregt und zur Anwendung gebracht.

$$\langle \supset \rangle: \quad \frac{A, b \rightarrow c}{A \rightarrow b \supset c}.$$

Hier soll jeweils der Doppelstrich besagen, dass nicht nur der Uebergang von der oberen Sequenz, bzw. den oberen Sequenzen, zu der unteren erlaubt sein soll, sondern auch der Uebergang von der unteren Sequenz zu der, bezw. den oberen.

In der Tat sind diese drei Schemata $\langle \& \rangle$, $\langle \vee \rangle$, $\langle \supset \rangle$ den Schematen $[\&, \vee, \supset]$ gleichwertig. Bei den Schematen $\langle \vee \rangle$ und $\langle \supset \rangle$ ist der Uebergang von der oberen zur unteren Sequenz bereits ein Schema aus $[\&, \vee, \supset]$. Die zugehörigen Umkehrungen sind:

$$\frac{A \rightarrow b \supset c}{A, b \rightarrow c}, \qquad \frac{A, b \vee c \rightarrow d}{A, b \rightarrow d}, \qquad \frac{A, b \vee c \rightarrow d}{A, c \rightarrow d}$$

Die erste davon ergibt sich folgendermassen: Aus der Sequenz $b, b \supset c \rightarrow c$, welche eine Grundsequenz von $[\&, \vee, \supset]$ ist, erhält man durch Verdünnung nach links $A, b, b \supset c \rightarrow c$. Andererseits erhält man aus der Sequenz $A \rightarrow b \supset c$ durch Verdünnung nach rechts $A, b \rightarrow b \supset c$. Die beiden erhaltenen Sequenzen ergeben mittels des Schnittes $A, b \rightarrow c$. Auf ganz entsprechende Art erhält man die beiden anderen Umkehrungen, indem man aus dem System $[\&, \vee, \supset]$ die Grundsequenzen $b \rightarrow b \vee c$ und $c \rightarrow b \vee c$ benutzt. Und die beiden in $\langle \& \rangle$ vereinigten Uebergänge: von $A, b, c \rightarrow d$ zu $A, b \& c \rightarrow d$ und umgekehrt, ergeben sich durch Benutzung der Grundsequenzen $b, c \rightarrow b \& c$, $b \& c \rightarrow b$, $b \& c \rightarrow c$ aus dem System $[\&, \vee, \supset]$ mit Anwendung der Schemata des reinen Sequenzen-Kalkuls.

Um andrerseits aus den Schematen $\langle \& \rangle$, $\langle \vee \rangle$, $\langle \supset \rangle$ die Grundsequenzen und Schluss-Schemata von $[\&, \vee, \supset]$ abzuleiten, hat man von $\langle \& \rangle$ und $\langle \vee \rangle$ die Uebergänge von den unteren zu den oberen Sequenzen, für leeres A, in Verbindung mit den tautologischen Sequenzen $b \& c \rightarrow b \& c$, $b \vee c \rightarrow b \vee c$ anzuwenden. Damit gewinnt man $b, c \rightarrow b \& c$, $b \rightarrow b \vee c$, $c \rightarrow b \vee c$. Weiter ergibt die Anwendung von $\dfrac{A \rightarrow b \supset c}{A, b \rightarrow c}$ in Verbinding mit der tautologischen Sequenz $b \supset c \rightarrow b \supset c$ die Sequenz $b \supset c, b \rightarrow c$ und somit auch

$b, b \supset c \rightarrow c$. Schliesslich ergibt die Anwendung von $\dfrac{A, b, c \rightarrow d}{A, b \,\&\, c \rightarrow d}$ für leeres A, in Verbindung mit den im reinen Sequenzen-Kalkul herleitbaren Sequenzen $b, c \rightarrow b$, $b, c \rightarrow c$ die Sequenzen $b \,\&\, c \rightarrow b$, $b \,\&\, c \rightarrow c$. Die beiden dann noch von $[\&, \vee, \supset]$ verbleibenden Schluss-Schemata sind direkt in $\langle \vee \rangle$, $\langle \supset \rangle$ enthalten.

Die Bedeutung der Schemata $\langle \& \rangle$ und $\langle \supset \rangle$ lässt sich am besten mit Hilfe des Begriffes der "Ableitungsgleichheit" kennzeichnen. Zwei Sequenzen mögen in Bezug auf ein System von Regeln "ableitungsgleich" heissen, wenn aus jeder von ihnen die andere gemäss diesen Regeln gewonnen werden kann. Offensichtlich ist diese Ableitungsgleichheit eine Aequivalenz-Beziehung. Ferner mögen zwei Aussagen-Formeln p, q ableitungsgleich heissen, wenn die Sequenzen $\rightarrow p$, $\rightarrow q$ ableitungsgleich sind.

Wenn zwei Formeln p, q inbezug auf unsere Grundsequenzen und Schluss-Schemata ableitungsgleich sind, so sind die Sequenzen $p \rightarrow q$ und $q \rightarrow p$ herleitbar. Dieses ergibt sich aufgrund des folgenden (für unsere Schemata leicht als gültig zu erweisenden) "Deduktions-Theorems": Wenn mit Hilfe der Formel p, (bezw. der Sequenz $\rightarrow p$) eine Sequenz $A \rightarrow c$ herleitbar ist, so ist ohne Benutzung jener Formel (bezw. Sequenz) die Sequenz $p, A \rightarrow c$ herleitbar.[12]

Aus der Herleitbarkeit der Sequenzen $p \rightarrow q$, $q \rightarrow p$ folgt auch diejenige der Formeln $p \supset q$, $q \supset p$ und damit auch diejenige der Formel $(p \supset q) \,\&\, (q \supset p)$. Umgekehrt können wir aus dieser Formel die Sequenzen $p \rightarrow q$, und $q \rightarrow p$ mittels unserer Schemata gewinnen.

Es ist nun aufgrund des Schemas $\langle \& \rangle$ jede Sequenz mit mehrgliedrigem Antecedens einer solchen mit eingliedrigem Antecedens ableitungsgleich, z.B. eine Sequenz $a, b, c \rightarrow d$ der Sequenz $a \,\&\, (b \,\&\, c) \rightarrow d$. (Hier kann auch im Antecedens die Reihenfolge der Konjunktionsglieder sowie die Klammerung beliebig geändert werden.) Aufgrund des Schemas $\langle \supset \rangle$ ist *jede Sequenz einer Formel ableitungsgleich*, z.B. die Sequenz $a, b, c \rightarrow d$ der Formel

[12] Dass diese Behauptung ohne Einschränkung gilt, beruht darauf, dass wir in unserem, gemäss den Sequenzen-Schematen und Schluss-Schematen sich vollziehenden Kalkul *keine Einsetzungen* haben.

$a \supset (b \supset (c \supset d))$. Hier erhält man ohne Mühe viele Folgerungen. Nur ein Beispiel: Da die Sequenz $a, b \rightarrow c$ einerseits der Sequenz $a \,\&\, b \rightarrow c$ und somit auch der Formel $(a \,\&\, b) \supset c$ und andrerseits der Formel $a \supset (b \supset c)$ ableitungsgleich ist, so sind die beiden Formeln $(a \,\&\, b) \supset c$ und $a \supset (b \supset c)$ einander ableitungsgleich. Es sind somit die Sequenzen $a \,\&\, b \supset c \rightarrow a \supset (b \supset c)$, $a \supset (b \supset c) \rightarrow (a \,\&\, b) \supset c$ herleitbar, welche die aussagenlogischen Gesetze der "Exportation" und der "Importation" ausdrücken.

Aus den Beziehungen der Ableitungsgleichheit zwischen Sequenzen und Formeln ergibt sich der Zusammenhang zwischen dem durch unsere Schemata bestimmten Sequenzen-Kalkul und dem aussagenlogischen Formel-Kalkul, insbesondere derjenigen Art des deduktiven Aussagen-Kalkuls, bei der als einziges Schluss-Schema der modus ponens $\dfrac{a, a \supset b}{b}$ verwendet wird. Diesem entspricht in dem Sequenzen-Kalkul der Uebergang von den Sequenzen $\rightarrow a$ und $\rightarrow a \supset b$ zu $\rightarrow b$, welcher sich hier so vollziehen lässt, dass man aus $\rightarrow a \supset b$ mittels des Schemas $\langle \supset \rangle$ die Sequenz $a \rightarrow b$ gewinnt, welche zusammen mit $\rightarrow a$ gemäss dem Schnitt-Schema $\rightarrow b$ ergibt. Ein solcher deduktiver Kalkul ist insbesondere derjenige, durch welchen A. HEYTING die *Brouwer'sche "intuitionistische" Aussagenlogik* formalisiert hat.[13]

Anhand der Beziehungen zwischen Sequenzen und Formeln und aufgrund der Ableitbarkeit des modus ponens im Sequenzen-Kalkul lässt sich nun zeigen, dass der Bereich der Formeln, die mittels der Schemata $[\&, \vee, \supset]$ oder statt dessen der Schemata $\langle \& \rangle$, $\langle \vee \rangle$, $\langle \supset \rangle$ unter Zugrundelegung des reinen Sequenzen-Kalkuls herleitbar sind, mit dem Bereich derjenigen Formeln aus dem Heyting'schen Aussagen-Kalkul übereinstimmt, die mittels der Verknüpfungen $\&, \vee, \supset$ gebildet sind. Für den Nachweis genügen aufgrund des zuvor Bemerkten die folgenden beiden Feststellungen: 1. Die Ausgangsformeln des Heyting'schen Kalkuls sind mittels unserer Sequenzen-Schemata herleitbar. 2. Mittels der Ersetzung einer jeden Sequenz durch eine ihr ableitungsgleiche Formel gehen die

[13] Vgl. A. HEYTING, *Die formalen Regeln der intuitionistischen Logik*, Sitzungsberichte der preussischen Akad. der Wissenschaften Phys.-Math. Klasse (Berlin 1930).

Grundsequenzen unseres Sequenzen-Kalkuls in ableitbare Formeln des Heyting'schen Kalkuls über und die Schluss-Schemata in Schemata von solchen Uebergängen zwischen Formeln, die sich im Heyting'schen Kalkul vollziehen lassen. Wir wollen hier diese Feststellungen, die keine grossen Schwierigkeiten bieten, nicht im Einzelnen ausführen.[14]

Ergänzend sei noch erwähnt, dass als Schema mit Umkehrung für die Konjunktion anstatt ⟨&⟩ auch das folgende Schema genommen werden kann: $\dfrac{A \to b;\ A \to c}{A \to b \,\&\, c}$ (Doppelstrich), von dem man leicht die Gleichwertigkeit (im Rahmen des reinen Sequenzen-Kalkuls) mit den Grundsequenzen $a \,\&\, b \to a$, $a \,\&\, b \to b$, $a, b \to a \,\&\, b$ erkennt.

Um nun für die ganze intuitionistische Aussagenlogik einen Sequenzen-Kalkul zu gewinnen, bedarf es noch der *Hinzunahme der Negation*. Die Negation einer Aussage wird in der intuitionistischen Logik als die Behauptung der Absurdität der Aussage verstanden. Die Formalisierung des Begriffs der Absurdität kann mittels der Einführung eines Symbols $\curlywedge$ für das Falsche geschehen. Wir kommen hier zurück auf das ganz zu Anfang erwähnte Folgerungs-Axiom von Tarski, welches die Existenz einer Aussage fordert, aus welcher jede in Betracht kommende Aussage gefolgert werden kann. Nehmen wir $\curlywedge$ als eine solche Aussage, so können wir die geforderte Eigenschaft von $\curlywedge$ durch das Sequenz-Schema $\curlywedge \to c$ formalisieren. Fügen wir dieses zum System unserer Schemata hinzu, so ergibt sich als ableitbares Schema $\dfrac{A \to \curlywedge}{A \to c}$. Nunmehr lässt sich die intuitionistische Negation explicite definieren

[14] Zu dem Ergebnis ist noch darauf hinzuweisen, dass für die mit den Verknüpfungen &, v, $\supset$, also ohne die Negation gebildeten Aussagen-Formeln die Herleitbarkeit im Heyting'schen Kalkul übereinstimmt mit der Herleitbarkeit in dem "Minimal-Kalkul" von INGEBRIGT JOHANSSON. Vgl.: Der Minimalkalkül, ein reduzierter intuitionistischer Formalismus. Compositio Mathematica **4**, Heft 1 (Groningen 1936). Der betrachtete Formelbereich lässt sich auch anhand des in den: Grundlagen der Mathematik I, S. 66 vorgelegten deduktiven Systems der Aussagenlogik kennzeichnen als die Gesamtheit derjenigen Formeln, die aus den Formeln I-III mittels Einsetzungen und des modus ponens zu gewinnen sind.

durch die Festsetzung, dass für eine jede Aussagenformel b die Implikation $b \supset \curlywedge$ ersetzbar sei durch $\neg b$ ("b ist absurd"), und umgekehrt $\neg b$ durch $b \supset \curlywedge$.

Anstatt so die Negation definitorisch einzuführen, können wir aber auch, anschliessend an die Einführung von $\curlywedge$ nebst dem Sequenzen-Schema $\curlywedge \rightarrow c$, die Negation durch Schemata kennzeichnen. Die Uebersetzung der von Gentzen in seinem "Kalkul des natürlichen Schliessens" angegebenen Schemata für die Negation "$\neg$" ergibt:

$$\frac{A, b \rightarrow \curlywedge}{A \rightarrow \neg b} \qquad a, \neg a \rightarrow \curlywedge.$$

Anstatt dieser zwei Schemata können wir auch das erste nebst seiner Umkehrung, also $\dfrac{A, b \rightarrow \curlywedge}{A \rightarrow \neg b}$ (Doppelstrich) nehmen.

Man wird sich fragen, ob hier nicht die Einführung des Symbols $\curlywedge$ erspart werden kann. Eine Möglichkeit hierfür, die von Gentzen benutzt wurde, besteht darin, dass man Sequenzen mit *leerem Succedens* zulässt. Das leere Succedens wird dann als Darstellung des Falschen, aber nicht als Formel genommen. Dem Sequenz-Schema $\curlywedge \rightarrow c$ entspricht dann das Schluss-Schema $\dfrac{A \rightarrow}{A \rightarrow c}$; dieses kann als Regel der Verdünnung im Succedens, also als Regel des reinen Sequenzen-Kalkuls aufgefasst werden. Inhaltlich ist dieses Schema so zu verstehen: Das Falsche ist das stärkste mögliche Folgerungsergebnis; die Folgerung einer beliebigen anderen Aussage kann, wenn sie nicht damit gleichbedeutend ist, nur eine Abschwächung sein.

Mit Anwendung des leeren Succedens lauten die Schemata der Negation in der ersten Fassung: $\dfrac{A, b \rightarrow}{A \rightarrow \neg b}$, $a, \neg a \rightarrow$, in der zweiten Fassung: $\dfrac{A, b \rightarrow}{A \rightarrow \neg b}$ (Doppelstrich).

Schliesslich können wir aber auch ohne eine solche Erweiterung auskommen, indem wir die Schemata $\dfrac{A, b \rightarrow \neg b}{A \rightarrow \neg b}$, $a, \neg a \rightarrow b$ nehmen.

Hier ist die direkte Umkehrung des ersten Schemas bloss eine Verdünnung und kann nicht das zweite Schema ersetzen. Die angemessene Umkehrung ist hier $\dfrac{A \rightarrow \neg b}{A, b \rightarrow c}$.

Ein kleiner Schönheitsfehler bei diesem dritten Verfahren ist, dass das Schema $\dfrac{A, b \rightarrow \neg b}{A \rightarrow \neg b}$ weder als "Einführung" noch als "Beseitigung" der Negation bezeichnet werden kann, da die Negation hier sowohl in der oberen wie in der unteren Sequenz auftritt.

Es lässt sich nun zeigen, dass bei Hinzunahme der Schemata für die Negation, in einer der verschiedenen angegebenen Formen, zu unseren vorherigen Schematen [&, $\vee$, $\supset$] die Gesamtheit der herleitbaren Formeln (soweit sie nicht das Symbol $\curlywedge$ enthalten) mit der Gesamtheit der im intuitionistischen Aussagenkalkul beweisbaren Formeln übereinstimmt.[15]

Zunächst ergibt sich diese Uebereinstimmung für die zuletzt angegebene Form der Schemata der Negation (ohne $\curlywedge$ und ohne leeres Succedens), und zwar auf ganz entsprechende Art wie zuvor für den negationsfreien Formelbereich (vgl. S. 14–15). Was ferner die Schemata für $\curlywedge$ und $\neg$: $\dfrac{A, b \rightarrow \curlywedge}{A \rightarrow \neg b}$, $a, \neg a \rightarrow \curlywedge$, $\curlywedge \rightarrow c$ betrifft, so sind aus diesen die Schemata $\dfrac{A, b \rightarrow \neg b}{A \rightarrow \neg b}$ und $a, \neg a \rightarrow b$ ableitbar. Andrerseits werden die Schemata für $\curlywedge$, $\neg$ aus unseren Schematen für die Implikation unmittelbar erhalten, wenn die definitorische Einführung der Negation nebst dem Formelschema $\curlywedge \supset c$ benutzt wird. Es ergibt sich daher, dass bei der Hinzufügung der Schemata für $\curlywedge$ und $\neg$ zu den Schematen [&, $\vee$, $\supset$] oder statt dessen der definitorischen Einführung der Negation (nebst dem Formelschema

[15] Nicht alle Formeln dieser Gesamtheit sind im Minimal-Kalkul beweisbar. Für den Minimal-Kalkul kann die Negation $\neg a$ definiert werden durch eine Implikation $a \supset P$, wobei P ein Aussagezeichen ist, für das keinerlei Schema gefordert wird. Statt dessen kann natürlich auch hier die Negation durch ein Schema eingeführt werden. Man kommt dann mit dem einen Schema $\dfrac{A, b \rightarrow \neg c}{A, c \rightarrow \neg b}$ aus, das zu sich selbst invers ist.

$\curlywedge \supset c$) alle mit &, $\vee$, $\supset$, $\neg$ gebildeten Formeln des intuitionistischen Aussagen-Kalkuls herleitbar sind. Dass andrerseits auch nicht mehr solcher Formeln so erhalten werden, ergibt sich, indem man $\curlywedge$ als Zeichen für eine Formel $a_0 \mathbin{\&} \neg a_0$ nimmt, (wobei a_0 eine beliebig ausgezeichnete Primformel ist), da dann jede Formel $\curlywedge \supset c$ im intuitionistischen Kalkul beweisbar ist) sowie auch, für jede Formel b, die Implikationen $\neg b \supset (b \supset \curlywedge)$ und $(b \supset \curlywedge) \supset \neg b$ beweisbar sind.

Was endlich die Schemata mit leerem Succedens betrifft, so erkennt man deren Gleichwertigkeit mit den Schematen für $\curlywedge$, $\neg$ (in Hinsicht auf die Herleitbarkeit von $\curlywedge$-freien Formeln), indem man allenthalben das leere Succedens durch $\curlywedge$ ersetzt.

In der Aussagenlogik führt man nicht nur für die Grundverknüpfungen, sondern auch für solche Verknüpfungen, die sich durch zusammengesetzte Ausdrücke darstellen, Symbole und Namen ein. Zum Teil stehen solche direkt aus der Umgangssprache zur Verfügung, wie insbesondere "weder-noch" und das ausschliessende "oder". Solche durch zusammengesetzte Ausdrücke definierbare Verknüpfungen können im allgemeinen durch verschiedene gleichwertige Ausdrücke dargestellt werden. Dabei ist aber mit Bezug auf die intuitionistische Logik zu beachten, dass hier oft zwei Ausdrücke, die in der üblichen, "klassischen" Aussagenlogik gleichwertig sind, unterschieden werden müssen, und zwar nicht nur sinnmässig, sondern auch hinsichtlich ihres Zutreffens.

Für die Verknüpfung "weder-noch" sind die beiden in Betracht kommenden Darstellungen $\neg a \mathbin{\&} \neg b$, $\neg (a \vee b)$ auch intuitionistisch gleichwertig. Für das ausschliessende "oder" hat man zunächst die zwei intuitionistisch gleichwertigen Ausdrücke $(a \mathbin{\&} \neg b) \vee (b \mathbin{\&} \neg a)$ und $(a \vee b) \mathbin{\&} \neg (a \mathbin{\&} b)$. In dem zweiten tritt als Konjunktionsglied $\neg (a \mathbin{\&} b)$ auf. Für diese Verknüpfung hat man das Sheffer'sche Symbol $a|b$, welches in der klassischen Logik auch definiert werden kann durch die Ausdrücke $a \supset \neg b$, $b \supset \neg a$, $\neg a \vee \neg b$. Von diesen sind die ersten beiden auch intuitionistisch gleichwertig mit $\neg (a \mathbin{\&} b)$, während $\neg a \vee \neg b$ intuitionistisch stärker ist.

Erwähnt sei noch die durch den Ausdruck $(a \supset b) \mathbin{\&} (b \supset a)$ definierte "Biimplikation". In der klassischen Logik haben wir für diese auch die Darstellungen durch die Ausdrücke $(\neg a \vee b) \mathbin{\&}$

$(\neg\, b \vee a)$ und $(a \,\&\, b) \vee (\neg\, a \,\&\, \neg\, b)$, welche aber intuitionistisch stärker sind.

Auf den Ausdruck $(a \supset b) \,\&\, (b \supset a)$ wurden wir anlässlich der Betrachtung der Ableitungsgleichheit geführt. Wir fanden, dass zwei Aussagenformeln p und q dann und nur dann ableitungsgleich sind, wenn die Formel $(p \supset q) \,\&\, (q \supset p)$ in unserem Sequenzen-Kalkul beweisbar ist. Dabei kann die Bedingung der Beweisbarkeit im Sequenzen-Kalkul auch durch diejenige der Beweisbarkeit im intuitionistischen Formelkalkul (aufgrund der erwähnten Gleichwertigkeit der beiden Kalkuli) ersetzt werden.

Die durch den Ausdruck $(a \supset b) \,\&\, (b \supset a)$ dargestellte Biimplikation – zu lesen: "a genau dann, wenn b" – für die wir das Symbol "$\equiv$" nehmen wollen, lässt sich, ohne Bezugnahme auf die Implikation und Konjunktion, auch direkt sequenzen-theoretisch charakterisieren durch die Schemata:

$$a \equiv b,\, a \rightarrow b, \qquad a \equiv b,\, b \rightarrow a$$

$$\frac{A,\, b \rightarrow c;\;\; A,\, c \rightarrow b}{A \rightarrow b \equiv c}$$

oder durch das Schema mit Umkehrung: $\dfrac{A,\, b \rightarrow c;\;\; A,\, c \rightarrow b}{A \rightarrow b \equiv c}$ (Doppelstrich).

Diese Schemata sind bei der definitorischen Einführung von $a \equiv b$ ableitbar, d.h. bei der Festsetzung, dass für einen Ausdrukc $(a \supset b) \,\&\, (b \supset c)$ gesetzt werden kann $a \equiv b$, und auch umgekehrt. Andrerseits kann bei der Einführung der Biimplikation durch die Schemata die Ableitungsgleichheit von $a \equiv b$ mit dem Ausdruck $(a \supset b) \,\&\, (b \supset a)$ festgestellt werden.

Das System des intuitionistischen Sequenzen-Kalkuls, welches durch Hinzufügung der Schemata

$$\frac{A,\, b \rightarrow \neg\, b}{A \rightarrow \neg\, b}, \qquad a,\, \neg\, a \rightarrow b$$

für die Negation, und

$$\frac{A,\, b \rightarrow c;\;\; A,\, c \rightarrow b}{A \rightarrow b \equiv c} \qquad a \equiv b,\, a \rightarrow b, \qquad a \equiv b,\, b \rightarrow a$$

für die Biimplikation zu den Schematen [&, ∨, ⊃] gewonnen wird, (wobei die Anwendung aller dieser Schemata im Rahmen des reinen Sequenzen-Kalkuls gemeint ist) möge kurz als das "System [&, ∨, ⊃, ¬, ≡]" bezeichnet werden.

3. Ausrichtung des Sequenzen-Kalkuls auf den Gentzen'schen Hauptsatz

Bei der Aufstellung der sequenzenlogischen Schemata für &, ∨, ⊃, ¬ sind wir ausgegangen von den Gentzen'schen Schlussfiguren des natürlichen Schliessens. Diese haben wir in Schemata des Sequenzen-Kalkuls übersetzt. Neben das so gewonnene System haben wir noch ein anderes gestellt, dessen Schemata sämtlich Regeln des Ueberganges von einer Sequenz zu einer anderen sind, wobei paarweise zwei Uebergänge zu einander invers sind.

Das von Gentzen selbst in seinen "Untersuchungen über das logische Schliessen" verwendete System des intuitionistischen Sequenzen-Kalkuls ist von den genannten beiden Formen des Sequenzen-Kalkuls verschieden. Die Anlage dieses Kalkuls ist ausgerichtet auf eine bestimmte Zielsetzung. Gentzen gibt hier den Herleitungen eine Normalform, welche dem Beweise seines Hauptsatzes für den Prädikaten-Kalkul dient: Die Schemata für die Aussagenverknüpfungen und für die prädikatenlogischen Operatoren werden so gefasst, dass *die Anwendung von Schnitten entbehrlich wird* und in jeder Sequenz einer Herleitung jede der Formeln (d.h. die Antecedens-Glieder und das Succedens) eine Teilformel von einer Formel der Endsequenz der Herleitung ist [16] (*Teilformel-Eigenschaft*). Die Möglichkeit einer solchen Gestaltung der Herleitung, ohne Beschränkung des Bereiches der beweisbaren Formeln, bildet den Inhalt jenes Hauptsatzes. Gentzen beweist diesen gleichermassen wie für den intuitionistischen auch für den klassischen Logik-Kalkul, der von ihm als eine Erweiterung des intuitionistischen Sequenzen-Kalkuls gestaltet wird, bei welcher

[16] Eine Formel a heisst im Aussagen-Kalkul eine Teilformel einer Formel b wenn der aussagenlogische Aufbau der Formel b über die Formel a führt, sowie auch, wenn a und b gestaltlich übereinstimmen. (Analog ist die Definition für den Prädikaten-Kalkul.)

der Sequenzen-Kalkul erst seine volle Abrundung und formale Harmonie gewinnt.[17]

Eine Darlegung dieses Beweises würde uns über den Rahmen unserer Betrachtungen hinaus führen.[17a] Wir wollen uns aber die Modifikationen des Sequenzen-Kalkuls vergegenwärtigen, durch welche im Bereich des Aussagen-Kalkuls, und zwar zunächst des intuitionistischen Aussagen-Kalkuls, die dem Hauptsatz entsprechende Normalform der Herleitung ermöglicht wird.

Was zuerst den reinen Sequenzen-Kalkul angeht, so fallen ja hier diejenigen Ableitbarkeiten dahin, welche auf der Anwendung des Schnittes beruhen. Dieses hat zur Folge, dass die Schemata der Vertauschung und der Zusammenziehung (vgl. S. 6–7) wieder als Grundschemata eingeführt werden müssen. Dafür brauchen wir dann die Verdünnung nur nach einer Seite (etwa nach links) als Grundschema zu nehmen.

Bei den Schematen für die Verknüpfungen bemerken wir zunächst, dass die direkten Sequenzen-Schemata (Grundsequenzen für die Verknüpfungen) nicht geeignet sind, wenn wir Schnitte vermeiden wollen. Um z.B. mittels der Schemata $a \rightarrow a \vee b$, $b \rightarrow a \vee b$ die Sequenz $b \rightarrow a \vee (b \vee c)$ herzuleiten, bedürfen wir eines Schnittes.

Sehen wir uns ferner die Schemata $\langle \& \rangle$, $\langle \vee \rangle$, $\langle \supset \rangle$ an, so bemerken wir, dass wir von jedem der Paare zu einander inverser Schemata nur je eines beibehalten können, nämlich dasjenige, bei welchem das zu dem Schema gehörige Verknüpfungssymbol in der unteren Formel neu auftritt. Die Umkehrung eines solchen Schemas dürfen wir dagegen nicht als Regel nehmen, wenn wir den Bedingungen der Normalform genügen wollen. Wollten wir z.B. das Schema

[17] Siehe in der genannten Gentzen'schen Abhandlung: Math. Zeitschrift **39** (1934) Teil I, §§1–3, S. 190–210. Ein neuerer Beweis für den Hauptsatz im Rahmen des Sequenzen-Kalkuls wurde von HASKELL B. CURRY gegeben. Siehe: A Theory of Formal Deducibility, Notre Dame Math. Lectures (1950) Chap. II, 7, III, 6, IV, 4. Eine Darstellung des Gentzen'schen Beweises gibt S. C. KLEENE in seiner *Introduction to Metamathematics* (Amsterdam 1952) § 78.

[17a] Für den Bereich des klassischen Aussagenkalkuls werden wir im Abschnitt 4 einen einfacheren Beweis des Gentzen'schen Hauptsatzes erhalten.

$\dfrac{A, b \vee c \rightarrow d}{A, b \rightarrow d}$ als Regel benutzen, so würden wir bei einer Anwendung keine Gewähr haben, dass die Formel $b \vee c$, die in der oberen Sequenz auftritt, eine Teilformel von einer Formel der Endsequenz der Herleitung ist. Die drei hiernach beibehaltenen Schemata für sind:

$$(\&, 1): \quad \frac{A, b, c \rightarrow d}{A, b \,\&\, c \rightarrow d}\,^{18}, \qquad (\vee, 1): \quad \frac{A, b \rightarrow d; \quad A, c \rightarrow d}{A, b \vee c \rightarrow d}$$

$$(\supset, 1): \quad \frac{A, b \rightarrow c}{A \rightarrow b \supset c}.$$

Diese Schemata sind allerdings auch in dem schnittfreien Sequenzen-Kalkul in einem gewissen Sinne umkehrbar, aber nicht so, dass allgemein aus der unteren Sequenz die obere (bezw. jede der oberen) gewonnen werden kann, sondern nur in dem Sinne, dass, wenn die untere Sequenz herleitbar ist, dann auch die obere (bezw. jede der oberen) herleitbar ist.

An die Stelle der Umkehrungen der drei eben genannten Schemata treten nun die Schemata

$$(\&, 2): \quad \frac{A \rightarrow b, \; A \rightarrow c}{A \rightarrow b \,\&\, c}, \qquad (\vee, 2): \quad \frac{A \rightarrow b}{A \rightarrow b \vee c}, \quad \frac{A \rightarrow c}{A \rightarrow b \vee c}$$

$$(\supset, 2): \quad \frac{A \rightarrow b; \quad A, c \rightarrow d}{A, b \supset c \rightarrow d}.$$

Um die Schluss-Schemata der Negation dem Zweck der Ausschaltung der Schnitte anzupassen, verwendet Gentzen hier die Methode der Zulassung eines leeren Succedens, wobei zum reinen Sequenzen-Kalkul das Schema $\dfrac{A \rightarrow}{A \rightarrow b}$ (Verdünnung im Succedens) hinzukommt. Die Schemata der Negation für den schnittfreien

[18] Gentzen benutzt anstatt des Schemas (&, 1) die Schemata $\dfrac{a, G \rightarrow d}{a \,\&\, b, G \rightarrow d}$, $\dfrac{b, G \rightarrow d}{a \,\&\, b, G \rightarrow d}$ welche zusammen jenen aufgrund der Regeln der Verdünnung, Vertauschung und Zusammenziehung gleichwertig sind.

Kalkul sind dann:

$$\frac{A, b \rightarrow}{A \rightarrow \neg b}, \qquad \frac{A \rightarrow b}{A, \neg b \rightarrow}.$$

Mit dieser Art der Gestaltung des Systems der Schemata wird den Bedingungen Genüge geleistet, unter denen der Beweis des Gentzen'schen Hauptsatzes geführt werden kann, d.h. den Bedingungen für die Eliminierbarkeit der Schnitte und für die Gewinnung der Teilformel-Eigenschaft. Wie man sieht, erfordert die Erfüllung dieser Bedingungen fast gar keine Komplikation der Schemata. Nur das Schema der Implikation erhält eine ungewohnte Form. Diese Form des Schemas bewährt sich aber sehr bei der eindringenderen Untersuchung des implikativen intuitionistischen Kalkuls, insbesondere für die Gewinnung von Entscheidungsverfahren. In gewisser Hinsicht erscheint das Schema ($\supset$, 2) als eine Verallgemeinerung des Schnittes; wir gewinnen nämlich aus ihm einen Schnitt, wenn wir für c speziell b nehmen, und in der resultierenden (unteren) Sequenz das Antecedens-Glied $b \supset b$ weglassen. Das Schema ($\supset$, 2) entspricht jedoch, im Unterschied von dem Schnitt, der Bedingung, dass jede Formel in den beiden oberen Sequenzen Teilformel von einer Formel der unteren Sequenz ist.

Gentzen verwendet die etwas erweiterte Form des Schemas: $\frac{A \rightarrow b; \quad B, c \rightarrow d}{A, B, b \supset c \rightarrow d}$, welche aufgrund der Regeln der Verdünnung, Vertauschung und Zusammenziehung dem Schema ($\supset$, 2) gleichwertig ist. Diese erweiterte Form ist oft für schnelles Deduzieren besonders günstig. So erhält man mittels dieses Schemas aus den Sequenzen $a \rightarrow a$, $b \rightarrow b$ sofort (mit leerem B): $a, a \supset b \rightarrow b$, und aus dieser Sequenz, zusammen mit $c \rightarrow c$, die Sequenz $a, a \supset b, b \supset c \rightarrow c$, welche mittels Vertauschungen und des Schemas ($\supset$, 1) die Sequenz $a \supset b, b \supset c \rightarrow a \supset c$ ergibt. Die eben erhaltene Sequenz $a, a \supset b, b \supset c \rightarrow c$ liefert übrigens, zusammen mit der Sequenz $a, a \supset b \rightarrow a$ (die aus der Grundsequenz $a \rightarrow a$ durch Verdünnung hervorgeht), gemäss dem Schema ($\supset$, 2) (worin für A die Folge $a, a \supset b$ genommen wird) die Sequenz $a, a \supset b, a \supset (b \supset c) \rightarrow c$, welche, nach Anwendung von Vertauschungen, gemäss ($\supset$, 1) die Sequenz $a \supset (b \supset c), a \supset b \rightarrow a \supset c$, ergibt, aus der man durch zweimalige Anwendung von $(\supset, 1)$ die Formel $(a \supset (b \supset c)) \supset ((a \supset b) \supset (a \supset c))$ erhält.

Als ein Beispiel für das schnelle Deduzieren im schnittfreien Kalkul sei noch die Ableitung der Sequenz

$$a \,\&\, (b \vee c) \to (a \,\&\, b) \vee (a \,\&\, c)$$

angegeben (welche den Hauptteil des einen distributiven Gesetzes für & und $\vee$ ausdrückt). Aus der Grundsequenz $a \to a$ erhalten wir durch Verdünnung und Vertauschung $a, b \to a$, aus $b \to b$ durch Verdünnung $a, b \to b$. Die beiden Sequenzen zusammen ergeben gemäss $(\&, 2)$: $a, b \to a \,\&\, b$ und gemäss $(\vee, 2)$: $a, b \to (a \,\&\, b) \vee (a \,\&\, c)$. Ganz entsprechend gewinnt man die Sequenz: $a, c \to (a \,\&\, b) \vee (a \,\&\, c)$. Die beiden erhaltenen Sequenzen zusammen ergeben nach $(\vee, 1)$: $a, b \vee c \to (a \,\&\, c) \vee (a \,\&\, c)$, und nach $(\&, 1)$: $a \,\&\, (b \vee c) \to (a \,\&\, b) \vee (a \,\&\, c)$.

Bemerkt sei noch, dass aufgrund der modifizierten Fassung der Schluss-Schemata die Sequenzen der Gestalt $a \to a$ nur für *Primformeln* a als Grundsequenzen genommen zu werden brauchen, während für zusammengesetztes a diese Sequenzen ableitbar sind. In der Tat ist leicht zu verifizieren, dass wir mittels der neuen Schemata für die Negation aus $a \to a$ die Sequenz $\neg a \to \neg a$, und mittels der neuen Schemata für $\&$, $\vee$, $\supset$ aus $a \to a$ und $b \to b$ die Sequenzen $a \,\&\, b \to a \,\&\, b$, $a \vee b \to a \vee b$, $a \supset b \to a \supset b$ (und zwar jeweils ohne Schnitte) gewinnen können.

Wollen wir die Biimplikation als selbstständige Verknüpfung in den schnittfreien Sequenzen-Kalkul einbeziehen, so kann dieses mittels der folgenden Schemata geschehen: zunächst das früher schon aufgestellte Schema

$$\frac{A, b \to c; \quad A, c \to b}{A \to b \equiv c} \qquad (\equiv, 1)$$

dazu das Paar der Schluss-Schemata

$$\frac{A \to b; \quad A, c \to d}{A, b \equiv c \to d}, \qquad \frac{A \to c; \quad A, b \to d}{A, b \equiv c \to d} \qquad (\equiv, 2)$$

welches bei Anwendung von Schnitten dem Paar der Sequenzen-

Schemata

$$a \equiv b, a \rightarrow b, \qquad a \equiv b, b \rightarrow a$$

gleichwertig ist, welche mit ($\equiv$ I) und ($\equiv$ II) bezeichnet werden mögen.

Beim Beweis für die Eliminierbarkeit der Schnitte lassen sich die Schemata ($\equiv$, 1), ($\equiv$, 2) ganz entsprechend behandeln wie die Schemata der Implikation. Es gilt daher der Gentzen'sche Hauptsatz, in seinem auf den intuitionistischen Kalkul bezüglichen Teil, mit Einschluss dieser Schemata für die Biimplikation.

Aus dem Satz über die Eliminierbarkeit der Schnitte, wie sie aufgrund der modifizierten Fassung der Schluss-Schemata für den intuitionistischen Sequenzen-Kalkul besteht, können wir auch eine Folgerung für das vorherige System [&, $\vee$, $\supset \vee \neg$, $\equiv$] entnehmen. Obwohl nämlich für dieses System die Eliminierbarkeit der Schnitte dahinfällt, so bleibt hier doch für *geeignete* Herleitungen die Teilformel-Eigenschaft gewahrt, wie man ersieht, wenn man die modifizierten Schemata durch Anwendungen der Schemata aus dem System [&, $\vee$, $\supset$, $\neg$, $\equiv$] ersetzt, wobei jeweils nur die Schemata für die gleiche Verknüpfung zur Anwendung kommen. (Die Ersetzungen ergeben sich fast allenthalben einfach mit Hilfe von Schnitten. Nur die Zurückführung der modifizierten Schemata der Negation – mit Verwendung des leeren Succedens – auf die Schemata $\dfrac{A, b \rightarrow \neg b}{A \rightarrow \neg b}$ und $a, \neg a \rightarrow b$ erfordert eine sorgfältigere Diskussion.)

Aus der auch für das System [&, $\vee$, $\supset$, $\neg$, $\equiv$] bewirkbaren Teilformel-Eigenschaft der Herleitungen folgt insbesondere, dass auch im Rahmen dieses Systems jede herleitbare Sequenz durch eine "*interne*" Herleitung gewonnen werden kann, d.h. eine solche, in der nur diejenigen Verknüpfungssymbole vorkommen, welche in der Endsequenz auftreten.

4. Der symmetrische Sequenzen-Kalkul

Die neuen Schluss-Schemata die in Angemessenheit für den Gentzen'schen Hauptsatz gewählt sind, weisen überdies noch eine durchgängige Paarung (für jede der Verknüpfungen) von *Einführung im Succedens* und *Einführung im Antecedens* auf, – wenigstens wenn für die Negation die mit Benutzung des leeren Succedens gebildeten Schemata genommen werden. Diese Paarung entspricht derjenigen von Einführung und Beseitigung im Kalkul des natürlichen Schliessens.

Eine volle Symmetrie inbezug auf Antecedens und Succedens lässt sich, wie Gentzen entdeckte, im *klassischen* Logik-Kalkul erreichen. Diese Möglichkeit beruht auf dem Dualismus der klassischen Logik, den man aus der Boole'schen Algebra kennt, aufgrund dessen sich a und $\neg a$, & und $\vee$ dual entsprechen. Dieser Dualismus involviert auch für die Sequenzen ein duales Entsprechen von Antecedens und Succedens. Um diese Dualität formal voll auszunutzen, müssen wir diejenige Unsymmetrie des Sequenzen-Kalkuls beseitigen, welche sich bei seiner bisherigen Form darin zeigt, dass im Antecedens eine Folge von Formeln, im Succedens aber nur eine Formel steht. Die Zulassung eines leeren Succedens bedeutete bereits einen Schritt zur Aufhebung des Unterschiedes. Lassen wir nun im Succedens auch eine Folge von Formeln zu, so ist die allgemine Form einer Sequenz anstatt $A \to b$ nunmehr $A \to B$. Die Regeln der Verdünnung, Vertauschung und Zusammenziehung beziehen sich jetzt gleichermassen wie auf das Antecedens auch auf das Succedens; und das Schema des Schnittes erhält folgende symmetrische Form:

$$\frac{A, b \to C; \quad A \to b, C}{A \to C}$$

Zur formalen Ausprägung der Dualität wird man die Schemata für die Verknüpfungen so fassen, dass diejenigen der Negation zueinander dual und die Schemata für & zu denen für $\vee$ dual sind. Bei den Schematen der Schlussfiguren für den schnittfreien Kalkul hat man in Entsprechung zu der willkürlichen Folge A im Antecedens eine gleichfalls beliebige Folge im Succedens einzufügen. Hiernach lauten die Schemata der Negation:

$$\frac{A, b \to C}{A \to \neg b, C}, \qquad \frac{A \to b, C}{A, \neg b \to C}.$$

Gemäss diesen Schematen darf man eine Formel aus dem Antecedens negiert in das Succedens verlegen und eine Formel aus dem

Succedens negiert in das Antecedens verlegen. Die Schemata der Konjunktion lauten nun:

$$\frac{A,\, b,\, c \rightarrow D}{A,\, b \mathbin{\&} c \rightarrow D}, \qquad \frac{A \rightarrow b,\, D; \quad A \rightarrow c,\, D}{A \rightarrow b \mathbin{\&} c,\, D}.$$

Dual zu diesen sind die Schemata der Alternative zu nehmen:

$$\frac{A \rightarrow b,\, c,\, D}{A \rightarrow b \vee c,\, D}, \qquad \frac{A,\, b \rightarrow D; \quad A,\, c \rightarrow D}{A,\, b \vee c \rightarrow D}.$$ [19]

Aufgrund des ersten dieser beiden Schemata ist eine Sequenz mit einem mehrgliedrigem Succedens ableitungsgleich einer Sequenz mit einer entsprechenden Alternative im Succedens, z.B. eine Sequenz $A \rightarrow b, c, d$ der Sequenz $A \rightarrow (b \vee c) \vee d$. Damit ergibt sich für die Sequenzen die Interpretation, dass aus den Gliedern des Antecedens die Alternative aus den Gliedern des Succedens gefolgert werden kann.

Das Schema $\dfrac{A \rightarrow b,\, c,\, D}{A \rightarrow b \vee c,\, D}$ tritt an die Stelle des vorherigen Schemata-Paares ($\vee$, 2), welches in der erweiterten Form (mit einer Folge D im Succedens) lautet:

$$\frac{A \rightarrow b,\, D}{A \rightarrow b \vee c,\, D}, \qquad \frac{A \rightarrow c,\, D}{A \rightarrow b \vee c,\, D}.$$

Jedes dieser beiden Schemata gewinnt man leicht aus dem neuen; aber auch das neue gewinnt man aus diesen beiden, indem man, mit Hilfe von ihnen, aus $A \rightarrow b, c, D$ zuerst $A \rightarrow b \vee c, c, D$, sodann $A \rightarrow c, b \vee c, D$, weiter daraus $A \rightarrow b \vee c, b \vee c, D$, und schliesslich durch Zusammenziehung $A \rightarrow b \vee c, D$ erhält.

Die Implikation brauchten wir in der klassischen Aussagenlogik nicht als Grundverknüpfung zu nehmen; wir könnten sie hier explicite definieren durch die Festsetzung, dass für $\neg a \vee b$ allenthalben $a \supset b$, und auch umgekehrt, gesetzt werden kann.

[19] Das Schema der Einführung der Konjunktion im Antecedens und das dazu duale Schema für $\vee$ findet sich in der hier angegebenen Form wohl zuerst in der Abhandlung von OIVA KETONEN, *Untersuchungen zum Prädikaten-Kalkül*. Annales Acad. Scient Fennicae (Helsinki 1944), welche die Gentzen'schen Ueberlegungen weiterführt.

Nämlich die Schemata, welche im erweiterten Sequenzen-Kalkul den vorherigen Schematen $(\supset, 1)$, $(\supset, 2)$ entsprechen, lauten ja

$$\frac{A, b \rightarrow c, D}{A \rightarrow b \supset c, D}, \qquad \frac{A \rightarrow b, D; \quad A, c \rightarrow D}{A, b \supset c \rightarrow D}.$$

Ersetzt man in diesen beidemal $b \supset c$ durch $\neg b \vee c$, so sind die entstehenden Schemata mittels unserer neuen Schluss-Schemata für $\neg$ und $\vee$ ableitbar: In der Tat, mittels dieser Schluss-Schemata erhalten wir aus $A, b \rightarrow c, D$ zunächst $A \rightarrow \neg b, c, D$ und daraus $A \rightarrow \neg b \vee c, D$; und aus $A \rightarrow b, D$ erhalten wir zunächst $A, \neg b \rightarrow D$, und diese Sequenz zusammen mit $A, c \rightarrow D$ ergibt $A, \neg b \vee c \rightarrow D$.

Wir werden aber im Folgenden diese Möglichkeit der definitorischen Einführung der Implikation nicht benutzen, sondern die Implikation durch die beiden obigen Schemata charakterisieren.

Desgleichen wollen wir die Biimplikation, die ja, wie wir wissen, durch & und $\supset$, und im klassischen Kalkul gleichwertig durch &, $\vee$, $\neg$ definierbar ist, hier durch zwei Schemata charakterisieren, nämlich die beiden folgenden:

$$\frac{A, b \rightarrow c, D; \quad A, c \rightarrow b, D}{A \rightarrow b \equiv c, D}, \qquad \frac{A \rightarrow b, c, D; \quad A, b, c \rightarrow D}{A, b \equiv c \rightarrow D}.$$

Mittels des zweiten von diesen lassen sich insbesondere aus $a \rightarrow a$ und $b \rightarrow b$ (mit Anwendung von Verdünnungen und Vertauschungen) die Sequenzen $a \equiv b, a \rightarrow b$, $a \equiv b, b \rightarrow a$ herleiten. In den Schematen der Implikation und der Biimplikation ist wiederum die Bedingung der Paarung von Einführung im Succedens und Einführung im Antecedens erfüllt.

Die 10 neuen Schluss-Schemata für $\neg$, & $\vee$, $\supset$, $\equiv$, – sie mögen insgesamt mit "$\{\neg, \&, \vee, \supset, \equiv\}$" bezeichnet werden –, sind in Verbindung mit den Regeln des reinen Sequenzen-Kalkuls anzuwenden. Von diesen sollen nunmehr einige, insbesondere die Schnitte, *ohne Berufung auf den Gentzen'schen Hauptsatz*, als entbehrlich erwiesen werden. Dazu machen wir uns zunächst klar, dass alle mittels unserer 10 Schemata und des reinen Sequenzen-Kalkuls herleitbaren Sequenzen "aussagenlogisch wahre" Sequenzen sind.

Der Begriff "aussagenlogisch wahr" nimmt Bezug auf das beannte Verfahren der Auswertung von Aussagenformeln für einek

"Belegung" ihrer Primformeln mit "Wahrheitswerten". Eine Belegung besteht darin, dass jeder vorkommenden Primformel einer der Werte "wahr", "falsch", (kurz bezeichnet durch "v" und "f") zugewiesen wird, und zwar gleichgestaltigen Primformeln der gleiche Wert. Und die Auswertung einer aus Primformeln mit den Symbolen $\neg$, &, $\vee$, $\supset$, $\equiv$ gebildeten Formel geschieht successive nach den geläufigen Anweisungen: $\neg\, a$ erhält den Wert v dann und nur dann, wenn a den Wert f erhält, sonst den Wert f; $a \,\&\, b$ erhält den Wert v dann und nur dann, wenn a und b beide den Wert v erhalten, sonst den Wert f; $a \vee b$ erhält den Wert f dann und nur dann, wenn a und b beide den Wert f erhalten, sonst den Wert v; $a \supset b$ erhält den gleichen Wert wie $\neg\, a \vee b$, d.h. $a \supset b$ erhält den Wert f dann und nur dann, wenn a den Wert v und b den Wert f erhält, sonst den Wert v; $a \equiv b$ erhält den Wert v dann und nur dann, wenn a und b den gleichen Wert erhalten, sonst den Wert f.

Hiernach ergibt sich für jede Aussagenformel bei einer gegebenen Belegung ihrer verschiedenen Primformeln mit Wahrheitswerten eindeutig einer der Werte v, f. Eine *Formel* heisse nun *aussagenlogisch wahr*, wenn sie bei jeder Belegung der verschiedenen in ihr enthaltenen Primformeln den Wert v erhält. Eine Sequenz heisse durch eine Belegung der verschiedenen in ihr vorkommenden Primformeln "erfüllt", wenn durch diese mindestens eine der Formeln des Antecedens den Wert f oder mindestens eine der Formeln des Succedens den Wert v erhält. Und eine *Sequenz* heisse *aussagenlogisch wahr*, wenn sie durch jede Belegung der verschiedenen in ihr vorkommenden Primformeln mit Wahrheitswerten erfüllt wird. Nach dieser Festsetzung ist eine Sequenz $a_1, \ldots, a_m \rightarrow b_1, \ldots, b_n$ dann und nur dann aussagenlogisch wahr, wenn die Formel

$$((\ldots((a_1 \,\&\, a_2) \,\&\, a_3) \ldots) \,\&\, a_m) \supset ((\ldots((b_1 \vee b_2) \vee \ldots) \vee b_n)$$

aussagenlogisch wahr ist.

Aufgrund dieser Definitionen ergeben sich nun folgende Feststellungen:

1. Jede Sequenz der Gestalt $a \rightarrow a$ ist aussagenlogisch wahr.
2. Durch Anwendung einer Verdünnung, einer Vertauschung oder einer Zusammenziehung entsteht aus einer aussagenlogisch

wahren Sequenz stets wieder eine solche. Denn bei jedem dieser Prozesse kann sich die Gesamtheit der verschiedenen Formeln im Antecedens, sowie die entsprechende Gesamtheit im Succedens nur erweitern.

3. Durch Anwendung eines Schnittes auf zwei aussagenlogisch wahre Sequenzen $A, b \to C$; $A \to b, C$ erhält man wieder eine aussagenlogisch wahre Sequenz $A \to C$. Denn betrachten wir irgend eine Belegung der verschiedenen Primformeln aus A und C mit Wahrheitswerten, so können wir diese zu einer Belegung der Primformeln aus A, b, C ergänzen. Dabei erhält die Formel b einen der Werte v, f. Erhalt sie den Wert v, so muss, da ja die Sequenz $A, b \to C$ aussagenlogisch wahr ist, eine der Formeln aus A den Wert f oder eine der Formeln aus C den Wert v erhalten. Das Gleiche ergibt sich für den Fall, dass bei der betrachteten Belegung die Formel b den Wert f erhält, durch Betrachtung der aussagenlogisch wahren Sequenz $A \to b, C$. Somit wird die Sequenz $A \to C$ durch jede Belegung der in ihr vorkommenden Primformeln erfüllt, d.h. sie ist aussagenlogisch wahr.

4. Die Anwendung eines der Schemata $\{\neg, \&, \vee, \supset, \equiv\}$ auf eine aussagenlogisch wahre Sequenz, bezw. auf ein Paar solcher Sequenzen, ergibt wieder eine aussagenlogisch wahre Sequenz. Es mag genügen, das Verfahren der Begründung dieser Behauptung an einem der Fälle darzulegen. Nehmen wir das Schema

$$\frac{A \to b, D; \quad A, c \to D}{A, b \supset c \to D}$$

und betrachten wir eine Belegung der verschiedenen Primformeln von A, b, c, D mit Wahrheitswerten. Wenn bei dieser eine Formel aus der Folge A den Wert f oder eine Formel aus D den Wert v erhält, so wird die untere Sequenz durch die Belegung erfüllt. Ist aber keines von beiden der Fall, so muss, weil ja die beiden oberen Sequenzen aussagenlogisch wahr sind, die Formel b bei der Belegung den Wert v und c den Wert f erhalten; daher erhält $b \supset c$ bei der Belegung den Wert f. Somit wird die Sequenz $A, b \supset c \to D$ durch jede Belegung der in ihr vorkommenden Primformeln erfüllt, sie ist also aussagenlogisch wahr. Ganz entsprechend gestaltet sich die Ueberlegung für die übrigen 9 Schemata.

Aus den Feststellungen 1.–4. entnehmen wir im Ganzen das Ergebnis: Jede ausgehend von Sequenzen der Form $a \rightarrow a$ mit Hilfe der Schemata des reinen Sequenzen-Kalkuls und der Schemata $\{\neg, \&, \vee, \supset, \equiv\}$ herleitbare Sequenz ist aussagenlogisch wahr.

Nun können wir aber auch zeigen: Jede aus einem Bestande von Primformeln mit den Verknüpfungen $\neg, \&, \vee, \supset, \equiv$ gebildete, aussagenlogisch wahre Sequenz kann aus Formeln der Gestalt $a \rightarrow a$, worin a jeweils eine der Primformeln ist, mit Hilfe von Verdünnungen und Vertauschungen sowie der Schemata $\{\neg, \&, \vee, \supset, \equiv\}$ gewonnen werden.

Es werden also Sequenzen $a \rightarrow a$ nur für Primformeln a als Grundsequenzen (Ausgangsformeln) verwendet und von den Regeln des reinen Sequenzen-Kalkuls nur Verdünnungen und Vertauschungen, also weder Schnitte noch Zusammenziehungen, und mit diesen eingeschränkteren Mitteln sind bereits alle (aus dem Bestande von Primformeln gebildeten) aussagenlogisch wahren Sequenzen herleitbar und somit alle diejenigen Sequenzen, welche mit den zuvor genannten stärkeren Mitteln hergeleitet werden können.

Der Nachweis erfolgt durch eine Induktion nach der Anzahl der in der betreffenden Sequenz auftretenden Verknüpfungssymbole (jedes in seiner Vielfachheit gezählt).

Wenn eine Sequenz $A \rightarrow B$ gar kein Verknüpfungssymbol enthält, so besteht sowohl A wie B nur aus Primformeln, und die Sequenz kann nur dann aussagenlogisch wahr sein, wenn A und B mindestens eine Primformel gemeinsam haben; dann aber entsteht die Sequenz aus einer Grundsequenz $a \rightarrow a$ (nötigenfalls) durch Verdünnungen und Vertauschungen.

Sei nun in einer aussagenlogisch wahren Sequenz die Anzahl der Verknüpfungssymbole n, und nehmen wir an, dass für Sequenzen mit weniger als n Verknüpfungssymbolen unsere Behauptung schon erwiesen sei. Wir greifen dann nach Belieben eines der Glieder des Antecedens oder des Succedens heraus, welches mindestens ein Verknüpfungssymbol enthält. Dieses hat dann eine der Formen $\neg b$, $b \& c$, $b \vee c$, $b \supset c$, $b \equiv c$. Hiernach ergeben sich, da das Glied im Antecedens oder im Succedens stehen kann, 10 verschiedene zu betrachtende Fälle. In jedem dieser Fälle hat die Sequenz, nach

Anwendung eventuell von Vertauschungen, die Gestalt der unteren Sequenz von einem unserer 10 Schemata. Diese Schemata sind nun alle so beschaffen, dass

1. wenn die untere Sequenz aussagenlogisch wahr ist, auch die obere Sequenz, bezw. jede der beiden oberen Sequenzen, aussagenlogisch wahr ist,

2. die obere Sequenz, bezw. jede der beiden oberen Sequenzen weniger Verknüpfungssymbole enthält als die untere Sequenz.

Somit ist in dem Schema, dessen untere Sequenz unsere betrachtete Sequenz ist, die obere Sequenz (bezw. jede der beiden oberen Sequenzen) aufgrund unserer Induktionsannahme mit den zugelassenen Mitteln herleitbar, und das gleiche gilt daher von der unteren Sequenz, d.h. von unserer betrachteten Sequenz. Damit ist der gewünschte Nachweis erbracht.

Die Herleitungen mit den betrachteten Mitteln besitzen die Teilformel-Eigenschaft, dass jede Formel einer vorkommenden Sequenz Teilformel von einer Formel der Endsequenz ist. Und hieraus folgt insbesondere, dass diese Herleitungen *interne* sind (vgl. S. 25), d.h. dass in ihnen nur solche Verknüpfungssymbole auftreten, die in der Endsequenz vorkommen, und daher auch nur solche der Schemata $\{\neg, \&, \vee, \supset, \equiv\}$ zur Verwendung kommen, die zu einer in der Endsequenz auftretenden Verknüpfung gehören.

Unsere Beweisführung zeigt auch, dass man die Herleitungen durch ein ganz schematisches Verfahren, ohne Ueberlegung, gewinnt, wobei man übrigens im allgemeinen viele Möglichkeiten zur Auswahl hat. Es ergibt sich hier eine nahe Beziehung zu der Methode der semantischen Tafeln von E. W. BETH.[20]

Wenn wir den dual-symmetrischen Sequenzen-Kalkul nicht im Hinblick auf die Elimination der Schnitte anlegen, dann können wir, wie schon Gentzen hervorhob, die Regeln für die Verknüpfungen merklich vereinfachen. Anstelle der Schluss-Schemata genügen dann – sofern wir als Grundverknüpfungen nur $\neg$, &, $\vee$ nehmen – die folgenden 8 Grundsequenzen-Schemata:

$$a, \neg a \rightarrow \qquad\qquad \rightarrow \neg a, a$$

[20] Entwickelt in der Abhandlung: *Semantic Entailment and Formal Derivability*, Mitteilungen d. kgl. Niederland. Akad. d. Wissenschaften, Neue Reihe 18, (Amsterdam 1955) No. 13.

(entsprechend dem Satz vom Widerspruch und dem Satz vom ausgeschlossenen Dritten),

$$a \,\&\, b \rightarrow a, \quad a \,\&\, b \rightarrow b \qquad a \rightarrow a \vee b, \quad b \rightarrow a \vee b$$

$$a \vee b \rightarrow a, b \qquad\qquad a, b \rightarrow a \,\&\, b.$$

(Die links-seitig angegebenen Sequenzen enthalten jeweils das Verknüpfungs-Symbol im Antecedens, die rechts-seitig angegebenen im Succedens.)

Da wir hierbei wieder das Schnitt-Schema zur Verfügung haben, so brauchen wir neben diesem als Schluss-Regeln für den reinen Sequenzen-Kalkul nur die Verdünnung nach links und nach rechts, die jetzt aber auch auf das Succedens Anwendung findet. Die Sequenzen $a \rightarrow a$ werden jetzt herleitbar, da man aus den Grundsequenzen für die Negation durch Verdünnung die Sequenzen $a, \neg a \rightarrow a; a \rightarrow \neg a, a$ und aus diesen mittels des Schnitt-Schemas, in seiner symmetrischen Form, $a \rightarrow a$ erhält.

Nachträgliche Bemerkung. Wie ich nach der Abfassung der vorliegenden Arbeit bemerkte, wurde die in diesem Abschnitt 4. benutzte Methode der Gestaltung des symmetrischen Sequenzen-Kalkuls, sowie des Nachweises der Zulänglichkeit des Kalkuls (ohne Anwendung von Schnitten) in ganz ähnlicher Weise von HAO WANG angewandt, in seiner Abhandlung "Toward Mechanical Mathematics", IBM Journal for Research and Development, vol. 4, 1960; siehe § 2 und § 5.

5. Neuere Behandlung des einfachen Sequenzen-Kalkuls

Der Sequenzen-Kalkul in seiner dual-symmetrischen Form besitzt unstreitig eine grosse Vollkommenheit. Er ist auch in neuerer Zeit für die metamathematischen Untersuchungen, insbesondere von G. TAKEUTI, zur Verwendung gebracht worden.

Andrerseits ist Gentzen selbst bei seinem ersten Widerspruchsfreiheitsbeweis für die Zahlentheorie [21] auf den unsymmetrischen Sequenzen-Kalkul mit eingliedrigem Succedens zurückgekommen. Für seinen zweiten Widerspruchfreiheitsbeweis hat er wieder den

[21] Die Widerspruchsfreiheit der reinen Zahlentheorie. Math. Annalen **112** (1936) Siehe insbesondere § 5, S. 511–517.

symmetrischen Sequenzen-Kalkul verwendet; jedoch findet sich hier bei seinen einleitenden Bemerkungen das Zugeständnis, dass der "neue Sequenz-Begriff" (gemeint ist derjenige des symmetrischen Sequenzen-Kalkuls) "schon ein Abrücken vom 'Natürlichen' bedeutet". [22] Ein Moment des nicht Natürlichen liegt in der Tat darin, dass der Dualismus, welcher eine Eigenschaft der klassischen Aussagen- und Prädikaten-Logik ist, hier auf die Formalisierung des Beweisbarkeits-Begriffes ausgedehnt wird. Der Sequenzen-Kalkul ist ja nicht eine direkte formale Axiomatisierung der logischen Gesetze, sondern eine Formalisierung einer Metalogik, welche die logischen Beziehungen im Rahmen einer allgemeinen Folgerungstheorie betrachtet. Diese Folgerungstheorie hat aber von sich aus keine duale Symmetrie, wenn man ihr nicht eigens eine solche aufprägt.

Jedenfalls behält, trotz der grossen formalen Vorzüge des symmetrischen Sequenzen-Kalkuls, doch der für die Interpretation einfachere unsymmetrische Sequenzen-Kalkul seine methodische Bedeutung. Wir wollen diesen im Unterschied von dem symmetrischen Kalkul, den "einfachen Sequenzen-Kalkul" nennen.

Im einfachen Sequenzen-Kalkul ist die klassische Aussagen-Logik repräsentiert durch die Gesamtheit der aussagenlogisch wahren Sequenzen mit eingliedrigem Succedens und ihrer Herleitungen. Für den intuitionistischen Aussagen-Kalkul haben wir beim einfachen Sequenzen-Kalkul die in den Abschnitten 2. und 3. aufgestellten Systeme von Sequenzen-Schematen und Schluss-Schematen als Mittel der Herleitungen zur Verfügung.

Nun besteht im Rahmen des einfachen Sequenzen-Kalkuls die Möglichkeit, den Uebergang vom intuitionistischen zum klassischen Aussagen-Kalkul durch Hinzunahme eines einzigen Grundsequenzen-Schemas zu bewirken. Hierfür können die älteren Ergebnisse betreffend den *Formel-Kalkul* verwertet werden. Aus diesen entnimmt man zunächst, dass es hier genügt, die Schemata für Implikation und Negation zu ergänzen, während für die übrigen

[22] Neue Fassung des Widerspruchfreiheitsbeweises für die reine Zahlentheorie, Forschungen zur Logik ... neue Folge, Heft 4 (Leipzig 1938) S. 21.

Verknüpfungen die Formel-Schemata des intuitionistischen Kalkuls beibehalten werden können.[23]

Bezüglich der Ergänzung des intuitionistischen Kalkuls für Implikation und Negation zum klassischen ergibt sich aus den Untersuchungen von FREGE und ŁUKASIEWICZ, dass es hierfür ausreichend ist, eines der Formel-Schemata $\neg\neg p \supset p$, $(\neg p \supset p) \supset p$ hinzuzufügen.[24]

Anstatt des letzteren genügt ebenfalls (wie man auch schon seit längerem weiss): [25] $((p \supset \neg p) \supset p) \supset p$. Dieses wiederum ist ein Spezialfall des Schemas der Formel von PEIRCE $((p \supset q) \supset p) \supset p$, welches also a fortiori ausreichend ist.

Wünscht man das deduktive System des Formel-Kalkuls so anzulegen, dass alle Formeln, welche nur die Implikation enthalten, auch ohne Einführung einer anderen Verknüpfung herleitbar sind, so wird man dieses Formel-Schema vor den anderen genannten bevorzugen.[26] Andrerseits hat das Formel-Schema $\neg\neg p \supset p$, als Ausdruck des logischen Gesetzes, dass die doppelte Verneinung einer Aussage ihre Bejahung ergibt, insofern seinen Vorzug vor demjenigen der Peirce'schen Formel, als diese kaum überhaupt sprachlich so formuliert werden kann, dass sie als ein Gesetz der "wenn-so"-Verknüpfung einsichtig ist.

[23] Im Anschluss an das Vorangehende betrachten wir hier auch den Formel-Kalkul als einen solchen mit Formel-Schematen, wobei der modus ponens die einzige Schlussregel ist, also die Einsetzungen wegfallen. Von dem Kalkul mit Einsetzungen und modus ponens gelangt man zu diesem, indem man in den Herleitungen die Einsetzungen in die Ausgangsformeln zurückverlegt, was (gemäss einer bekannten einfachen Ueberlegung) stets möglich ist.

[24] Vgl. hierzu J. ŁUKASIEWICZ und A. TARSKI, Untersuchungen über den Aussagenkalkül, Comptes Rendus de Varsovie **23** (1930) Classe III, Fussnote 9, sowie Satz 6 (beides auf S. 6).

[25] Siehe in dem Enzyklopädie-Bericht: Mathematische Logik, von H. HERMES und H. SCHOLZ, Abschnitt 6., S. 37, die Feststellung "(g)".

[26] Dieser Gesichtspunkt wird z.B. hervorgehoben in der Abhandlung von STIG KANGER, A note on Partial Postulate Sets for Propositional Logic, Theoria **21** (1955) 99–104. Der reine implikative Kalkul der klassischen Aussagenlogik wurde schon sehr frühzeitig behandelt. Vgl. hierüber in der in Fussnote 24 zitierten Abhandlung von Łukasiewicz und Tarski, Satz 29 (S. 14).

Die Sachlage erhält noch eine anderweitige Beleuchtung durch den Uebergang vom Formel-Kalkul zum Sequenzen-Kalkul. Durch den Vergleich der beiden Arten des Kalkuls werden wir darauf aufmerksam, dass im Formel-Kalkul die Implikation gewissermassen in zwei verschiedenen Rollen auftritt: einerseits als Verknüpfung, andrerseits in der Rolle der Sequenz-Beziehung. Beim Sequenzen-Kalkul wird die Vorrangstellung der Implikation aufgehoben. So treten hier an die Stelle der Formel-Schemata $\neg\neg p \supset p$ und $((p \supset q) \supset p) \supset p$ die Sequenz-Schemata

$$\neg\neg p \to p, \qquad (p \supset q) \supset p \to p;$$

im ersten kommt als Verknüpfung nur die Negation, im zweiten nur die Implikation vor.

Man kann sich nun fragen, ob noch für eine andere unserer fünf Verknüpfungen ein Sequenz-Schema mit dieser Verknüpfung als der allein auftretenden angegeben werden kann, welches hinzugefügt zu dem intuitionistischen Sequenzen-Kalkul den klassischen Sequenzen-Kalkul, im Rahmen des einfachen Sequenzen-Kalkuls liefert. Diese Frage wurde neuerdings in einem Kreis von Logikern aufgeworfen und dahin beantwortet: [27]

1. Für die Verknüpfungen & und v gibt es kein derartiges Sequenzen-Schema, und es gilt sogar der Satz: Jede aussagenlogisch wahre Sequenz, worin & und v die alleinigen Verknüpfungssymbole sind, ist mittels der Schemata für & und v des *intuitionistischen* Sequenzen-Kalkuls, (im Rahmen des reinen Sequenzen-Kalkuls) herleitbar, oder wie wir es kurz ausdrücken wollen "im intuitionistischen &-v-Sequenzen-Kalkul herleitbar".

2. Für die Biimplikation gibt es Sequenzen-Schemata von der geforderten Eigenschaft.

Der Nachweis für den unter 1. ausgesprochenen Satz kann man aus den folgenden Feststellungen entnehmen:

a. Jede mit & und v als alleinigen Verknüpfungen gebildete

[27] Vgl. HUGUES LEBLANC, Etudes sur les règles d'inférence dites règles de Gentzen, Dialogue, Canadian Philosophical review **1**, No. 1 (Bruges 1962), ferner H. LEBLANC, Proof Routines for the Propositional Calculus, Notre Dame Journal of Formal Logic **4**, No. 2 (1963), sowie die in diesen Abhandlungen zu findenden Literaturangaben.

Sequenz des einfachen Sequenzen-Kalkuls ist aufgrund des intuitionistischen &-v-Sequenzen-Kalkuls ableitungsgleich mit einer solchen Sequenz, die ein eingliedriges Antecedens hat und worin sowohl die Formel im Antecedens wie diejenige im Succedens eine "konjunktive Normalform" ist; dabei bedeutet hier eine konjunktive Normalform eine solche Konjunktion, deren Glieder Alternativen (v-Verknüpfungen) aus Primformeln sind.

b. Sind p und q konjunktive Normalformeln (der eben genannten Art), so ist die Sequenz $p \rightarrow q$ dann und nur dann aussagenlogisch wahr, wenn für jedes Konjunktionsglied c von q die Bedingung erfüllt ist, dass bei mindestens einem der Konjunktionsglieder von p alle darin auftretenden Primformeln auch in c vorkommen.

c. Eine Sequenz $p \rightarrow q$, welche dem in b. angegebenen Kriterium genügt, ist mittels des intuitionistischen &–v–Sequenzen-Kalkuls herleitbar.

Was den Punkt 2. betrifft, so lässt sich das von H. Leblanc und N. D. Belnap erhaltene Ergebnis in gewisser Hinsicht verbessern. Sie geben das Paar der Sequenzen-Schemata:

$$a, (c \equiv a) \equiv (c \equiv b) \rightarrow b, \qquad ((1))$$

$$a, (c \equiv b) \equiv (c \equiv a) \rightarrow b \qquad ((2))$$

als ein solches an, welches an die Stelle des Paares $(\equiv_{\text{I}})$, $(\equiv_{\text{II}})$, d.h. $a \equiv b, a \rightarrow b$; $a \equiv b, b \rightarrow a$ (vgl. S. 25) gesetzt, den intuitionistischen Sequenzen-Kalkul zum klassischen ergänzt.[28] Als Schema der Einführung von $\equiv$ im Succedens nehmen sie dabei $(\equiv, 1)$.

Hier kann zunächst ((2)) aus ((1)) abgeleitet werden. Nämlich mittels des Schemas $(\equiv, 1)$ und des reinen Sequenzen-Kalkuls ergeben sich zunächst die Sequenzen:

$$\rightarrow a \equiv a, \qquad c \rightarrow (a \equiv a) \equiv c \qquad ((3))$$

$$c \rightarrow c \equiv (b \equiv b). \qquad ((4))$$

[28] Die Fragestellung ist bei diesen Autoren insofern etwas verschieden von der hier erörterten, als sie den Uebergang vom intuitionistischen zum klassischen Kalkul nicht durch Hinzufügung eines Sequenzen-Schemas sondern durch *Verstärkung* eines der Schemata bewirken wollen, womit sie beabsichtigen, die vorherige Zweiheit von "Einführung" und "Beseitigung" aufrecht zu erhalten.

Nun liefert das Sequenzen-Schema ((1)) durch Spezialisierungen:

$$a,\ (a \equiv a) \equiv (a \equiv b) \rightarrow b \qquad ((5))$$

$$a,\ (b \equiv a) \equiv (b \equiv b) \rightarrow b. \qquad ((6))$$

Als Spezialfall von ((3)) erhalten wir:

$$a \equiv b \rightarrow (a \equiv a) \equiv (a \equiv b), \qquad ((7))$$

als Spezialfall von ((4)):

$$b \equiv a \rightarrow (b \equiv a) \equiv (b \equiv b). \qquad ((8))$$

((7)) and ((5)) liefern durch Verdünnungen und Schnitt:

$$a \equiv b,\ a \rightarrow b;$$

((8)) and ((6)) liefern gleichermassen: $b \equiv a,\ a \rightarrow b$ und somit auch $a \equiv b,\ b \rightarrow a$. Und so bekommen wir unsere Schemata ($\equiv_{\mathrm{I}}$) und ($\equiv_{\mathrm{II}}$) zurück. Diese beiden zusammen liefern mittels ($\equiv$, 1) die Sequenz: $a \equiv b \rightarrow b \equiv a$ und somit in spezieller Anwendung $(c \equiv b) \equiv (c \equiv a) \rightarrow (c \equiv a) \equiv (c \equiv b)$. Und diese Sequenz zusammen mit ((1)) liefert durch Verdünnungen, Vertauschung und Schnitt die Sequenz ((2)).

Wie wir zugleich sehen, sind aus den Schematen ($\equiv$, 1) und ((1)) die Schemata ($\equiv_{\mathrm{I}}$), ($\equiv_{\mathrm{II}}$) des intuitionistischen Sequenzen-Kalkuls ableitbar. Es lässt sich nun von dem Schema ((1)) gewissermassen der nicht-intuitionistische Bestandteil absondern, in der Form des Sequenzen-Schemas

$$a \equiv (a \equiv b) \rightarrow b\ . \qquad (\equiv^*)$$

In der Tat ist, bei Zugrundelegung der früheren Schemata ($\equiv$, 1), ($\equiv_{\mathrm{I}}$), ($\equiv_{\mathrm{II}}$) (und des reinen Sequenzen-Kalkuls), aus ((1)) das Schema ($\equiv^*$) und aus diesem auch ((1)) ableitbar.

Um ($\equiv^*$) aus ((1)) zu gewinnen, leitet man zunächst mittels der genannten intuitionistischen Schemata für $\equiv$ folgende Sequenzen ab:

$$\rightarrow a \equiv a, \qquad c \equiv (a \equiv a) \rightarrow c, \qquad c \rightarrow c \equiv (a \equiv a).$$

$$\rightarrow (c \equiv (a \equiv a)) \equiv c,$$

$$p \equiv q \rightarrow (p \equiv r) \equiv (q \equiv r),$$

und aus diesen die Sequenz

$$\rightarrow ((c \equiv (a \equiv a)) \equiv (c \equiv b)) \equiv (c \equiv (c \equiv b)),$$

die abgekürzt mit $\rightarrow u \equiv (c \equiv (c \equiv b))$ angegeben werde. Nun wird das Schema ((1)) in der Spezialisierung

$$a \equiv a, (c \equiv (a \equiv a)) \equiv (c \equiv b) \rightarrow b$$

angewandt. Hier kann das erste Glied des Antecedens, wegen $\rightarrow a \equiv a$, durch einen Schnitt entfernt werden, sodass man $u \rightarrow b$ erhält. Das Sequenz-Schema ($\equiv_{\text{II}}$) liefert:

$$u \equiv (c \equiv (c \equiv b)), c \equiv (c \equiv b) \rightarrow u,$$

und diese Sequenz, zusammen mit den erhaltenen: $\rightarrow u \equiv (c \equiv (c \equiv b))$ und $u \rightarrow b$, ergibt durch Verdünnungen und zweimaligen Schnitt: $c \equiv (c \equiv b) \rightarrow b$, worin noch a für c genommen werden kann.

Um andrerseits ((1)) aus ($\equiv^*$) zu gewinnen, beginne man mit der Herleitung der Sequenz

$$a \rightarrow (c \equiv a) \equiv c, \qquad ((9))$$

welche mittels ($\equiv_{\text{II}}$) und ($\equiv$, 1) erfolgt. Aus der bereits erhaltenen Sequenz

$$p \equiv q \rightarrow (p \equiv r) \equiv (q \equiv r)$$

ergibt sich durch spezielle Anwendung

$$(c \equiv a) \equiv c \rightarrow ((c \equiv a) \equiv (c \equiv b)) \equiv (c \equiv (c \equiv b)).$$

Diese Sequenz zusammen mit ((9)) ergibt durch einen Schnitt:

$$a \rightarrow ((c \equiv a) \equiv (c \equiv b)) \equiv (c \equiv (c \equiv b)).$$

Wird diese Sequenz abgekürzt, durch $a \rightarrow u \equiv v$ angegeben, so ist die herzuleitende Sequenz ((1)): $a, u \rightarrow b$. Nun haben wir hergeleitet $a \rightarrow u \equiv v$; nach ($\equiv_{\text{I}}$) haben wir $u \equiv v, u \rightarrow v$, und nach ($\equiv^*$): $v \rightarrow b$. Diese drei Sequenzen ergeben durch Verdünnung, Vertauschung und zweimaligen Schnitt: $a, u \rightarrow b$.

Anstatt also das Paar der Sequenzen-Schemata ($\equiv_{\text{I}}$), ($\equiv_{\text{II}}$) zu dem Schema ((1)) zu verstärken, können wir das Schema ($\equiv^*$) hinzufügen.

Dieses Schema ist in seiner inhaltlichen Auffassung einigermassen ansprechend; es besagt ja: "Daraus dass a genau dann, wenn a genau dann wenn b, kann entnommen werden, dass b".

Die Rolle des Schemas $(\equiv^*)$ im Sequenzen-Kalkul der Biimplikation beruht insbesondere darauf, dass aus ihm das assoziative Gesetz

$$a \equiv (b \equiv c) \rightarrow (a \equiv b) \equiv c \tag{(10)}$$

herleitbar ist. Nämlich aufgrund von $(\equiv, 1)$ erhält man ja ((10)) aus den Sequenzen

$$a \equiv (b \equiv c), a \equiv b \rightarrow c$$

und

$$a \equiv (b \equiv c), c \rightarrow a \equiv b. \tag{(11)}$$

Die erste von diesen wird (durch Verdünnungen und Schnitt) aus den Sequenzen

$$a \equiv (b \equiv c), a \equiv b \rightarrow b \equiv (b \equiv c) \text{ und } b \equiv (b \equiv c) \rightarrow c$$

gewonnen, von denen die erste sich aus $(\equiv, 1)$, $(\equiv_{\mathrm{I}})$ und $(\equiv_{\mathrm{II}})$ ergibt, während die zweite eine Anwendung von $(\equiv^*)$ ist. ((11)) wird aus den Sequenzen

$$a \equiv (b \equiv c), c, a \rightarrow b, \qquad a \equiv (b \equiv c), c, b \rightarrow a$$

erhalten, die sich bereits aus $(\equiv, 1)$, $(\equiv_{\mathrm{I}})$ und $(\equiv_{\mathrm{II}})$ ergeben.

Die Umkehrung von ((10))

$$(a \equiv b) \equiv c \rightarrow a \equiv (b \equiv c) \tag{(12)}$$

erhält man aus ((10)) durch mehrmalige Anwendung der Sequenz $p \equiv q \rightarrow q \equiv p$, welche sich aus $(\equiv, 1)$, $(\equiv_{\mathrm{I}})$, $(\equiv_{\mathrm{II}})$ ergibt und aus der man auch

$$\rightarrow (a \equiv b) \equiv (b \equiv a) \tag{(13)}$$

gewinnt. ((10)) und ((12)) zusammen liefern:

$$\rightarrow (a \equiv (b \equiv c)) \equiv ((a \equiv b) \equiv c). \tag{(14)}$$

Nach der Herleitung von ((10)), ((12)), ((13)), ((14)) ist es nicht mehr schwer zu zeigen, dass für den klassischen Kalkul der Biimplikation die Schemata ($\equiv$, 1), ($\equiv_{\mathrm{I}}$), ($\equiv_{\mathrm{II}}$), ($\equiv$*) ausreichen.[29]

Im Sequenzen-Kalkul mit Biimplikation und Negation können wir bei Anwesenheit des Schemas ($\equiv$*) die Sequenzen der Form $\neg \neg b \rightarrow b$ aus den beiden intuitionistischen Schematen der Negation $\dfrac{A, b \rightarrow \neg b}{A \rightarrow \neg b}$; $a, \neg a \rightarrow b$ und denjenigen für die Biimplikation herleiten.

Nämlich zunächst erhalten wir aus ($\equiv_{\mathrm{II}}$): $\neg a \equiv a, a \rightarrow \neg a$ und daraus (mittels des eben genannten Schlussschemas der Negation): $\neg a \equiv a \rightarrow \neg a$, also auch $\neg \neg a, \neg a \equiv a \rightarrow \neg a$. Ferner erhalten wir durch Anwendung des Schemas $a, \neg a \rightarrow b$: $\neg \neg a, \neg a \rightarrow \neg a \equiv a$. Die beiden erhaltenen Sequenzen ergeben gemäss ($\equiv$,1): $\neg \neg a \rightarrow \neg a \equiv (\neg a \equiv a)$. Andrerseits haben wir als Anwendung von ($\equiv$*): $\neg a \equiv (\neg a \equiv a) \rightarrow a$. Somit erhalten wir, mit einem Schnitt: $\neg \neg a \rightarrow a$.

Desgleichen kann man – was hier nicht genauer ausgeführt sei – aus dem Schema $\neg \neg a \rightarrow a$ das Schema ($\equiv$*) mittels der intuitionistischen Schemata für die Biimplikation und die Negation ableiten.

Generell lässt sich von den drei Sequenzen-Schematen

$$\neg \neg a \rightarrow a, \qquad (a \supset b) \supset a \rightarrow a, \qquad a \equiv (a \equiv b) \rightarrow b$$

zeigen, dass je zwei von ihnen ableitungsgleich sind im Rahmen des intuitionistischen Sequenzen-Kalkuls für die beiden in ihnen auftretenden Verknüpfungen. Jedes der drei Schemata für sich ergänzt den vollen intuitionistischen Sequenzen-Kalkul zum einfachen klassischen Sequenzen-Kalkul.

Nach dem Vorschlag von Leblanc sollten wir jedes von diesen Schematen mit dem Schema der "Beseitigung" (Einführung im Antecedens) des in ihm auftretenden Verknüpfungs-Symbols zu einem verstärkten Beseitigungs-Schema vereinigen.[30] Seine An-

[29] Man vergleiche hierzu die Ausführungen über den Formel-Kalkul der Biimplikation bei A. Church, *Introduction to Mathematical Logic*, Vol. 1 (Princeton 1956) Exercise 26, p. 143.

[30] Er betrachtet ein System, das durch solche Verkopplungen von Schematen aus einem unserem System [&, v, $\supset$, $\neg$, $\equiv$] (vgl. Ende von Abschnitt 2, S. 20) gleichwertigen System des intuitionistischen Sequenzen-

sicht, dass dieses Verfahren sachgemäss ist, entspringt gewiss zum Wesentlichen aus der Feststellung, dass bei seiner Wahl der Schemata der Negation für den intuitionistischen Sequenzenkalkul, nämlich: $\dfrac{A, b \to c; \quad A, b \to \neg c}{A \to \neg b}$; $a, \neg a \to b$, die Hinzufügung des Schemas $\neg\neg a \to a$ eine Abhängigkeit ergibt, indem das Schema $a, \neg a \to b$ ableitbar wird.

Eine solche Abhängigkeit tritt jedoch nicht ein, wenn man das Schema $\dfrac{A, b \to c; \quad A, b \to \neg c}{A \to \neg b}$ durch das speziellere, von uns benutzte: $\dfrac{A. b \to \neg b}{A \to \neg b}$ ersetzt, welches zusammen mit $a, \neg a \to b$ jenes allgemeinere Schema (mit Hilfe von Verdünnungen und Schnitten) abzuleiten gestattet.

Dass in der Tat das Schema $a, \neg a \to b$ aus den übrigen Schematen des Systems [&, v, $\supset$, $\neg$, $\equiv$], auch bei Hinzunahme des Schemas $\neg\neg a \to a$, nicht abgeleitet werden kann, lässt sich durch die Methode der endlichen Modelle (Matrix-Methode) zeigen, die sich auf den Sequenzenkalkul in der Weise anwenden lässt, dass man einer jeden Sequenz $a_1, \ldots, a_n \to b$ die Formel $(\ldots (a_1 \,\&\, a_2) \,\&\, \ldots) \,\&\, a_n \supset b$ zuordnet.

Es wird ein Modell mit 5 Werten $\alpha, \beta, \gamma, \delta, \varepsilon$ genommen, von denen α der "ausgezeichnete" Wert ist. Über dem Bereich dieser 5 Werte werden &, v, $\supset$, $\equiv$, $\neg$ als Funktionen definiert durch folgende Festsetzungen:

Für beliebige Werte x, y gilt:[31]

kalkuls hervorgeht. Für dieses System des einfachen klassischen Sequenzenkalkuls beweist er, dass mittels der Schemata von diesem jede aussagenlogisch wahre Sequenz durch eine *interne* Herleitung erhalten werden kann. Den Nachweis hierfür erbringt er durch eine Diskussion der verschiedenen Teilsysteme der Kalküle, die den möglichen Beschränkungen der Verknüpfungen auf eine Teilgesamtheit entsprechen. Dieses Verfahren ist erheblich mühsamer als dasjenige, durch welches sich, wie wir (im Abschnitt 4) sahen, für den symmetrischen Sequenzenkalkul die Möglichkeit der schnittfreien Herleitungen zeigen lässt.

[31] Das Zeichen "=" steht in üblicher Weise zur Angabe von Wertgleichheit.

$$x \,\&\, y = y \,\&\, x, \quad x \vee y = y \vee x, \quad x \equiv y = y \equiv x;$$
$$x \,\&\, x = x, \quad x \vee x = x, \quad x \supset x = \alpha, \quad x \equiv x = \alpha;$$
$$\alpha \,\&\, x = x, \quad \alpha \vee x = \alpha, \quad \alpha \supset x = x, \quad x \supset \alpha = \alpha, \quad \alpha \equiv x = x;$$
$$\beta \,\&\, x = \beta, \quad \beta \vee x = x, \quad \beta \supset x = \alpha;$$
$$\text{für } x \neq \beta \text{ ist } x \supset \beta = \beta, \quad x \equiv \beta = \beta.$$

Im übrigen ist:

$$\gamma \,\&\, \delta = \varepsilon, \quad \gamma \,\&\, \varepsilon = \varepsilon, \quad \delta \,\&\, \varepsilon = \varepsilon; \quad \gamma \vee \delta = \alpha, \quad \gamma \vee \varepsilon = \gamma, \quad \delta \vee \varepsilon = \delta;$$
$$\gamma \supset \delta = \delta, \quad \delta \supset \gamma = \gamma, \quad \gamma \supset \varepsilon = \delta, \quad \delta \supset \varepsilon = \gamma, \quad \varepsilon \supset \gamma = \alpha, \quad \varepsilon \supset \delta = \alpha;$$
$$\gamma \equiv \delta = \varepsilon, \quad \gamma \equiv \varepsilon = \delta, \quad \delta \equiv \varepsilon = \gamma;$$
$$\neg\, \alpha = \beta, \quad \neg\, \beta = \alpha, \quad \neg\, \gamma = \delta, \quad \neg\, \delta = \gamma, \quad \neg\, \varepsilon = \alpha.$$

Aufgrund dieser Festsetzungen erhält jede Formel bei einer Belegung der Primformeln mit Werten aus der Menge $\{\alpha, \beta, \gamma, \delta, \varepsilon\}$ einen Wert aus dieser Menge; desgleichen erhält auch jede Sequenz einen solchen Wert, indem wir als Wert der Sequenz den Wert der zugeordneten Formel erklären.

Betrachten wir nun die Sequenzen-Schemata und Schluss-Schemata des Systems $[\&, \vee, \supset, \neg, \equiv]$, jedoch unter Ersetzung des Schemas $a, \neg\, a \rightarrow b$ durch das Schema $\neg\,\neg\, a \rightarrow a$, so können wir feststellen, dass jede gemäss einem der Sequenzen-Schemata gebildete Sequenz bei jeder Belegung ihrer Primformeln den ausgezeichneten Wert α erhält und dass bei jeder Anwendung eines der Schluss-Schemata die Bedingung erfüllt ist, dass jede Belegung der auftretenden Primformeln, für welche die obere Sequenz (bezw. jede der oberen Sequenzen) den Wert α erhält, auch für die untere Sequenz den Wert α liefert; und das Gleiche gilt für die Schemata der Verdünnung und des Schnittes. Hieraus ergibt sich, dass jede mittels der betrachteten Schemata herleitbare Sequenz bei jeder Belegung ihrer Primformeln den Wert α erhält. Dieses ist dagegen nicht der Fall bei dem Schema $a, \neg\, a \rightarrow b$. Werden hier für a und b verschiedene Primformeln p, q genommen und belegt man p mit den Wert γ, q mit dem Wert β, so erhält die der Sequenz $p, \neg\, p \rightarrow q$ zugeordnete Formel $(p \,\&\, \neg\, p) \supset q$ den Wert $(\gamma \,\&\, \neg\, \gamma) \supset \beta = (\gamma \,\&\, \delta) \supset \beta = \varepsilon \supset \beta = \beta$.

Hiermit wollen wir unsere Betrachtungen abschliessen, obwohl es noch mancherlei Anknüpfendes zu erörtern gäbe.[32] Nur die Bemerkung möge noch vorgebracht werden, dass mit diesen Ausführungen nicht die Meinung vertreten werden soll, dass der Sequenzenkalkul unbedingt dem Formelkalkul überlegen sei. Was insbesondere den Gentzen'schen Hauptsatz betrifft, so hat ja KURT SCHÜTTE gezeigt, dass man auch beim Formelkalkul die Möglichkeit hat, die Schlussregeln so zu wählen, dass die dem Schnitt entsprechende Schlussweise – (sie wird auch hier als "Schnittschema" bezeichnet) – eliminiert werden kann; und zwar besteht diese Möglichkeit hier ebenfalls sowohl für den klassischen wie für den intuitionistischen Kalkul.[33]

Für die Logik aufgefasst als Folgerungstheorie hat der Sequenzenkalkul sein Gutes durch seine Beziehung zum Annahmenkalkul und durch die Möglichkeit, im reinen Sequenzenkalkul die allgemeinsten Gesetze des Folgerns darzustellen. Unter diesen Gesichtspunkten hat der einfache Sequenzenkalkul als das methodisch natürlichere Verfahren gegenüber dem symmetrischen den Vorzug, während andrerseits erst in der symmetrischen Form der Sequenzenkalkul seine volle formale Abrundung und Eleganz erreicht, – wobei man sich allerdings von vornherein an die Gesetzlichkeit der klassischen Logik zu binden hat. –

Zürich, Schweiz

[32] Für das Studium der angrenzenden Fragen sei speziell hingewiesen auf das neue Werk von HASKELL B. CURRY, *Foundations of Mathematical Logic* (McGraw Hill 1963).

[33] Siehe K. SCHÜTTE, Schlussweisen-Kalküle der Prädikatenlogik. Math. Annalen Bel. **122** (1950) 47–65. Eine eingehende Behandlung des zu dem schnittfreien Sequenzen-Kalkul analogen Formelkalkuls enthält das Lehrbuch von H. ARNOLD SCHMIDT, Mathematische Gesetze der Logik I. Grundlehren d. math. Wissenschaften 69. Band (1960).

CHAPTER 2

REMARKS ON INFERENTIAL DEDUCTION

HASKELL B. CURRY

Gentzen in his thesis [9] proposed a formulation of logical calculus in which certain inferential rules played an essential role. The term "inferential deduction" is here used to describe deduction based on inferential rules of Gentzen type. A number of persons have studied such deduction; some of these, such as Jaśkowski, arrived at similar formulations independently.[1]

A considerable study of inferential deduction appears in my book [4]. The manuscript of that book was sent to the publishers on May 20, 1961. In the meanwhile there have appeared several publications dealing with inferential deduction; these have suggested ideas which, naturally, I was not able to take account of there. Among these publications are Anderson and Johnstone [1], – an elementary textbook which is the first serious attempt to present certain forms of inferential deduction to comparatively immature students, – and the defence of the dialogue approach to logical calculus in Lorenzen [14] and [13]. This paper contains some comments which these publications have suggested.

The paper deals with three rather loosely connected topics. Anderson and Johnstone present the Gentzen "natural" rules in a linear form such as Jaśkowski [10] and Fitch [8] used before them; it is easy to see that this formulation is, in principle, equivalent to the formulation by proof trees which Gentzen used; however, for certain rather technical purposes it is desirable to have an explicit process or algorithm for transforming either of these forms of proof into the other. This technicality is my first topic; it is expounded

[1] For the history and references see [5].

in § 2 after a preliminary result in § 1. The second topic is the semantics of Gentzen L-rules, about which it seems necessary (because some improvements in my previous explanations seem possible) to make some remarks in § 3. The rest of the paper deals with a form of dialogue differing in some details from those of Lorenzen (l.c.), Stegmüller [16], and Lorenz [11].[2] The second and third parts of the paper are quite independent of the first.[3]

1. Construction trees

The natural rules of Gentzen have the peculiarity that some of the rules involve discharge of assumptions. Before embarking on an analysis of such trees, I shall review some facts concerning proof trees for systems where the rules are of the ordinary sort.

In [4] secs. 2A6 and 3B1 [4] there is a discussion of constructions and the tree diagrams associated with them. This discussion will be supposed known here. I shall call such trees *construction trees*. I shall consider only the case where the construction trees are proof trees, i.e., the nodes are statements and the junctions are inferences by virtue of the rules. Further, I shall restrict attention to the case of an assertional system, with the elementary statements of the form

$$\vdash A, \tag{1}$$

where A is a kind of ob called a *formula*.[5] A proof tree is such a construction tree if the inferential rules are elementary, i.e. schemes of the form

$$A_1, \ldots, A_m \vdash B,$$

where $A_1, \ldots, A_m, B$ are elementary formula schemes (which be-

[2] It has been impossible for me to see a copy of this thesis in time to use it in the preparation of the present paper. I know it only second hand from information given in Stegmüller [16].

[3] The reference to § 2 in § 6 does not require previous reading of § 2.

[4] This treatment is revised from earlier treatments in [3] and [7] § 2B1.

[5] In my previous publications I used the term "proposition" in this sense. The shift to "formula" is an experiment. Perhaps it will serve to emphasize that I am talking about an object rather than an abstraction. Cf. [4] pp. 168–172.

come elementary formulas when the U-variables are specialized to specific formulas).

The normal construction sequence defined in [4] Sec. 2A6 constitutes a form of linearization of a construction sequence. Such a sequence may be defined as follows. If X and Y are nodes, we say X is to the left of Y just when the branch leading down from X lies to the left of that from Y at the node where they join. Then X precedes Y in the normal sequence just when X is over Y or X is to the left of Y.

This definition can be extended to proofs which contain superfluous formulas. Such proofs give rise to construction trees with detached parts. We can then say that if X and Y are nodes in different detached parts, X is to the left of Y when the terminus of the part in which X lies is to the left of that in which Y lies.

A normal construction sequence of course contains repetitions. We can get a construction sequence without superfluous repetitions as follows. Let us say that nodes X and Y are *homologous* just when (i) X and Y are initial (top) nodes which are alike, or (ii) X and Y are obtained by inferences such that the same rule is used in both cases and the premises are respectively homologous. Then the linearization is an assignment of nodes to the lines $N_1, \ldots, N_n$, such that when those assigned to $N_1, \ldots, N_m$ have been determined, the first not previously assigned node in the normal construction sequence together with all nodes homologous to it are assigned to N_{m+1}. Then a node will not be assigned to N_{m+1} until all those preceding it have been assigned to some of the $N_1, \ldots, N_m$. The process will stop when we reach an N_n such that every node is assigned to one of the $N_1, \ldots, N_n$. It is not clear that the conclusion of the original proof will be assigned to N_n; but that will certainly be the case if the tree has no detached parts.

2. T-proofs

We turn now to discuss the application of these ideas to proof trees constructed by the natural rules of Gentzen. These rules I shall call T-rules,[6] and the proofs based on them T-proofs.

[6] Gentzen called them N-rules. The change from "N" to "T" was madeto avoid conflict with the "N" standing for negation. See [4] p. 248 at the b ottom.

As already stated, the peculiarity of such rules is that they may involve the discharge of assumptions. Thus a T-proof consists of a construction tree and a list of assumptions such that certain conditions are fulfilled. The formulation of these conditions varies with the extent to which repetitions are allowed. Suppose we are rather liberal in regard to repetitions; then the conditions are as follows:

(i) Every initial node is an instance of a specific assumption or an axiom. [7]

(ii) An assumption is either not discharged at all, or is discharged at a unique node; assumptions which differ as to their node of discharge are distinct as assumptions, even though they may be instances of the same formula.

If it is desired to modify (ii) to eliminate repetitions, we may proceed thus. Let us call two assumptions *semi-identifiable* when (i) they are instances of the same formula, and (ii) if they have distinct points of discharge, neither discharge point is under the other. Let X_1, X_2 be two semi-identifiable assumptions and let Y_1, Y_2 be the nodes such that X_1, X_2 are discharged in the inferences leading to Y_1, Y_2 respectively. Let Δ_1, Δ_2 be the subtrees terminating in Y_1, Y_2 respectively. Then if 1) Δ_1 and Δ_2 are homologous as construction trees, and 2) every assumption of Δ_1 which is discharged below Δ_1 has been previously shown to be identifiable with the assumption made at the corresponding point of Δ_2 and discharged below it, and vice versa; then X_1 and X_2 are also identifiable. Then we can modify (ii) to allow identifiable assumptions to be considered the same; but otherwise assumptions which are unlike or are discharged at different points are distinct.

We then say that two nodes X and Y are *homologous* when (i) X and Y are identifiable assumptions; or (ii) X and Y are conclusions of inferences by the same rule, such that corresponding premises are homologous and corresponding discharged assumptions are identifiable.

[7] We may need to consider cases in which there are axioms of the underlying system, for instance in predicate calculus with equality (cf. [4], Sec. 7D, Exercise 5). Indeterminates occurring in these axioms are still to be regarded as indeterminates in applying such rules as Πi and Σe.

The linearization process consists of an assignment of nodes of Δ to the "lines" $N_1, \ldots, N_n$ of a linearized proof. At each stage of the process a subtree Δ_1 of Δ is determined. Initially $m = 0$ and Δ_1 is the whole of Δ. Given $N_1, \ldots, N_m$ and Δ_1, a node X is *free* if and only if it has not been previously assigned, and either (i) X is an assumption which is not discharged in Δ_1, or (ii) X is not an assumption and all nodes in Δ_1 above X have already been assigned. Then the nodes assigned to N_{m+1}, or the Δ_1' which is to replace Δ_1, are determined as follows:

Case 1. If there are any free nodes in Δ_1, then the first (in the normal construction sequence, i.e. left-hand-most) such node, together with all nodes homologous to it, are assigned to N_{m+1}, and Δ_1' is the same as Δ_1.

Case 2. If there are no free nodes in Δ_1, but there is a node Y in Δ_1 which is the conclusion of an inference at which an assumption not previously assigned to one of $N_1, \ldots, N_m$ is discharged, let Z be the unique such Y such that there is no other such Y under or to the left of Z. Then take Δ_1' to be the subtree over Z.

Case 3. If neither of these cases holds and Δ_1 is Δ the process terminates. If Δ_1 is not Δ, take Δ_1' to be Δ.

By this process a T-tree can be transformed into a linear series $N_1, \ldots, N_n$, which will be called a *linearized T-proof*. This series can be provided with an analysis [8] stating for each N_k whether or not it is an assumption; and if it is not, by what rule it is inferred, from what premises, and what assumptions are discharged in the process. Conversely given such a T-proof with analysis one can construct a T-tree by starting at the bottom and working upward.

3. Semantical discussion

In his original presentation Gentzen commented on the fact that his inferential rules, particularly the T-rules, followed quite naturally from the meanings of the logical connectives. This point can be elaborated; and in fact there is a rather simple semantical justification for all of the Gentzen rules. Although this justification has

[8] This term is due to Kleene [10a] p. 87.

been presented several times in a variety of different ways, it still seems to be little known and often misunderstood. The following discussion contains some improvements over those contained in previous publications.

In setting up a logical calculus we are dealing with a system L in which we make statements of a specified sort about certain objects. These objects I shall call *formulas*. They may be thought of, if one prefers, as a sort of expressions in an object language; but that is not necessary, and no symbols or expressions of such an object language are exhibited here. The formulas are generated from certain *elementary formulas* by the logical operations. If quantifiers are included, we must suppose there are also objects called *terms* which can occur as parts of formulas; and a quantifier is an operation combining a term and a formula to make a formula. To fix the notation I state that the logical operations are those which form, from given formulas A (and B), the formulas $\neg A$ (non-A), $A \wedge B$ (A et B or A meet B or A con B), $A \vee B$ (A vel B or A join B or A ad B), $A \supset B$ (A ply B), $(\forall x)\, A$, (for all x, A) $(\exists x)\, B$ (for some x, B). Formulas which are not elementary I shall call *compound*.

So far nothing has been said about any meanings which may be associated with the formulas. We now bring semantics into the situation as follows. Let us associate with each formula A a statement A^* belonging to a set of statements V^*; I shall call A^* the *value* of A. In saying that the values are statements, I imply that we have some way of picking out from V^* those values which are true statements. [9] It is presumed also that the values of the elementary formulas, or at any rate of some of them, are arbitrary, and consequently the truth of these values is also; but for the compound formulas the question of whether the values are true or not is to be determined by uniform rules from similar questions related to the arguments from which they are formed. These rules are what determine the meanings of the logical connectives. The question is, how shall the rules be constituted?

In the classical case it is supposed that the value statements are

[9] Thus we are concerned with that branch of semantics for which I have proposed the term aletheutics (cf. [4], p. 91).

divided into two exhaustive and mutually exclusive classes, the true and the false; in other words that the value statements are definite. In such a case the question is answered by the usual truth tables together with analogous explanations for the quantifiers. But suppose the value statements are such that the true statements form an inductive class which is generated from certain initial statements, the axioms, by inferential rules. In such a case it is well known that there are cases where we cannot determine whether a value statement is true or not. However, we suppose that it is always possible to verify definitely the correctness or incorrectness of a proof; Lorenzen expresses this by saying that the value statements are *proof-definite* (beweisdefinit). A determination of the meanings of the operations which takes account of this actual situation may be called an *effective* logic. How is such a determination to be made?

Before attacking this question it is necessary to answer another. So far the formulas are simply objects; but if we are to make a formal system dealing with these objects, it is necessary to specify some way of making statements about formulas. The new question is how we do this. In the classical case the most natural answer would be that the basic statement to be made about a formula A is that its value be true; and we can formalize this statement by introducing a predicate $\vdash$ so that the elementary statements of the formal system are of the form (1) where A is a formula. We could do this in the case of an effective logic also; but it is Gentzen's suggestion, in effect, that we take as elementary statements concerning formulas those of the form

$$A_1, \ldots, A_m \Vdash B, \tag{2}$$

where $A_1, \ldots, A_m$, B are formulas and $m \geq 0$. The statement (2) will be associated with a statement

$$A_1^*, \ldots, A_m^* \rightarrow B^* \tag{3}$$

in V^*, in which the connector "$\rightarrow$" has yet to be explained; for the present let it suffice to say that when (3) is true, and all of $A_1^*, A_2^*, \ldots, A_m^*$ are also true, then B^* is true. It can be shown that this type of elementary statement is also suitable for a classical logic, although somewhat less natural.

Let us now return to the interpretation of the logical operations. The fundamental heuristic principle of the analysis is that the meanings of these operations are fixed by the conditions under which a formula formed by one of them can be introduced into a discourse from which it was previously absent. Since the basic statements of our formal discourse are of the form (2), such a constituent may be introduced either on the left or on the right. In an effective logic there will thus be two rules of introduction for each operation. I shall discuss this more in detail for the ply operation.

Our intuitions would lead us to suppose that $(A \supset B)^*$ should be $A^* \to B^*$. This reduces the explanation of $\supset$ to that of $\to$. The latter will concern us presently. But without regard to that point note that the identification of $(A \supset B)^*$ with $A^* \to B^*$ motivates, in combination with common sense properties of $\to$, the rule for ply introduction on the right, viz.

P* $$\frac{X, A \Vdash B}{X, \Vdash A \supset B,}$$

where A and B are any formulas, and X is any finite sequence of formulas.

In order to proceed further it is necessary to interpret (3). In [6] and subsequent publications $\to$ was interpreted as formal deducibility; i.e. (3) was interpreted as saying that there was a method, using the deductive rules in effect at that point, of deducing B^* from $A_1^*, \ldots, A_m^*$[1] as premises. On this basis and the above heuristic principle it was shown that the rule P* and the rule for introduction on the left, viz.

*P $$\frac{X, \Vdash A \qquad X, B \Vdash C}{X, A \supset B \Vdash C}$$

could be justified. I shall not go further with this, because Lorenzen has another way. He interprets (3) as a rule (for introducing B^* if $A_1^*, \ldots, A_m^*$ can be established) which is true just when it is "*admissible*" (zulässig), i.e. when any conclusion derived by means of it can be established without it. This, on the face of it, is a non-constructive concept; let us strengthen it by the requirement that any derivation obtained by the use of the rule can be constructively transformed into one obtained without such use.[10] This must be understood to include the possibility that it can be constructively shown that the rule can never be used (because its premises cannot be verified); then the rule is *vacuously admissible*.

That the rules $*P*$ can be justified in terms of admissibility can be shown as follows.[11]

For $P*$. If there is a constructive process of obtaining B^* from X^*, A^*, that establishes the admissibility of $A^* \rightarrow B^*$ on the basis of X^*. If the premise is vacuously admissible, then we may suppose (as a heuristic assumption) that either X^* cannot be verified, in which case the conclusion is vacuously admissible, or that A cannot be verified when the X^* are, in which case $A^* \rightarrow B^*$ is vacuously admissible on the basis of X^*.

For $*P$. If the left premise is vacuously admissible, so is the conclusion. Suppose there is a constructive method of obtaining A^* from X^*. Then if the right premise is vacuously admissible, so is the conclusion. Otherwise let there be a verification of $A^* \rightarrow B^*$, then by the right premise there is a verification of C^*. Thus there is a constructive method of obtaining C^* from X^* and $A^* \rightarrow B^*$.

In these arguments it must be understood that the A, B, C, may be elementary or compound; if the latter, and they involve $\supset$, their values are themselves rules, whose verification means a constructive demonstration of their admissibility. This admissibility means their eliminability in terms of those considered earlier, and ultimately in terms of the rules of the "ground system" S which is supposedly present in V^* to begin with. (This ground system S must not be confused with the system L). In effective logic this S is unspecified and may be void; effective logic can therefore be applied in connection with any such system.[12]

[10] Cf. Lorenzen [12] pp. 174f.

[11] In these explanations X^* means the sequence composed of the values of the members of X.

[12] The restriction made in [4] that the rules of the ground system be "elementary" is not relevant to the semantical discussion, although it would be if we went on to prove theorems.

All the other rules of the Gentzen calculus LJ can be justified in a similar way. It is not necessary to go into detail about them here. Concerning the rules for $\wedge$ and $\vee$, and the structural rules, the situation is clear. Concerning negation I have nothing to add to what is said in [4], Chap. 6; in this paper I shall regard negation as defined by

$$\neg A \equiv A \supset F, \tag{4}$$

where F is a specific elementary formula.

It will be necessary, however to make a few remarks about quantification. Lorenzen, in [14] pp. 20ff argues that $(\forall x)\ A$, where $A(x)$ is some formula in which x occurs, is not in general proof-definite, even when $A(a)$ is proof-definite for every a. But if one examines the way in which $(\forall x)\ A(x)$ is established in a dialogue (Lorenzen l.c. pp. 27 and 31) one finds that one concludes it by establishing $A(a)$ where a is an indeterminate.[13] Since $A(a)$ is proof-definite, $(\forall x)\ A(x)$ is also. Thus the Gentzen rules for introducing $(\forall x)\ A(x)$ on the right and $(\exists x)\ A(x)$ on the left can be justified semantically if one understands $(\forall\ x)\ A(x)$ as meaning, not $A(t)$ for every actual term t, but $A(a)$ in a term extension formed by adjoining an indeterminate a. This is a kind of generality called schematic in [4]. The justification can then be given as in [4] Sec. 7A.

The conclusion of all this is that there is a semantics for effective logic, and that in principle it has been known for a long time. According to that semantics – if one takes the position that negation is to be identified with absurdity [14] – the rules of Gentzen's calculus LJ are valid; and consequently the formulas which they show to be assertible, viz. those of the Heyting calculus, are valid also. Whether, conversely, every semantically valid formula is so deducible is another matter, with which I am not here concerned.[15]

[13] Cf. also Lorenzen [12] p. 175.

[14] As defined, e.g., in [4] Sec. 6A1.

[15] The example in Lorenzen [12] p. 173 shows that a rule may be valid in the admissibility sense of (3) but not in that of formal deducibility. By the conservation property ([4] Theorem 5E3) that would not be derivable, even in a classical system. Thus we have incompleteness relative to the admissibility interpretation of (3) when the ground system is not void.

A few supplementary remarks will now close this discussion. In the first place I have not claimed that the interpretation sketched here is the only valid interpretation of the Heyting calculus, or even the first, but simply that it is valid and natural. In the second place it is significant that no matter whether one interprets (3) in terms of formal deducibility or admissibility, the effective logic is the same.[16] In either case there are some heuristic assumptions, but they are not nearly so questionable as in the more usual semantical theories.[17] In the third place it is possible to have a treatment of classical logic along the lines indicated here. This treatment uses elementary statements of form (2), rather than (1); one needs to postulate an additional rule, called Px, which can be presented in a form slightly different from that used in [4], viz.[18]

$$\text{Px} \qquad \frac{X, A \Vdash C \qquad X, A \supset B \Vdash C}{X \Vdash C}.$$

There are also parallel treatments of either classical or effective logic which use elementary statements of the form

$$(5) \qquad A_1, \ldots, A_m \Vdash B_1, \ldots, B_n.$$

For the details see [4].

4. Dialogues

We turn now to discuss the game-theoretical approach which appears in the work of Lorenzen, Lorenz, and Stegmüller cited in the introduction. This idea is an outgrowth of the semantic tableaux of

[16] Cf. footnote 15.

[17] Cf. Stegmüller [16].

[18] This rule is the translation (by putting an arbitrary B in the place of F) of the rule Nx used in Chap. 6 of [4], whereas the Px used there in Chap. 5 is the translation of an older form of Nx. The change from the old to the new of Nx was made in Chap. 6 (in order to obtain ET for $\mathrm{LD_m}$) too late to affect Chap. 5. There is an error in Chap. 6 (in the proofs of Theorems 1 and 2 on page 281) due to the fact that the older form of Nx was referred to and the necessary correction was overlooked. The new Px would enable simplifications to be made in the proof of the elimination theorem. The new Px is equivalent (from the point of view of semantics) to postulating that $\vdash A \vee . A \supset B$.

Beth.[19] But the German logicians have given the idea a peculiar twist, in which the game aspects have become more prominent.

The basic idea of the game is the following: We suppose there are two players, R and L. The game begins by R setting forth a thesis which I shall call R_0. Thereafter moves are made by the two players alternately; first L_1, then R_1, then L_2, then R_2, etc. The moves consist in writing entries consecutively in the left and right columns respectively of a tableau;[20] I shall identify the move with the entry so made. The entries consist either of setting a formula, i.e. writing it in the appropriate space, or of making a challenge; of the latter there are five kinds: ?, ?1, ?2, $?a$, $?t$, in which a is an indeterminate and t is an arbitrary term. Every move must either "attack" some previous move made by the opponent or "defend" some previous move of the same player against an attack made by the other.

The appropriate attacks against the various kinds of moves and the defenses against them are given for a modified form of Lorenzen's game in table 1. Here the original move is in M_1, possible attacks in M_2, defenses (against the attacks in M_2) in M_3 and M'_3. Additional conventions are explained below.

(For Lorenzen's own game one should omit the column M'_3; the rows 2, 7b, 8b, requiring that rows 7a and 8a hold for both R and L; and also omit the F, leaving a counter-attack as the only possible response in row 6.) The "E" in row 1 stands for an elementary formula. Such a formula cannot be attacked if made by L, or if made by R after it has been made by L; if R is compelled to set such a formula which L has not set previously, then R loses the game. If, without violating this condition, the game reaches a position where R cannot be attacked, then R wins; otherwise R loses.

Now it seems to be Lorenzen's contention that these rules deter-

[19] These tableaux are called proof tableaux in [4]; references to the basic papers of Beth may be found there. Beth [2], p. 18, mentions the game idea in a slightly different connection. Lorenzen ascribes certain ideas to Beth in [13] p. 195, 196 and [14] p. 29.

[20] In Stegmüller's presentation this condition is not satisfied. [Now that Lorenz [11] has become available, it develops that this point is an essential difference between his type of dialogue and that considered here.]

TABLE 1

	M_1	by	M_2	M_3	M_3'
1a	E	L	none		
1b	E	R	?	none	
2	$[A]$	R	?	A	free
3	$A \wedge B$	L,R	$\begin{cases} ?1 \\ ?2 \end{cases}$	A B	$[A]$ $[B]$
4	$A \vee B$	L,R	?	$\begin{Bmatrix} A \\ B \end{Bmatrix}$	$[A \vee B]$
5	$A \supset B$	L,R	A	B	$[B]$
6	$\neg A$	L,R	A	F	$[F]$
7a	$(\forall x)A(x)$	L	$?t$	$A(t)$	
7b		R	$?a$	$A(a)$	$[A(a)]$
8a	$(\exists x)A(x)$	R	?	$A(t)$	$[(\exists x)A(x)]$
8b		L	?	$A(a)$	

mine the meaning of the logical connectives. In effect he defines a formula A to be valid in effective logic just when R has a win strategy for the game with A as R_0. Indeed he goes on to derive the Gentzen rules on that basis. But although there is a certain semantical justification for that approach, it involves some highly arbitrary and unnatural conditions, such as the requirement (not previously mentioned here) that only the last R_n can be attacked whereas any L_k can be attacked as often as R likes.[21] It is far less satisfactory than the semantical approach of § 3, to which, incidentally, Lorenzen has also contributed.

Suppose, however, we interchange the horse and the cart in all this. If one takes the semantics of § 3, and with it the Gentzen rules, as basis, then the game-theory approach does produce something which is useful and interesting. As a technique for discovering proofs it suggests a procedure which is an improvement over other

[21] For discussion of this point see Stegmüller [16]. Lorenzen is actually a little vague about this condition (and some others). Stegmüller clarifies these conditions (following Lorenz) in a "structural rule"; on the lack of intuitive justification for it see his Remark 5.4.

existing procedures. But for that purpose it invites some modification.

Stegmüller [16] gives one modification of the dialogue, which I understand has affinities with Lorenz [11]. I shall propose presently another modification which, in contrast to that of Stegmüller, has the following features.[22]

1. It allows much less choice to L, so that it is easier to check whether or not one has a win strategy. In fact L is compelled to attack (and, with few exceptions, R to defend) whenever such action is possible. If there are no moves of the form $A \vee B$ for L and none of the form $A \wedge B$ for R, then L has no choice whatever.

2. It opens with given formulas for $L_1, \ldots, L_m$, and R_m, $m \geq 0$, the first move being L_{m+1}. This is a slight generalization of the foregoing, which is the special case $m = 0$. This is of no consequence so far as generality of the theorem is concerned, but helps with certain inductions in the proof.

3. It requires that under certain circumstances moves on the left be cancelled. This is a nuisance, but I claim the advantages compensate for it.

4. It defines negation by (4) of § 3, and allows F to appear as a move on either side without necessarily terminating the dialogue. In the full intuitionistic logic the appearance of such an F uncancelled on the left allows an arbitrary formula to be set on the right without possibility of being attacked; in the minimal logic this is not the case.

5. A win for R consists in the confirmation, in a sense to be shortly explained, of R_m, and not merely the attainment of a state which cannot be attacked.[23] If such a confirmation cannot be attained, R loses.

6. It allows moves which are formulas in brackets; these are the only moves which R is not obliged to defend immediately. This is a purely technical device which could be dispensed with; but it

[22] An additional point of difference has already been mentioned in footnote 20.

[23] It may be that one can prove as an epitheorem that it is sufficient to terminate the game when certain conditions, analogous to Lorenzen's winning positions, are fulfilled; but I have not investigated this.

helps to keep track of the R-moves which are still undefended. If R_m is elementary it is required to be bracketed.[24]

After these preliminaries we proceed to the formulation of the rules. For this purpose we need some definitions. If "attack" and "defend" are terms to be defined by the rules below, these definitions are as follows.

A *general challenge* is one of the form "?". Other challenges are called *special challenges*.

R_n is *firm* $\rightleftarrows$ R_n is a formula which cannot be attacked.

R_n *confirms* $R_q \rightleftarrows R_n$ is firm and either $q = n$, or R_n confirms some R_s which defends R_q.

A *round* [25] is a set of R-moves, not necessarily consecutive, such that 1) the first move, called the *head* of the round, is either R_m or a set formula which attacks; [26] and 2) if R_q is in the round then R_s is also if either R_s defends R_q, or R_s is a challenge and $s = q + 1$, or R_s is a challenge and all R-moves between R_q and R_s belong to rounds which have been previously closed. A round is *closed* when there is a firm R_n in it; then the round is said to be *closed* at R_n. It follows that no member of the round is below R_n, and that R_n confirms every formula of the round.

R_n *cancels* $L_p \rightleftarrows R_n$ closes a round with head at R_q, $q < p \leq n$, and no R_s, $s < n$, has cancelled L_p.

Then the rules of the dialogue are as follows.

Rule 1. Every move, except the initial ones, must either defend some previous move on the same side or attack some previous move on the opposite side. The move must state whether it is an attack or defense, and what previous move it attacks or defends; this statement is called the *justification* for the move. No move can state more than one justification; if two or more justifications are possible, one must be chosen as the reason for the move.

[24] The bracketed formulas are a partial substitute for the possibility of leaving a "round" open and filling the blank space later which Stegmüller uses. The suggestion for using them came from Stegmüller's completeness proof.

[25] This notion of round must not be confused with the similarly named but quite different notion of Stegmüller and Lorenz.

[26] In either case it sets a new formula to be defended.

Rule 2. Let M_1 be a move having a form indicated in the M_1 column of table 1. If M_2 attacks M_1, M_2 must have the form indicated in the same row of the M_2 column of table 1. If M_2 attacks M_1 and M_3 defends M_2, then M_3 must have the form indicated in the same row of either the M_3 or the M_3' columns of table 1, except that where "free" occurs the move is not a defense; further more, there must be no M_4 between M_2 and M_3 which defends M_1 against M_2. A move which does not appear in the M_1 column of table 1, in particular a challenge, can be neither attacked nor defended.

Rule 3. R_n can set a formula A only if one of the following conditions holds: (1) A is compound; (2) A is of the form $[B]$; or (3) A occurs as an uncancelled L_p for $p \leq n$; or (4) (in the full intuitionistic calculus only), F appears as such an L_p.

Rule 4. R_n can defend R_p only if there is no formula R_q, $p < q < n$, which has not been confirmed.

Rule 5. R_n can attack $L_p, p \leq n$, only if L_p has not been cancelled.

Rule 6. If there is an undefended R_p above R_n, R_n must take action as indicated in table 1 with respect to one such R_p. If R_p is $A \wedge B$, $A \supset B$, $(\forall x)\, A(x)$ the M_3 of table 1 must be made unless this is impossible by Rule 3. In the other cases either M_3 or M_3' defenses may be used. Only in those cases where "free" appears in table 1 is an attack permitted.[27]

Rule 7. If R_n is the same formula as some uncancelled L_p, $p \leq n$, or, in the full intuitionistic calculus, if F is such an L_p, then R_n is firm; likewise an R_n which has once been attacked cannot be attacked again.

Rule 8. L_{n+1} must attack R_n unless (by Rules 2,7) this cannot be attacked.

Rule 9. If R_n is a challenge attacking L_p, L_{n+1} must defend L_p.

Rule 10. If R_n is firm, let L_p such that it is uncancelled and R_n confirms an R_q which attacks L_p. Then L_{n+1} must defend L_p, using the M_3 defense in table 1. (There will always be one and only one such L_p; viz., the L_p attacked at the head of the round closed by R_n.)

[27] For a possible alternative here see Remark 1 under § 5.

Let $\mathrm{Di}(A_1, \ldots, A_m; B)$ denote a dialogue which begins with $L_1 = A_1, \ldots, L_m = A_m, R_m = (B)$ where (B) is $[B]$ if B is elementary and otherwise $(B) = B$. The notation

$$(6) \qquad A_1, \ldots, A_m \Vdash^{\mathrm{D}} B$$

shall state that there is a win strategy for a dialogue $\mathrm{Di}(A_1, \ldots, A_m; B)$.

The remainder of this paper will be devoted to proving that (6) is necessary and sufficient for (2). The necessity will be proved in § 5, the sufficiency in §§ 6 and 7.

5. Proof of necessity

In proving the necessity of (6), we shall have occasion to draw rather heavily on theorems of [4]. In fact it is because of these theorems, notably the inversion theorem, that it is possible to restrict the range of choices in the way stated under property 1 of § 4. I shall therefore have no hesitation in using rather technical results and terminology from [4].

The form of LJ* which is postulated in this proof is a singular form coming under Formulation I, but with quasi-principal constituents on the left in all premises, so that Rules *W* are unnecessary. There is no advantage in the Ketonen form of $*\Lambda$; accordingly it is not used. The rule *K is to be applied only initially. The range of the elementary statements is not explicitly indicated; it could be introduced and taken care of without difficulty. Negation is taken as defined in terms of F by (4), so it does not have to be treated explicitly.

In regard to the inversion theorem, the proof given in [4] (§ 5D1, 6B2, 7B2) is for multiple systems. But the proof of direct invertibility of P*, $\Pi*$, $\Lambda*$, $*\Sigma$ and *V as given there is valid for singular systems; V* and $*\Lambda$ are of course not directly invertible; and $*\Pi$, $\Sigma*$ are not directly invertible even in multiple systems.

We proceed now with the proof of the following theorem.

THEOREM 1. *If* (2) *holds in* LJ*, *then* R *has a win strategy for* $\mathrm{Di}(A_1, \ldots, A_m; B)$.

Proof. Let Δ be a derivation of (2) and let it have degree n. If $n = 0$, then (2) is quasi-prime; the dialogue $\mathrm{Di}(A_1, \ldots, A_m; B)$ is then uniquely determined and closes at once. We may then use an induction on n. We suppose the necessity holds in all cases where the degree is less then n, also for all proofs of degree n which are shorter than Δ. We then have the following cases.

Case 1. B is $A_{m+1} \supset C$ and B is introduced into Δ by P*.[28] The first step in the dialogue is then compulsory and reduces the dialogue to $\mathrm{Di}(A_1, \ldots, A_{m+1}; C)$. By the inversion theorem there is a proof of degree less than n of

$$A_1, \ldots, A_m, A_{m+1} \Vdash C;$$

hence by the inductive hypothesis, there is a win strategy for the reduced dialogue, and therefore also for the original one.

Case 2. B is $(\forall x)\ C(x)$ and B is previously introduced into Δ by Π*. This case is similar. By the inversion theorem there is a derivation of degree less than n of

$$A_1, \ldots, A_m \Vdash C(a),$$

where a is an indeterminate. The first step of the dialogue, which is compulsory, reduces it to Di $(A_1, \ldots, A_m; C(a))$. Since there is a win strategy for the latter dialogue, there is also one for the original one.

Case 3. B is $C_1 \wedge C_2$, and B is previously introduced into Δ by Λ*. Then by the inversion theorem there are proofs of degrees $< n$ for both of

$$A_1, \ldots, A_m \Vdash C_1, \qquad A_1, \ldots, A_m \Vdash C_2.$$

Hence there are win strategies for both $\mathrm{Di}(A_1, \ldots, A_m; C_1)$ and $\mathrm{Di}(A_1, \ldots, A_m; C_2)$. The move L_{m+1} must be a special challenge, and the defense will reduce the original dialogue to one or the other of these two. Since there is a win strategy for each of them, there is for the original dialogue.

Case 4. B is $C_1 \vee C_2$ or $(\exists x)\ C(x)$. If the last inference in Δ used B as principal constituent, let D be its subaltern. Then there

[28] Such a B must be introduced into Δ (not necessarily at the preceding step) by P* or Fj. If it is introduced by Fj we have Case 5. Cf. Remark 3.

is a win strategy for $\mathrm{Di}(A_1, \ldots, A_m; D)$. R will then choose the M_3 – defense with (D), and thus has a win strategy. If the principal constituent is not B, R will choose the M_3' defense.

Case 5. B is introduced into Δ by Fj. Then there will be a proof Δ' of degree $\leq n$ and shorter than Δ of

$$A_1, \ldots, A_m \Vdash F.$$

By the inductive hypothesis there will be a win strategy G for this. Then R will proceed with the dialogue until he reaches a point where he can use an M_3' defense. Let $[C]$ be the result of this. Then R can proceed with G until L is forced to set F; then R can introduce C and win.

If none of these cases occur, then the principal constituent of the last step in Δ must be some A_j. Further (B) must be $[B]$ and L_{m+1} is a general challenge. Thus R_{m+1} is free to attack.

Case 6. A_j is $C_1 \supset C_2$. Then the premises of the last inference are

$$A_1, \ldots, A_m \Vdash C_1, \tag{7}$$

$$A_1, \ldots, A_m, C_2 \Vdash B. \tag{8}$$

By the inductive hypothesis there is a win strategy for each of these. If C_1 is composite, set $R_{m+1} = C_1$; by the win strategy for (7), C_1 can be confirmed and L forced to set C_2. If C_1 is elementary, the same effect can be achieved by taking R_{m+1} as the first R-move (the first L-move is a general challenge) in the strategy for (7); then L is forced to set C_1 (since it is elementary) and hence C_2. Then R wins by the strategy for (8).

Case 7. A_j is $C_1 \wedge C_2$ or $(\forall x)\ C(x)$ or $(\exists x)\ C(x)$. Let D be the subaltern. By a challenge in R_{m+1}, R can force L to set D, and then R wins by the strategy for the premise.

Case 8. A_j is $C_1 \vee C_2$. Here there are two premises

$$A_1, \ldots, A_m, C_1 \Vdash B, \qquad A_1, \ldots, A_m, C_2 \Vdash B,$$

with a win strategy corresponding to each one. By a general challenge L is forced to set either C_1 or C_2, and R has a win strategy for either case.

Remark 1. In the cases where A_j is $C_1 \vee C_2$ or $(\exists x)\ C(x)$ one could also use the inversion theorem. One would then need to consider the possibility that the derivation(s) of the premise(s) have the same degree; this can happen when all the parametric ancestors of the principal constituent enter by $*$K. In this case a secondary induction on the K-order (of [4] p. 338) will take care of the situation. This proof shows that we could modify Rule 6 to allow R to attack immediately in such cases. Presumably it is to R's advantage to attack such an A_j as soon as possible.

Remark 2. There is unfortunately some confusion in regard to the notion of degree in [4], particularly with respect to Fj.[29] Thus throughout § 2E6 the word "rank" is erroneously used instead "degree". The footnote on page 265 is incorrect, and should be replaced by the remark that if all the quasi-parametric ancestors of M are introduced by Fj, then that particular instance of Fj can be taken as the R of the theorem; and, when so taken it can be regarded as nonstructural. This vitiates the proof of VIII on page 339; but in that case the point is that the result of VIII can be obtained from the datum by K$*$, and therefore has a Δ of the same degree.

Remark 3. The inversion theorem requires that in Δ the prime statements be elementary. This corresponds to requiring that the R_n in the first part of Rule 7 be elementary. But if the more restricted system is sufficient, so is the stronger.

6. Simple dialogues

In proving the sufficiency of (6) for (2), we shall look first at the case where there are no instances of $A \wedge B$ on the right or of $A \vee B$ on the left. In that case L has no choice, and we have to do, not with a strategy, but with a single dialogue. Such a dialogue will be called a *simple dialogue*. It will be said to *close* if eventually the principal round, whose head is the initial R-move R_m, closes.

It will appear that if we have such a simple dialogue Δ, then from the moves of Δ one can pick out in a systematic manner a sequence $N_1, N_2, \ldots,$ such that after omission of challenges, brackets, and

[29] [The errors described in the following paragraph have mostly been corrected in the second printing, which appeared in the late summer of 1963.]

needless repetitions, one has a linearized T-proof in the sense of § 2; further one eventually reaches every R_n which is confirmed in Δ. This result is probably of sufficient interest to be set up as an independent theorem. But before we tackle it, it will be necessary to explain some conventions.

The positions of the N_i are defined as follows. We take N_1 from L_1. If the positions of the $N_1, \ldots, N_r$ are determined, then N_{r+1} is to be taken from the position shown in table 2.

TABLE 2

	Position of N_r	Conditions	Position of N_{r+1}
1	L_n	$n \leq m$	L_{n+1}
2	L_n	R_n does not close a round	L_{n+1}
3	L_n	R_n closes a round	R_n
4	R_s	R_q immediate predecessor of R_s in round closed by R_n	R_q
5	R_s	R_s head of round closed by R_n	L_{n+1}

Thus one forms the N_r by going down the left column to the end of a round, then shifting over to the right column and going upwards to the head of the round, then going back to the left column and continuing. An R_q will not be a position for an N_r unless all L_j for $j \leq n$, where R_n is the closure of the round containing R_q, have been included among the $N_1, \ldots, N_{r-1}$; and an L-move L_n will not be an N_r until all R-moves in previously closed rounds, and only those R-moves, occur previously among the $N_1, \ldots, N_{r-1}$.

The foregoing provisions describe the positions from which the N_i are taken. When we actually write the N_i, we modify the moves as follows: If the move in the position indicated for N_i is a set formula A, we take N_i to be A; if it is $[A]$ we take N_i to be A; and if it is a challènge we take N_i to be a repetition of N_{i-1}. We can, if we wish, avoid a few anomalous positions at the beginning by taking N_0 to to be a provable formula T.

Let an *assumption* be defined as an L-move which is a set formula (not a challenge) and either 1) is one of the $L_1, \ldots, L_m$, 2) attacks, or 3) defends against a general challenge. If N_i is an L-move L_k, it will be said to be *discharged* in $N_1, \ldots, N_r$ when R_{k-1} appears as N_j for $i < j < r$. This is particularly important when L_k is an assumption, but will be allowed generally.

If N_i is an L-move which is discharged in forming N_j, then any N_k such that $i \leq k < j$ will be said to be *covered* by N_j. An N_k which is covered by some $N_j, j \leq r$ will be said to be covered in $N_1, \ldots, N_r$.

Let X_r be the set of all assumptions in $N_1, \ldots, N_r$ which are not discharged in $N_1, \ldots, N_r$; Z_r the set of all R_q in $N_1, \ldots, N_r$ such that R_q is head of a round and is not covered in $N_1, \ldots, N_r$; and U_n the set of all $L_j, j \leq n$ which are formulas and have not been cancelled by some R_q with $q < n$. One should note that X_r and Z_r depend only on the level at which they occur.

In terms of these explanations the theorem to be proved is:

THEOREM 2. *Let Δ be a simple dialogue* $\mathrm{Di}(L_1, \ldots, L_m; R_m)$. *Let $N_1, N_2, \ldots$ be determined as above described. Then*

(9) $$X_r, Z_r \Vdash N_r.$$

Proof. If, $r \leq m$, $N_r = L_r$, $X_r = L_1, \ldots, L_r$ and Z_r is void; then (9) is clear. If $m = 0$ and $r = 1$, then $N_1 = L_1$; if L_1 is a challenge, (9) is vacuous; if not $X_1 = L_1, Z_1$ is vacuous, and (9) is again clear. These two possibilities take care of the basic step of an induction on r.

Suppose, then, that (9) holds for a given r and all smaller values. We show that it holds for $r + 1$. This new (9) will be called (9)$'$. We divide into cases according to table 2. Case 1 is already taken care of in the basic step.

Case 2. $N_r = L_n$, and R_n does not close a round; $N_{r+1} = L_{n+1}$. Then

(10) $$Z_{r+1} = Z_r.$$

If L_{n+1} is an assumption, then, since R_n has not been confirmed, L_{n+1} is not discharged; therefore it is in X_{r+1}, and (9)$'$ follows at once.

If L_{n+1} is not an assumption, then

(11) $$X_{r+1} = X_r.$$

If L_{n+1} is a challenge, then (9)′ is identical to (9). Otherwise L_{n+1} defends some L_p in U_n against attack by R_n. Since R_n does not close a round,[30] its attack on L_p must be a special challenge; thus L_{n+1} must follow from L_p by Λe or Πe, and so we can infer (9)′ from the following lemma.

LEMMA. *If $N_r = L_n$ and L_j is in U_n, then $X_r, Z_r \Vdash L_j$.*

Proof. If L_j is an assumption, then it cannot be covered by any previously closed round; hence it is in X_r. This will be the case, in particular, if L_j is the first L-move in U_n. It will therefore suffice to prove the lemma for a given j on the assumption that it is true for all smaller values of j.

If L_j is not an assumption it defends some previous L_i against attack by some R_p, where either $p = j - 1$ and R_p is a special challenge, or R_p is head of a round closed at R_{j-1}. In the former case L_j follows from L_i by Λe or Πe, and the lemma follows by the inductive hypothesis. In the latter case R_p cannot be covered by a round closed at R_s, because such a round would have to include L_j. Hence R_p is in Z_r; and since L_j follows from L_i and R_p by Pi, we again have the lemma by the inductive hypothesis.

This completes the proof of the lemma, and of Case 2 under Theorem 2.

Case 3. $N_r = L_n$ and R_n closes a round, $N_{r+1} = R_n$. Then we have (11). By the rules of the dialogue, R_n is the same as some L_p in U_n and hence, by the lemma of Case 2;

$$X_r, Z_r \Vdash N_{r+1}.$$

Since (10) holds, unless R_n is the head of the round it closes, in which case $Z_r \subseteq Z_{r+1}$, we have (9)′.

Case 4. $N_r = R_s$ in the round closed by R_n, $N_{r+1} = R_q$, the next preceding R-move in the same round.

In cases where there is a gap between R_s and R_q, that gap must be filled by previously closed rounds. All lines in these rounds, except possibly the head lines, will be covered when the rounds are

[30] If R_n were a set formula it would not be firm, and then L_{n+1} would be an assumption; likewise L_{n+1} would be an assumption if R_n were a general challenge.

circuited; hence, in considering the effects on X_r and Z_r in going from r to $r+1$, we can ignore these lines and write only the uncovered head lines. If R_{k+1} is one of these, then R_k must close the preceding round; for otherwise R_{k+1} would also be covered when the round of R_k is circuited. Hence every such L_{k+1}, except possibly that where $k = q$, is a defense against attack by R_k. Consequently the only possible effect on X_r is to delete L_{q+1}, so that we have one or the other of (11) or

(12) $$X_r = X_{r+1} \cup \{L_{q+1}\}.$$

As for Z_r, let $B_1, B_2, \ldots, B_t$ be the heads of the rounds. Then

(13) $$Z_r = Z_{r+1} \cup \{B_1, B_2, \ldots, B_t\}.$$

The gaps cannot occur on the first round to be closed; hence we can use an induction on the number of such rounds. By applying our result to these subordinate rounds we get the following deducibilities

$$\begin{array}{l} X' \Vdash^{\mathrm{T}} B_1 \\ X', B_1 \Vdash^{\mathrm{T}} B_2 \\ \cdots\cdots\cdots\cdots\cdots \\ X', B_1, \ldots, B_{t-1} \Vdash^{\mathrm{T}} B_t \\ X', B_1, \ldots, B_t \Vdash^{\mathrm{T}} N_r. \end{array}$$

Where X' is X_r, Z_{r+1}, and the last statement is (9) applied to $R_s = N_r$. The result of all these is to give

(14) $$X' \Vdash^{\mathrm{T}} N_r.$$

Let us now consider the various separate subcases.

If R_q is $[A]$, L_{q+1} is a general challenge, and we have (11), also $N_{r+1} = N_r$. Thus (9)′ follows from (14).

If R_q is a special challenge, L_{q+1} is a defensive move which is not an assumption. Again we have (11) and $N_{r+1} = N_r$. Hence we have the same result as before.

If R_q is a general challenge, then L_{q+1} is an assumption which is discharged at R_q. We then have (12). Further L_{q+1} is of the form $A(c)$, where c does not occur above row $q + 1$,[31] and $(\exists\, x)\, A(x)$

[31] We must have Case 8b of Table 1, since Case 4 is excluded by the hypothesis of simplicity.

is in U_q. Also $N_{r+1} = N_r$, and this, since a challenge does not change the formula defended, is the same as some $[A]$ occurring above row q, and hence not containing c. Then we can eliminate L_{q+1} from (14) by Σe, and so obtain (9)'.

In the remaining subcases we have $s = q + 1$, $Z_{r+1} = Z_r$.

If R_q is $A \vee B$, $(\forall x)\ A(x)$, or $(\exists x)\ A(x)$, L_{q+1} is a challenge, and R_q follows from R_{q+1} by Vi, Πi, or Σi. Further (11) and (10) hold. Hence (9)' follows from (9).

If R_q is $B\ A \supset$ (or its special case $\neg A$), then L_{q+1} is A, and from (14) for $N_r = B$ (or F) one can go to (9)' by Pi (or Ni).

Case 5. $N_r = R_q$, the head of the round closed by $R_n, N_{r+1} = = L_{n+1}$. Then, since L_{n+1} is not an assumption, and no assumptions are discharged in adding L_{n+1}, we have (11) and

$$Z_{r+1} = Z_r \cup [R_q].$$

By the dialogue rules L_{n+1} defends an L_p, $p \leq q$, where L_p is in U_q. Hence by the lemma of Case 2,

$$X_r, Z_r \Vdash L_p.$$

From this, since L_p is $R_q \supset L_{n+1}$ and R_q is in Z_{r+1}

$$X_{r+1}, Z_{r+1} \Vdash L_{n+1},$$

q.e.d.

This completes the proof of Theorem 2.

Theorem 2 takes care of the sufficiency of a simple dialogue. For if the dialogue closes, we must eventually reach an r such that $N_r = R_m$; then $X_r = X_m$ and Z_r is void.

7. Multiple dialogues

In this section we shall extend the result of § 6 to the case of dialogues not restricted to be simple. Such a dialogue will be called a *multiple dialogue*. The procedure will be an induction on the number of occurrences of L-moves of form $A \vee B$ or of R-moves of form $A \wedge B$. Such moves will be called *splitting moves*.

One can conceive of a multiple dialogue as carried out in the following manner. One starts writing the moves on a tableau. When one comes to a splitting move, one starts a new tableau, copying on it all moves made so far, and then uses one alternative on one of the tableaux, the other on the other. Each component tableau may split again, and so on. Since we have a win strategy by hypothesis, the process must come to an end. Each of the component tableaux of the final result will be called a *sheet* of the dialogue. A single sheet will be like a simple dialogue except that a particular alternative will be chosen as assumption in response to a challenge attacking a splitting move. Our induction will be on the number of sheets.

Now we notice one fact concerning a simple dialogue. Let Δ be such a dialogue, and suppose that the qth row is in a round whose head is at R_p and closure at R_n, and that C is the last unconfirmed formula on the right at the qth row or above. Then the rows from $q + 1$ to n inclusive constitute a dialogue Δ_q with the U_q as initial moves on the left and C (or $[C]$) as initial move on the right. When the process of forming the N_r reaches R_q, the dialogue Δ_q has closed, and this establishes that

$$U_q \Vdash^{\mathrm{T}} C. \tag{15}$$

The rest of the argument makes use only of information in rows above the qth row and below the nth row.

Suppose then we have a multiple dialogue Δ. Select one of its sheets as a master sheet. Let q be the number of the first row such that R_q is either a splitting move or a challenge against a splitting move higher up on the left. Then the first q rows will be identical on all sheets. At the next row the dialogue will split into two dialogues Δ' and Δ''. Each of these will have a smaller number of sheets, and therefore they will close by the inductive hypothesis. If we can infer from this that (15) holds, the rest of the proof can be carried out on the master sheet only, as in the case of a simple dialogue.

It is now necessary to handle the two cases separately.

If C is $A \wedge B$, then in Δ'_q and Δ''_q (using only the parts analogous to Δ_q) we have

$$U_q \Vdash A, \qquad U_q \Vdash B,$$

from which (15) follows by Λi.

If C is an attack on an L_k of the form $A \vee B$, then L_{q+1} will be A in Δ' and B in Δ''. These are new assumptions. If we suppose them added to U_q, the parts Δ_q' and Δ_q'' of Δ' and Δ'' will establish

$$U_q,\ A \Vdash C, \qquad\qquad U_q,\ B \Vdash C.$$

Hence, since $A \vee B$ is in U_q, we have (15) by Ve.

Thus we have

THEOREM 3. *If* (6) *holds, so does* (2).

8. Concluding remarks

The notion of dialogue explained in the foregoing is probably more closely akin to the Beth semantic tableaux than are the systems of Stegmüller and Lorenz. But it differs from the Beth formulation in a number of particulars. In the first place it is based on the singular type of inferential formulation, whereas the Beth systems, at least in their original form, were based on multiple formulations. Again, although the amount of freedom has been cut down, there is still some choice left, chiefly in that R can choose what attacks to make when he has a chance to do so. I do not know any theorems which would indicate general conditions as to which choice would be best in such a situation. Thus the method is not an algorithm, and it is conceivable that a rather lengthy enumeration of cases may be necessary before one can conclude that no derivation exists (even if quantifiers are not present).

The exposition here is adapted to the system LJ*; one could adapt it to LM* by omitting the provision that any A is firm if there is an uncancelled F on the L side. For other systems (LD*, LE*, LK*) one could permit adding to the initial assumptions arbitrary instances of Peirce's law or the law of excluded middle (Cf. Theorem 8B2 of [4]).

It is natural to suppose that one could get a classical system by omitting Rule 4. This amounts to admitting statements of form (5). Presumably by modifying Rule 4 so that a defense could not pass over an undefended $A \supset B$ or $(\forall x)\ A(x)$ one could get a formulation for LJ_m^* and similar systems. But it is not clear how such concepts as confirmation and cancellation would be formulated in such a system.

The Pennsylvania State University, University Park, Penn., USA

bibliography

[1] J. M. Anderson and H. Johnstone, Jr., *Natural Deduction; the Logical Basis of Axiom Systems* (Belmont, Calif., Wadsworth Publishing Co., Inc. 1962).

[2] E. W., Beth, Remarks on Intuitionistic Logic. *Constructivity in Mathematics*, ed. A. Heyting (Amsterdam, North-Holland Publishing Co. 1959) pp. 15–25.

[3] H. B. Curry, On the Definition of Substitution, Replacement and Allied Notions in an Abstract Formal System, Rev. Philos. Louvain **50** (1952) 251–269.

[4] H. B. Curry, *Foundations of Mathematical Logic* (New York, McGraw-Hill Book Co., Inc. 1963).

[5] H. B. Curry, The Inferential Approach to Logical Calculus, Logique et Analyse **3** (1960) 119–136 and **4** (1961) 5–22.

[6] H. B. Curry, A Theory of Formal Deducibility, Notre Dame Mathematical Lectures, no. 6 (Notre Dame, Indiana 1950) 2nd ed. 1957.

[7] H. B. Curry and R. Feys, *Combinatory Logic.* Vol. 1 (Amsterdam, North-Holland Publishing Co. 1958).

[8] F. B. Fitch, *Symbolic Logic* (New York Ronald Press Co. 1952).

[9] G. Gentzen, Untersuchungen über das logische Schliessen, Math. Zeitschrift **39** (1934) 176–210, 405–431.

[10] S. Jaśkowski, On the Rules of Suppositions in Formal Logic, Studia Logica, no. 1 (Warsaw, 1934).

[10a] S. C. Kleene, *Introduction to Metamathematics* (Amsterdam, North-Holland Publishing Co., and Groningen, P. Noordhoff N.V., 1952).

[11] K. Lorenz, Arithmetik und Logik als Spiele. Dissertation, (Kiel, 1961).

[12] P. Lorenzen, Über die Begriffe Beweis und Definition, *Constructivity in Mathematics*, ed .A. Heyting (Amsterdam, North-Holland Publishing Co. 1959) pp. 169–177.

[13] P. Lorenzen, Ein dialogisches Konstruktivitäts kriterium. *Infinitistic Methods*; Proceedings of the Symposium on the Foundations of Mathematics, Warsaw 2–9 September, 1959 (Pergamon Press 1961) pp. 193–200.

[14] P. Lorenzen, *Metamathematik* (Mannheim, Bibliographisches Institut 1962).

[15] K. Ono, On a Practical Way of Describing Formal Deductions, Nagoya Math. J. **21** (1962) 115–121.

[16] W. Stegmüller, Remarks on the Completeness of Logical Systems Relative to the Validity Concepts of P. Lorenzen and K. Lorenz, Ditto Ms., 1963 (to be published).

CHAPTER 3

MARGINALIA ON GENTZEN'S SEQUENZEN-KALKÜLE

HUGUES LEBLANC

I

As many a reader knows, Gentzen fashioned in [3] two novel versions of elementary logic or, to be more precise, of the quantificational calculus of first order (QC). One kalkül, called LK, sports besides the standard primitives of QC a fresh primitive, the symbol "$\rightarrow$"; it also sports besides the standard formulas of QC so-called *sequents*, that is, expressions of the kind

$$A_1, A_2, \ldots, A_n \rightarrow B_1, B_2, \ldots, B_m,$$

where $A_1, A_2, \ldots, A_n$ $(n \geq 0)$, $B_1, B_2, \ldots,$ and B_m $(m \geq 0)$ are standard formulas of QC. As for Gentzen's other kalkül, NK, it can be thought of as a fragment of LK, a fragment that also boasts the primitive "$\rightarrow$" but only owns sequents of the sort

$$A_1, A_2, \ldots, A_n \rightarrow B,$$

where $A_1, A_2, \ldots, A_n$ $(n \geq 0)$, and B are again standard formulas of QC.[1]

Since Gentzen meant a sequent of the kind

$$A_1, A_2, \ldots, A_n \rightarrow B_1, B_2, \ldots, B_m,$$

where $n \geq 1$ and $m \geq 1$, to be valid when and only when the formula

$$(A_1 \,\&\, A_2 \,\&\, \ldots \,\&\, A_n) \supset (B_1 \vee B_2 \vee \ldots \vee B_m)$$

of QC is valid, the commas which interspace the so-called *antecedent-formulas* $A_1, A_2, \ldots,$ and A_n in $A_1, A_2, \ldots, A_n \rightarrow B_1, B_2, \ldots,$

[1] The present account of NK, though departing from the letter of [3], stems from [4], p. 512, footnote 9, and hence meets Gentzen's intentions.

B_m may be treated as so many conjunction signs, those which interspace the so-called *consequent formulas* B_1, B_2, ..., and B_m as so many disjunction signs, and the arrow "$\rightarrow$" as a conditional sign. Under this understanding of Gentzen's commas and arrows,[2] a sequent of the kind

$$A_1, A_2, \ldots, A_n \rightarrow B_1, B_2, \ldots, B_m,$$

where $n \geq 1$ and $m \geq 1$, may be treated as an inference-form with $A_1, A_1, \ldots$, and A_n as premisses and the disjunction $B_1 \vee B_2 \vee \ldots \vee B_m$ as conclusion. Sequents without antecedent-formulas, on the other hand, are tantamount to sequents with "$p \vee \sim p$" as antecedent-formula, and sequents without consequent-formulas tantamount to sequents with "$p \;\&\; \sim p$" as consequent-formula.

Gentzen supplied in [3] a list, revised in [4], of so-called *introduction* and *elimination rules of inference* for NK; only in [4], however, did he trouble to write out the one axiom schema and the so-called *structural rules of inference* he contemplated for that kalkül. Borrowing from [4] and recent papers of mine, I shall take NK to be fitted here with the following axiom schema:

$$A \rightarrow A,$$

occasionally known as the *Reiteration Rule*, and the following fifteen rules of inference, in which the Greek capitals "Γ" and "Δ" range over (possibly empty) arrays of formulas and commas:

TABLE 1

Structural rules of NK:

Permutation: $\dfrac{\Gamma, A, B, \Delta \rightarrow C}{\Gamma, B, A, \Delta \rightarrow C}$

Expansion: $\dfrac{\Gamma \rightarrow A}{B, \Gamma \rightarrow A}$

Contraction: $\dfrac{A, A, \Gamma \rightarrow B}{A, \Gamma \rightarrow B}$

[2] See [3], p. 180.

TABLE 1 *(continued)*

Int(roduction-)elim(ination) rules of NK:

	Introduction rules	Elimination rules
For "$\supset$":	$\dfrac{\Gamma, A \rightarrow B}{\Gamma \rightarrow A \supset B}$	$\dfrac{\Gamma \rightarrow A \supset B \text{ and } \Gamma \rightarrow (A \supset C) \supset A}{\Gamma \rightarrow B}$
For "$\sim$":	$\dfrac{\Gamma, A \rightarrow B \text{ and } \Gamma, A \rightarrow \sim B}{\Gamma \rightarrow \sim A}$	$\dfrac{\Gamma \rightarrow \sim \sim A}{\Gamma \rightarrow A}$
For "&":	$\dfrac{\Gamma \rightarrow A \text{ and } \Gamma \rightarrow B}{\Gamma \rightarrow A \,\&\, B}$	$\dfrac{\Gamma \rightarrow A \,\&\, B \text{ and } \Gamma, A, B \rightarrow C}{\Gamma \rightarrow C}$
For "$\vee$":	$\dfrac{\Gamma \rightarrow A \text{ or } \Gamma \rightarrow B}{\Gamma \rightarrow A \vee B}$	$\dfrac{\text{(a) } \Gamma \rightarrow A \vee B, \text{ (b) } \Gamma, A \rightarrow C, \text{ and (c) } \Gamma, B \rightarrow C}{\Gamma \rightarrow C}$
For "$\forall$":	$\dfrac{\Gamma \rightarrow A}{\Gamma \rightarrow (\forall X)\, A}$	$\dfrac{\Gamma \rightarrow (\forall X)\, A \text{ and } \Gamma, A \frac{Y}{X} \rightarrow B}{\Gamma \rightarrow B}$
For "$\exists$":	$\dfrac{\Gamma \rightarrow A \frac{Y}{X}}{\Gamma \rightarrow (\exists A)\, X}$	$\dfrac{\Gamma \rightarrow (\exists X)\, A \text{ and } \Gamma, A \rightarrow B}{\Gamma \rightarrow B}$

Notes: (a) The individual variable X is presumed in the introduction rule for "$\forall$" not to be free in Γ;
(i) The same variable is presumed in the elimination rule for "$\exists$" not to be free in Γ nor in B;
(c) $A \frac{Y}{X}$ is understood in the remaining two intelim rules for "$\forall$" and "$\exists$" to be like A except for exhibiting free occurrences of the individual variable Y wherever A exhibits free occurrences of the individual variable X.

Gentzen lavished more attention on LK, for which he proposed the axiom schema

$$A \rightarrow A$$

and the following sixteen rules of inference, in which the Greek capitals "Γ", "Δ", "Θ", and "Λ" range over (possibly empty) arrays of formulas and commas:

TABLE 2

Structural rules of LK:

	in the antecedent	in the consequent
Permutation:	$\dfrac{\Gamma, A, B, \Delta \to \Theta}{\Gamma, B, A, \Delta \to \Theta}$	$\dfrac{\Gamma \to \Delta, A, B, \Theta}{\Gamma \to \Delta, B, A, \Theta}$
Expansion:	$\dfrac{\Gamma \to \Delta}{A, \Gamma \to \Delta}$	$\dfrac{\Gamma \to \Delta}{\Gamma \to \Delta, A}$
Contraction:	$\dfrac{A, A, \Gamma \to \Delta}{A, \Gamma \to \Delta}$	$\dfrac{\Gamma \to \Delta, A, A}{\Gamma \to \Delta, A}$

Cut:

$$\frac{\Gamma \to \Delta, A \text{ and } A, \Theta \to \Lambda}{\Gamma, \Theta \to \Delta, \Lambda}$$

Introduction rules of LK:

For "$\supset$":	$\dfrac{\Gamma \to \Delta, A \text{ and } B, \Theta \to \Lambda}{A \supset B, \Gamma, \Theta \to \Delta, \Lambda}$	$\dfrac{A, \Gamma \to \Delta, B}{\Gamma \to \Delta, A \supset B}$
For "$\sim$":	$\dfrac{\Gamma \to \Delta, A}{\sim A, \Gamma \to \Delta}$	$\dfrac{A, \Gamma \to \Delta}{\Gamma \to \Delta, \sim A}$
For "&":	$\dfrac{A, \Gamma \to \Delta \text{ or } B, \Gamma \to \Delta}{A \,\&\, B, \Gamma \to \Delta}$	$\dfrac{\Gamma \to \Delta, A \text{ and } \Gamma \to \Delta, B}{\Gamma \to \Delta, A \,\&\, B}$
For "$\vee$":	$\dfrac{A, \Gamma \to \Delta \text{ and } B, \Gamma \to \Delta}{A \vee B, \Gamma \to \Delta}$	$\dfrac{\Gamma \to \Delta, A \text{ or } \Gamma \to \Delta, B}{\Gamma \to \Delta, A \vee B}$
For "$\forall$":	$\dfrac{A\frac{Y}{X}, \Gamma \to \Delta}{(\forall X)\, A, \Gamma \to \Delta}$	$\dfrac{\Gamma \to \Delta, A}{\Gamma \to \Delta, (\forall X)\, A}$
For "$\exists$":	$\dfrac{A, \Gamma \to \Delta}{(\exists X)\, A, \Gamma \to \Delta}$	$\dfrac{\Gamma \to \Delta, A\frac{Y}{X}}{\Gamma \to \Delta, (\exists X)\, A}$

Notes: (d) The individual variable X is presumed in the introduction rule for "$\forall$" in the consequent and the introduction rule for "$\exists$" in the antecedent not to te free in Γ nor in Δ;

(e) $A\frac{Y}{X}$ is understood in the remaining two introduction rules for "$\forall$" and "$\exists$" to be as in Note (c).

With the above axiom schema and rules of inference on hand, I shall say: (a) that a finite column of sequents of NK or of LK constitutes in that kalkül a proof of a sequent S of the kalkül if, first, every entry in the said column is an axiom or follows from previous entries in the column by means of a rule of inference of the kalkül, and, second, the closing entry in the column is S; (b) that a sequent S of NK or of LK is provable in that kalkül if there exists a proof of S in the kalkül; and (c) that a sequent S of NK or of LK is provable in that kalkül by means of m ($m \geq 0$) given rules of inference of the kalkül, say $R_1, R_2, \ldots,$ and R_m, if there exists a proof of S in the kalkül and every entry in the said proof that follows from previous entries in the proof follows from them by means of $R_1, R_2, \ldots,$ or R_m.

Passing on to semantical matters, let the formula associate of a sequent

$$A_1, A_2, \ldots, A_n \rightarrow B$$

of NK be B or $(A_1 \,\&\, A_2 \,\&\, \ldots \,\&\, A_n) \supset B$ according as $n = 0$ or $n > 0$, that of a sequent

$$A_1, A_2, \ldots, A_n \rightarrow B_1, B_2, \ldots, B_m$$

of LK be $B_1 \vee B_2 \vee \ldots \vee B_m$, $\sim(A_1 \,\&\, A_2 \,\&\, \ldots \,\&\, A_n)$ or $(A_1 \,\&\, A_2 \,\&\, \ldots \,\&\, A_n) \supset (B_1 \vee B_2 \vee \ldots \vee B_m)$ according as $n = 0$ and $m > 0$, $n > 0$ and $m = 0$, or $n > 0$ and $m > 0$. I shall then say that a sequent S is classically valid or, for short, valid if the formula associate of S is classically valid; intuitionistically valid, on the other hand, if the formula associate of S is intuitionistically valid.[3] I shall also say that a rule of inference R is intuitionistically sound if any sequent that follows by means of R from one or more intuitionistically valid sequents is itself intuitionistically valid.

[3] For lack of a handier criterion of intuitionistic validity, the reader may take a formula A to be intuitionistically valid if A is a theorem of Heyting's QC.

II

In one respect NK and LK run neck and neck: "Every valid sequent of either kalkül is provable in that kalkül", as Gentzen showed of LK and as is easily shown of the present version of NK. Gentzen, nonetheless, preferred LK to NK because – as he saw it – the valid sequents of LK are susceptible in LK of more direct proofs than those of NK are in NK. I should like to assess here the comparative merits of his two kalküle and justify his partiality for LK in the light of some results of his and mine.

Let a formula A be declared (i) a subformula of A, (ii) a subformula of $\sim B$ if A is a subformula of B, (iii) a subformula of $B \supset C$, $B \,\&\, C$, and $B \vee C$ if A is a subformula of B or of C, and (iv) a subformula of $(\forall X)B$ if A is a subformula of B or of $B\frac{Y}{X}$, where $B\frac{Y}{X}$ is like B except for exhibiting free occurrences of Y wherever B exhibits free occurrences of X. Next, let a finite column of sequents, say, the sequents $S_1, S_2, \ldots$, and S_q, constitute a subformula-preserving proof of S_q if the column constitutes a proof of S_q and any antecedent- or consequent-formula of any one of $S_1, S_2, \ldots$, and S_{q-1} is a subformula of some antecedent- or consequent-formula of S_q. Then, let such a column constitute a constant-preserving proof of S_q if the column constitutes a proof of S_q and any connective or quantifier letter that occurs in any one of $S_1, S_2, \ldots$, and S_{q-1} also occurs in S_q. Finally, let a sequent S be susceptible of a subformula-preserving proof if there exists a subformula-preserving proof of S, susceptible of a constant-preserving proof if there exists a constant-preserving proof of S.

The reader need only glance at the various rules of inference of LK to realize that any sequent of LK which is provable in LK by means of the rules of inference of LK minus Cut is susceptible in LK of a subformula-preserving proof. But Gentzen effectively proved – the result is known as the "Hauptsatz" – that any sequent of LK which is provable in LK is provable in LK by means of the rules of inference of LK minus Cut.[4] Hence:

[4] To simplify matters I assume here with Gentzen that no individual

THEOREM 1. *Every valid sequent of LK is susceptible in LK of a subformula-preserving proof.*

This corollary of Gentzen's "Hauptsatz" has a corollary of its own, which Gentzen did not bother to record, namely:

THEOREM 2. *Every valid sequent of LK is susceptible in LK of a constant-preserving proof.*

Since subformula-preserving proofs never stray – so to speak – from their closing entry [5] and constant-preserving ones stray less than others from that entry, the valid sequents of LK are thus susceptible in LK of direct proofs in two senses (a stronger one and a weaker one) of the word "direct."

So much, however, for LK. Consider now those sequents of NK, dubbed for the occasion *propositional* sequents, in which neither one of "$\forall$" and "$\exists$" occurs. Gentzen's intelim rules for "$\supset$", "$\sim$", "&", "v", "$\forall$", and "$\exists$" in [3] were all intuitionistically sound.[6] To permit proof of sequents which, though classically valid, are not intuitionistically valid, Gentzen accordingly had to appoint a second axiom schema for NK, to wit: $\rightarrow A \vee \sim A$. In such a version of NK a good many valid propositional sequents of NK were simply not susceptible of constant-preserving, let alone subformula-preserving, proofs, and the race was thrown from the very start. The present version of NK, however, fares much better than Gentzen's. I effectively showed in [5] that every valid propositional sequent S of NK is provable in NK by means of the structural rules of NK and the intelim rules of NK for such and such only of the connectives "$\supset$", "$\sim$", "&", and "v" as occur in S.[7] It accordingly follows that:

variable is permitted to occur both bound and free in a sequent S.

[5] To quote Gentzen's own characterization of a subformula-preserving proof, "Er macht keine Umwege." See [3], p. 177.

[6] Or, to be more exact, Gentzen's intelim rules for the said symbols and the propositional constant "f", which Gentzen also adopts as a primitive of NK (though not of LK).

[7] In [5] "$\vdash$" does duty for "$\rightarrow$" and is intended as a primitive of the metalanguage of PC, the propositional calculus, rather than a primitive of PC; all results, nonetheless, carry over. In the paper in question "$\equiv$"

THEOREM 3. *Every valid propositional sequent of NK is susceptible in NK of a constant-preserving proof,*

a result which at least puts NK back in the running.

The instructions supplied in [5] make for proofs which, besides being constant-preserving, are also variable-preserving. They unfortunately do not always make for subformula-preserving proofs, nor can they be so amended as to always make for subformula-preserving proofs, for some valid sequents of NK are simply not susceptible in NK of such proofs. Consider, for instance, the sequent

$$(p \supset q) \supset p \rightarrow p.$$

Since the sequent, though classically valid, is not intuitionistically valid, and since – among the structural rules of NK and the intelim rules of NK for "$\supset$" – only the elimination rule for "$\supset$", call it HE, is not intuitionistically sound, at least one entry in any proof of "$(p \supset q) \supset p \rightarrow p$" must, for one thing, follow by HE from two previous entries in the proof and, for another, fail to be intuitionistically valid. But among the sequents which qualify as entries in a subformula-preserving proof of "$(p \supset q) \supset p \rightarrow p$", each and every one that follows by HE from two others in the group is intuitionistically valid as well as classically valid. "$(p \supset q) \supset p \rightarrow p$" is therefore not susceptible in NK of a subformula-preserving proof. Hence:

THEOREM 4. *Not every valid propositional sequent of NK is susceptible in NK of a subformula-preserving proof.*

also figures as a primitive, a primitive governed by the following two intelim rules:

$$\text{BI:}\quad \frac{\Gamma, A \rightarrow B \text{ and } \Gamma, B \rightarrow A}{\Gamma \rightarrow A \equiv B}$$

$$\text{BE:}\quad \frac{\Gamma \rightarrow A \text{ and either } \Gamma \rightarrow (C \equiv A) \equiv (C \equiv B) \text{ or } \Gamma \rightarrow (C \equiv B) \equiv (C \equiv A)}{\Gamma \rightarrow C}$$

and the result obtained runs (mutatis mutandis): "Every valid propositional sequent S of NK is provable in NK by means of the structural rules of NK and the intelim rules of NK for such and such only of the connectives '$\supset$', '$\sim$', '&', 'v', and '$\equiv$' as occur in S."

I have not pursued this matter any further, but would conjecture that Theorem 4 holds true whatever rules of inference of the familiar sort NK may be fitted with, and hence that the valid propositional sequents of NK are at best susceptible in NK of constant-and-variable preserving proofs.[8]

An even more disappointing result is in store when it comes to the non-propositional or, if you wish, quantificational sequents of NK. Consider, for instance, the sequent

$$(\forall x)(p \vee f(x)) \rightarrow p \vee (\forall x)\, f(x).$$

Since the sequent, though classically valid, is not intuitionistically valid, and since all the structural rules of NK and all the intelim rules of NK for "$\forall$" and "$\vee$" are intuitionistically sound, no constant-preserving, let alone subformula-preserving, proof of "$(\forall x)(p \vee f(x)) \rightarrow p \vee (\forall x)\, f(x)$" is possible in NK. Hence the two negative results:

THEOREM 5. *Not every valid quantificational sequent of NK is susceptible in NK of a constant-preserving proof.*

THEOREM 6. *Not every valid quantificational sequent of NK is susceptible in NK of a subformula-preserving proof.*

I would conjecture that Theorems 5 and 6 hold true whatever rules of inference of the familiar sort NK may be fitted with. We know, for one thing, that any structural rule and any intelim rule for "$\vee$" NK may be fitted with is intuitionistically sound.[9] I suspect, for another, that any intelim rule for "$\forall$" NK may be fitted with is likewise intuitionistically sound. If this surmise of mine is borne out, then the sequent "$(\forall x)(p \vee f(x)) \rightarrow p \vee (\forall x)\, f(x)$" will not be susceptible in any version of NK of a constant-preserving proof (nor, a fortiori, of a subformula-preserving one).

Our box score thus reads:

(i) All valid sequents of LK are susceptible in LK of direct proofs in both our senses of the word "direct";

[8] Requirements to be met by a rule if it is to qualify as a structural rule or an intelim rule of such a kalkül as NK are laid down in [1].

[9] See [1] and [2].

(ii) All valid propositional sequents of NK are susceptible in NK of direct proofs in the weaker sense of the word "direct";
(iii) Not all valid propositional sequents of NK are susceptible in NK of direct proofs in the stronger sense of the word "direct";
(iv) Not all valid quantificational sequents of NK are susceptible in NK of direct proofs in the weaker (nor, a fortiori, in the stronger) sense of the word "direct".

The reader will have noticed that both "$(p \supset q) \supset p \to p$", the sequent I used to obtain Theorem 4, and "$(\forall x)(p \vee f(x)) \to p \vee (\forall x)\, f(x)$", the one I used to obtain Theorems 5 and 6, fail to be intuitionistically valid. He might, as a result, wonder whether NJ, the intuitionist kin of NK, does not fare better NK. A somewhat offhand remark of Gentzen's in [3] suggests that it does.[10] So far as I know, however, the question is still an open one.

Let NJ sport the same sequents as NK and be fitted with the same axiom schema and rules of inference as NK except the elimination rule for "$\supset$", now weakened to read:

$$\frac{\Gamma \to A \text{ and } \Gamma \to A \supset B}{\Gamma \to B},$$

and the elimination rule for "$\sim$", now weakened to read:

$$\frac{\Gamma \to A \text{ and } \Gamma \to \sim A}{\Gamma \to B}.$$

Quite a few intuitionistically valid sequents of NJ are known to be susceptible in NJ of a constant-preserving proof: (i) those in which none of "$\supset$", "$\sim$", "&", "v", "$\forall$", and "$\exists$" occurs, (ii) those in which "&" or "v" or both occur, but none of "$\supset$", "$\sim$", "$\forall$", and "$\exists$", (iii) those in which "$\supset$" occurs, either alone or along with any one, any two, any three, any four, or all five of "$\sim$", "&", "v", "$\forall$", and "$\exists$", and so on. I would, as a result, willingly surmise that the same holds true of all other intuitionistically valid sequents of NJ.[11] But I know of no proof of it, nor do I know of any proof that every intuitionistically valid sequent of NJ is susceptible in NJ,

[11] (Added in proof.) The author has since arrived at a proof of the conjecture. See his forthcoming "Two separation theorems for natural deduction."
[10] See [3], p. 177.

as Gentzen apparently believed, of a subformula-preserving proof.

LJ, however, the intuitionist kin of LK, fares as well as LK, a result due to Gentzen himself. This fourth kalkül sports sequents of only two kinds:

$$A_1, A_2, \ldots, A_n \rightarrow B,$$

where $n \geq 0$, and

$$A_1, A_2, \ldots, A_n \rightarrow,$$

where $n \geq 0$ again, and is fitted, subject to amendments (a)-(c) below, with the same axiom schema and rules of inference as LK:
(a) Permutation in the consequent and Contraction in the consequent are dropped as inoperative;
(b) "Δ" is deleted in Expansion in the consequent, Cut, the two introduction rules for each one of "$\supset$" and "$\sim$", and the introduction in the consequent rule for each one of "&", "v", "$\forall$", and "$\exists$";
(c) "Δ" is understood in Expansion in the antecedent, Contraction in the antecedent, and the introduction in the antecedent rule for each one of "&", "v", "$\forall$", and "$\exists$" to range over a single formula or no formula at all; so is "Θ" in Permutation in the antecedent; so, finally, is "Λ" in Cut and the introduction in the antecedent rule for "$\supset$".

Since Gentzen's "Hauptsatz" holds true of LJ as well as LK, every intuitionistically valid sequent of LJ is susceptible in LJ of a subformula-preserving proof and hence of a constant-preserving one.

Bryn Mawr College, Bryn Mawr, Penn., USA

Bibliography

[1] N. D. Belnap, Jr., H. Leblanc, and R. H. Thomason, On Not Strengthening Intuitionistic Logic, Notre Dame Journal of Formal Logic, **4**, no. 4, (1963) pp. 313–320.

[2] N. D. Belnap, Jr., and R. H. Thomason, A Rule-completeness Theorem, Notre Dame Journal of Formal Logic **4**, no. 1 (1963) pp. 39–43.

[3] G. Gentzen, Untersuchungen über das logische Schliessen, Mathematische Zeitschrift **39** 176–210 and 405–431.

[4] G. Gentzen, Widerspruchsfreiheit der Zahlentheorie, Math. Annalen **112** (1936) 493–565.

[5] H. Leblanc, Proof Routines for the Propositional Calculus, Notre Dame Journal of Formal Logic **4**, no. 2 (1963) 81–104.

CHAPTER 4

A MODAL LOGIC WITH EIGHT MODALITIES

STORRS McCALL

The pure strict implicational fragments of the Lewis systems S3–5 have been investigated in [5]. We find there the following axiomatic bases for these fragments, named by the authors C3–5:
Rules (common to C3–5): Substitution and *modus ponens*.
Axioms

C3: (1) $CCpqCCqrCpr$ [1]
(2) $CCpCpqCpq$
(3) $CQCpp$
C4: (1) and (2) plus (4) $CqCpp$
C5: (1) and (4) plus (5) $CCCPqPP$.

The authors of [5] established that their axiomatization of C5 was complete with respect to just those theses of S5 containing only the operator C, and similar completeness results for C3 and C4 are found in the work of Hacking [4]. This author constructs Gentzen-type systems which are complete with respect to the implicative parts of S3 and S4. He then shows these systems to be equivalent to two systems S3I and S4I, whose bases may be re-formulated to consist of the rules of substitution and *modus ponens*, and the following axioms:

S3I: (6) $CCpCqrCCpqCpr$
(7) $CCqrCCpqCpr$
(8) Cpp

[1] In this paper the operator C occurring in a Lewis system will always denote strict implication. Capitalized variables take as values only formulae which are themselves strict implications, e.g. the formula Cpq.

(9) $CPCQP$

S4I: (6) and (8) plus (10) $CPCqP$.

These axioms may be deduced from those for C3 and C4 above. Hacking also obtains completeness results for the implicational fragments of S5, S2 and Feys' System T, confirming the conjecture of Lemmon in [5] that the rule of *modus ponens* will not suffice for the axiomatization of C2.

In this paper a modal logic will be presented whose pure implicational fragment is a different one, namely Church's weak positive implicational calculus of [3]. This latter system is based on substitution, *modus ponens* and the following axioms:

WPIC: (1), (2), (8) and (11) $CCpCqrCqCpr$.

It will be noted that WPIC is stronger than C5 in containing (11) instead of C5's weaker laws of commutation (12) $CCpCQrCQCpr$ and (13) $CCpCqRCqCpR$, but weaker even than C3 in lacking (9), which C3 has.

It is a property of the Lewis systems that most of the modal features characterizing them are mirrored in those of their theses which contain only operators for strict implication and negation. Theorem 18.14 of [6] asserts the strict equivalence of Lp and $CNpp$ even in S1, and hence corresponding to every formula C, N and L there will be an equivalent formula in C and N alone. For example, to $CLpp$ there corresponds $CCNppp$, the *consequentia mirabilis* of the Scholastics. We may in fact define Lp as $CNpp$, and since every thesis which holds for strict implication also holds for material (but not *vice versa*) exhibit the C–N fragments of the Lewis systems as proper parts of classical two-valued logic (PC). This way of viewing the Lewis systems has the effect of allowing the differences between strict and material implication to emerge as differences *between* each of S1–5 and PC, rather than, as now, *within* S1–5. It has the virtues of simplicity, not calling for the mixing of different kinds of implication within the same logical system.

The modal logic of this paper, R8M, will be constructed by adding appropriate negation axioms to WPIC, and defining Lp as above. The result will be yet another C–N fragment of PC, independent of the C–N fragment of S5. Its difference from S5 will lie mainly in its different means of reducing iterated modalities. Thus in S5 we have LMp equivalent to Mp, and MLp to Lp (in S5 strings of L's and M's reduce to the last L or M), but in R8M LMp and MLp are equivalent to one another. In both S5 and R8M LL is L, and MM is M. Thus in R8M we deal with iterated modalities as in the following example. Given

$$NMLNMLLMMNp,$$

first eliminate interior N's by the equivalence of L to NMN, the rule for the replacement of equivalents, and double negation:

$$NMNMNNMLLMMNp = LMMLLMMNp,$$

then eliminate pairs of L's and M's:

$$LMLMNp,$$

then replace LM by ML, or *vice versa*, and again eliminate pairs:

$$LMMLNp = LMLNp = LLMNp = LMNp.$$

Note that in R8M $LMNp = NMLp = NLMp$. In all we have in R8M eight non-equivalent modalities, as compared with S5's six, namely the following and their negations:

TABLE 1

S5: $\begin{array}{c} Lp \\ \downarrow \\ p \\ \downarrow \\ Mp \end{array}$ R&M $\begin{tikzcd} & Lp \arrow[dl] \arrow[dr] & \\ p \arrow[dr] & & LMp\ (= MLp) \arrow[dl] \\ & Mp & \end{tikzcd}$

It remains to construct R8M. Its basis consists of the rules of substitution and *modus ponens*, and the following definitions and axioms. Those theses comprising the axioms of WPIC are starred.

Definitions

Df L: $Lp = CNpp$
Df M: $M = NLN$

Axioms

	1.	$CCpqCCqrCpr$*
	2.	$CCpCpqCpq$*
	3.	$CpCCpqq$
	4.	$CCNpNqCqp$
	5.	$CpNNp$
	6.	$CCNppp$
	7.	$CCpNpCpCpNp$
	8.	$CCCpNppCCCNppNpq$.

In what follows the key theses (21, 27, 38 and 47) for the reduction of R8M's modalities are deduced. Proof notation is based on that of [7], p. 81, with substitutions omitted. "(w, x, y, RE)" on line z means that z is the result of replacing in w one expression by another shown to be equivalent to it through the implications x and y, it being noted that in R8M the rule for the replacement of equivalents is derivable by repeated applications of theses 1, 12 and 19 below.

$1=C1-$	9.	$CCCCqrCprsCCpqs$	
$9=C9-$	10.	$CCpCqrCCsqCpCsr$	
$10=C2-$	11.	$CCpCqrCqCpr$ *	
$11=C1-$	12.	$CCqrCCpqCpr$	
$4=C5-$	13.	$CNNpp$	
$1=C5-C13-$	14.	Cpp *	
$3=C14-$	15.	$CCCppqq$	
$12=C5-$	16.	$CCpqCpNNq$	
$1=C13-$	17.	$CCpNNqCNNpNNq$	
$1=C16-C17-$	18.	$CCpqCNNpNNq$	
$1=C18-C4-$	19.	$CCpqCNqNp$	
(4, 5, 13, RE)	20.	$CCNpqCNqp$	
(6, Df L)	21.	$CLpp$	(whence $CpMp$)
(7, 5, 13, RE, Df L)	22.	$CLpCNpLp$	
(2, Df L)	23.	$CCNpLpLp$	
(22, 23, 22, RE)	24.	$CLpCNpCNpLp$	
$1=C24-C20-$	25.	$CLpCNpCNLpp$	
$1=C25-C11-$	26.	$CLpCNLpCNpp$	
(26, Df L)	27.	$CLpLLp$	(whence $CMMpMp$)
$1=C21-$	28.	$CCpNpCLpNp$	

(28, 5, 13, RE, Df L) 29.	$CLpCLNpp$
(8, Df L) 30.	$CCLNppCCLpNpq$
$1=C29-C30-31.$	$CLpCCLpNpq$
$11=C31-32.$	$CCLpNpCLpq$
$1=C28-C32-33.$	$CCpNpCLpq$
(33, Df L) 34.	$CLNpCLpCLNpNLp$
$1=C34-C11-35.$	$CLNpCLNpCLpNLp$
$1=C35-C19-36.$	$CLNpCNCLpNLpNLNp$
$11=C36-37.$	$CNCLpNLpCLNpNLNp$
(37, Df L, Df M) 38.	$CMLpLMp$
$1=C30-C19-39.$	$CCLNppCNqNCLpNp$
(39, 5, 13, RE) 40.	$CCLNppCCppNCLpNp$
$1=C40-C15-41.$	$CCLNppNCLpNp$
(33, Df L) 42.	$CLNLNpCLLNpp$
(42, 21, 27, RE, Df M) 43.	$CLMpCLNpp$
(43, 5, 13, RE, Df M) 44.	$CNMLpCLpNp$
$20=C44-45.$	$CNCLpNpMLp$
$1=C43-C41-46.$	$CLMpNCLpNp$
$1=C46-C45-47.$	$CLMpMLp$.

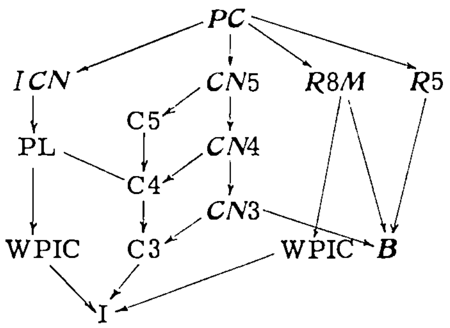
We have now proved theses permitting the reduction of R8M's modalities to the four of table 1 and their negations. The matrix in table 2, which satisfies the axioms 1–8, shows that no further reductions are possible: the implicational part of this matrix is to be found in [11], where it is used to show the non-derivability of $CpCpp$ in WPIC.

TABLE 2

C	1	2	3	4	N
*1	1	4	4	4	4
*2	1	2	3	4	3
3	1	4	2	4	2
4	1	1	1	1	1

Finally, table 3 shows the relation of R8M to other C and C–N fragments of classical logic. CN3–5 are the C–N fragments of the Lewis systems S3–5; PL is the C fragment, and ICN the C–N fragment, of the intuitionist calculus; I is what is conjectured by Anderson and Belnap to be the implicational fragment of Ackermann's system of [1], axiomatized by (1), (2), (8) and (12) above (for various axiomatizations of I see [2]); B is Moh Shaw-Kwei's weak modal base of [10], with Lp defined as $CNpp$; and R5 is another modal system of the author's (see [8]), in which occurs not only the equivalence of LMp to MLp, but also of both to plain p. In the table, C logics are in plain face, C–N logics in bold face, and the arrows denote containment.

TABLE 3



McGill University, Montreal, Canada

Bibliography

[1] W. ACKERMANN, Begründung einer strengen Implikation, The Journal of Symbolic Logic **21** (1956) 113–128.

[2] A. R. ANDERSON, N. D. BELNAP, Jr., and J. R. WALLACE, Independent Axiom Schemata for the Pure Theory of Entailment, Zeitschrift für math. Logik und Grundlagen der Math. **6** (1960) 93–95.

[3] A. CHURCH, *The Weak theory of Implication, Kontrolliertes Denken,* (Munich 1951) pp. 22–37.

[4] I. M. HACKING, What is Strict Implication? The Journal of Symbolic Logic **28** (1963) 51–71.

[5] E. J. LEMMON, C. A. MEREDITH, D. MEREDITH, A. N. PRIOR and I. THOMAS, *Calculi of Pure Strict Implication* (1956) (mimeographed). Many of the results of [5] have been published in [9].

[6] C. I. Lewis and C. H. Langford, *Symbolic Logic* (New York 1932).
[7] J. Lukasiewicz, *Aristotle's Syllogistic* (Oxford 1951).
[8] R. S. McCall, Contrariety, paper read at the meeting of the Eastern Division of the American Philosophical Association (December 1961).
[9] C. A. Meredith and A. N. Prior, Investigations into Implicational S5, Zeitschrift für math. Logik und Grundlagen der Math. **10** (1964) 203–220.
[10] Moh Shaw-Kwei, Modal Systems with a Finite Number of Modalities, reviewed in The Journal of Symbolic Logic **25** (1960) 183–4.
[11] B. Sobocinski, A Note Concerning the Many-valued Propositional Calculi, The Notre Dame Journal of Formal Logic **2** (1961) 127–8.

CHAPTER 5

ZUR SYLLOGISTIK STRIKT PARTIKULÄRER URTEILE

ALBERT MENNE

0. William Hamilton [1] hat bereits darauf hingewiesen, dass in den partikulären Urteilen der Form "Einige *S* sind *P*" angedeutet "SiP" bzw. "Einige *S* sind nicht *P*" angedeutet "SoP" das "einige" zwei Deutungen zulassen könne, nämlich

1. "wenigstens einige" (some at least; some, perhaps all: indefinite meaning)
2. "nur einige" (Some at most; some only; some, not all: definite meaning).

In der klassischen Logik wird den partikulären Urteilen stets die erste Bedeutung zugeordnet. Nur dann sind ja die Subalternationsgesetze sowie die Konversion von SiP und die Kontraposition von SoP allgemeingültig. Und nur dann ergeben sich alle 24 Syllogismen Barbara, Celarent usf. Nun hat man häufig bei logistischer Behandlung der Syllogistik [2] die vier klassischen Urteilsarten in abweichender Bedeutung definiert, so dass die Subalternationsgesetze, die unreinen Konversionen und Kontrapositionen sowie die fünf abgeschwächten Syllogismen Barbaria, Celarent usf. sowie die (deshalb auch "angefochten" genannten) Syllogismen Darapti, Felapton, Bamalip und Fesapo nicht mehr gelten.

[1] W. HAMILTON, *Lectures on Logic*, II Bde (Edinburgh 1866) S. 284.

[2] J. JÖRGENSEN, *A Treatise of Formal Logic*, III Bde (London 1931) Bd. II p. 14 Anm. 22 u. S. 126 ff. D. HILBERT and P. BERNAYS, *Grundlagen der Mathematik*. II Bde (Berlin 1934–39) Bd. I, S. 106; HANS REICHENBACH, *Elements of Symbolic Logic* (New York 1947) p. 94; HEINR. SCHOLZ, *Grundzüge der Mathematischen Logik*, II Bde (Münster 1949/50) Bd. I S. 169; D. HILBERT und W. ACKERMANN, *Grundzüge der theoretischen Logik*, 4. Aufl. (Berlin 1959) S. 59.

Wir wollen uns hier die Frage stellen, welche logischen Gesetze sich bei der Einführung des "strikt partikulären" Urteils (worunter wir ein partikuläres Urteil der zweiten Art verstehen wollen) neben oder anstatt der üblichen partikulären Urteile ergeben.

Aus der klassischen Urteilslehre benötigen wir die drei Axiome:

0.11 $\vdash$ SaP $\leftrightarrow$ P'aS'

0.12 $\vdash$ MaP $\wedge$ SaM $\rightarrow$ SaP

0.13 $\vdash$ SaP $\rightarrow$ $\overline{\text{SaP}'}$

und die drei Definitionen:

0.14 SeP =df SaP'

0.15 SiP =df $\overline{\text{SaP}'}$

0.16 SoP =df $\overline{\text{SaP}}$

und die beiden Regeln:

0.17 Für eine Termvariable (*S*, *P*, *M* und deren Komplemente) darf eine andere eingesetzt werden, wenn zugleich für alle mit der Termvariablen isomorphe mit der einzusetzenden isomorphe eingesetzt werden.

0.18 Eine doppelte Komplementation hebt sich wieder auf. (Also darf z.B. für M'' einfach *M* gesetzt werden)

Aus dem Aussagenkalkül benötigen wir die Definitionen:

0.21 $\bar{p} =_{df} p/p$

0.22 $p \wedge q =_{df} (p/q)/(p/q)$

0.23 $p \vee q =_{df} (p/p)/(q/q)$

0.24 $p \rightarrow q =_{df} p/(q/q)$

0.25 $p \leftrightarrow q =_{df} ((p/(q/q))/((p/p)/q))/((q/(q/q))/((p/p)/q)))$

0.26 $p \succ\!\!-\!\!\prec q =_{df} ((p/q)/((p/p)/(q/q)))/((p/q)/((p/p)/(q/q)))$

und die Regeln:

0.31 für eine Aussagevariable (*p*, *q*, *r*) darf eine syllogistische Aussage (z.B.MaP oder SiM usf.) eingesetzt werden, wobei für alle mit der Aussagevariablen isomorphen Aussagevariablen der syllogistischen Aussage isomorphe Aussagen eingesetzt werden müssen.

0.32 Eine Aussage darf durch eine ihr äquivalente Aussage beliebig ersetzt werden. (Also nicht unbedingt müssen alle isomorphen Aussagen hier gleicherweise ersetzt werden.)

0.33 $p \wedge q$ ist äquivalent $q \wedge p$
0.34 $\bar{\bar{p}}$ ist äquivalent p
0.35 Wenn $\vdash p \wedge q \rightarrow r$ so $\vdash p \wedge \bar{r} \rightarrow \bar{q}$
0.36 Wenn $\vdash p \wedge q \rightarrow r$ und $\vdash s \rightarrow q$ so $\vdash p \wedge s \rightarrow \mathrm{r}$
0.37 Wenn $\vdash p \rightarrow q$ und $\vdash q \rightarrow r$ so $\vdash p \rightarrow r$

Aus 0.11–0.37 lassen sich alle Gesetze der klassischen Urteilslehre und sämtliche Syllogismen herleiten.[3]

1. Das strikt partikuläre Urteil wollen wir mit "SuP" andeuten. Es lässt sich wie folgt definieren:

1.11 $\mathrm{SuP} =\mathrm{df}\ \overline{\mathrm{SaP}} \wedge \mathrm{SiP}$
durch Anwendung von 0.16 daraus:
1.12 $\vdash \mathrm{SuP} \leftrightarrow \mathrm{SoP} \wedge \mathrm{SiP}$.

Das negative strikt partikuläre Urteil wird entsprechend definiert:

1.13 $\mathrm{SuP}' =\mathrm{df}\ \overline{\mathrm{SaP}'} \wedge \mathrm{SiP}'$
daraus nach 0.15; 0.15, 0.18, 0.16; 0.33:
1.14 $\vdash \mathrm{SuP}' \leftrightarrow \mathrm{SoP} \wedge \mathrm{SiP}$
aus 1.14 und 1.12 nach 0.32:
1.15 $\vdash \mathrm{SuP} \leftrightarrow \mathrm{SuP}'$.

Das strikt partikuläre und das negativ strikt partikuläre Urteil sind also äquivalent. Mit anderen Worten: Das strikt partikuläre Urteil ist invariant gegen Postnegation.

Nehmen wir nun zusätzlich eine Regel zu Hilfe, die sich im Aussagenkalkül leicht als zulässig erweisen lässt:

1.21 Wenn $\vdash p \leftrightarrow q \wedge r$ und $\vdash q \rightarrow r$ so $\vdash r \leftrightarrow p \vee q$.
Wird darin jetzt für p SuP, für q SaP und für r SiP eingesetzt, so ergibt sich unter Berücksichtigung von 1.12, 0.16 und 0.13, 0.15:
1.22 $\vdash \mathrm{SiP} \leftrightarrow \mathrm{SuP} \vee \mathrm{SaP}$.
Durch Einsetzung P'/P und nach 1.15 sowie 0.15, 0.18, 0.16, 0.14 ergibt sich:
1.23 $\vdash \mathrm{SoP} \leftrightarrow \mathrm{SuP} \vee \mathrm{SeP}$.
Die beiden gewöhnlichen partikulären Urteile lassen sich also als zusammengesetzt darstellen aus dem strikt

[3] Vgl. A. MENNE, *Logik und Existenz* (Meisenheim 1954) S. 57ff.; auch die Vollständigkeit und Widerspruchsfreiheit der syllogistischen Spezialaxiome 0.11–0.13 wird dort S. 61f. bewiesen.

partikulären und dem positiv bzw. negativ universellen Urteil.

Nehmen wir nochmals eine Regel aus dem Aussagenkalkül zu Hilfe:

1.24 Wenn $\vdash q \leftrightarrow \bar{p} \wedge r$ und $\vdash p \rightarrow r$ so $\vdash q \rightarrow r$.
Setzen wir dort ein p/SaP, q/SuP, r/SiP so erhalten wir aufgrund von 1.12, 0.16; 0.13, 0.15:

1.25 ⊢ SuP → SiP
daraus durch P'/P, 1.15; 0.15, 0.18, 0.16:

1.26 ⊢ SuP → SoP.
Das strikt partikuläre Urteil impliziert also das positive wie das negative gewöhnliche partikuläre Urteil. Es ergeben sich also zwei neue Subalternationsgesetze.
Aus 1.25 ergibt sich mit 0.24, 0.21, 0.15, 0.14:

1.27 ⊢ SuP/SeP
entsprechend folgt aus 1.26:

1.28 ⊢ SuP/SaP
Es ergeben sich also zwei neue Gesetze der konträren Opposition.

Reine Konversion und reine Kontraposition sind für das U-Urteil nicht allgemeingültig. Analog zu den universellen Urteilen lassen sich jedoch wie dort mit Hilfe der Subalternation "unreine" Operationen als gültig erweisen.

Aus 1.25 und der auf 0.11 basierenden Konversion des I-Urteils ergibt sich:

1.31 ⊢ SuP → PiS.

Aus 1.26 und der auf 0.11 basierenden Kontraposition des O-Urteils ergibt sich:

1.32 ⊢ SuP → P′oS′.
Wird in 1.31 S/P und P/S substituiert, erhalten wir ferner:

1.33 ⊢ PuS → SiP.
Wird in 1.32 S/P′ und P/S′ substituiert, erhalten wir nach 0.18:

1.34 ⊢ P′uS′ → SoP.

2. Aus 0.12 (Barbara) und 0.35 ergibt sich:

2.11 ⊢ MaP ∧ SiM → SiP (Darii).
Hieraus gemäss 0.36 und 1.25.

2.12 ⊢ MaP ∧ SuM → SiP (Darui).
Da dieser Syllogismus mit einer strikt partikulären Prämisse dem Syllogismus Darii mit gewöhnlich partikulärer Prämisse entspricht, möge er analog dazu Darui heissen. 2.12 P/P' ergibt nach 0.14; 0.15, 0.18, 0.16:

2.13 ⊢ MeP ∧ SuM → SoP (Feruo).
2.12 nach 0.11, 0.32; P/P', M/M'; 1.15, 0.15, 0.18, 0.16:

2.14 ⊢ PaM ∧ SuM → SoP (Baruco).
2.14 M/M', 0.14, 1.15:

2.15 ⊢ PeM ∧ SuM → SoP (Festuno).
2.12 S/M, M/P, P/S' nach 0.35, 0.33; 0.15; 0.11, 0.18, 0.15:

2.16 ⊢ MuP ∧ MaS → SiP (Dusamis).
2.16 P/P'; 1.15; 0.15, 0.18, 0.16:

2.17 ⊢ MuP ∧ Mas → SoP (Bucardo).
Im Aussagenkalkül gilt die Regel:

2.18 Wenn $\vdash p \rightarrow q$ und $\vdash p \rightarrow r$ so $\vdash p \rightarrow q \wedge r$.
Wird in 2.18 p/MuP ∧ MaS, q/SiP, r/SoP eingesetzt, so ergibt sich aufgrund von 2.16 und 2.17:

2.19 ⊢ MuP ∧ MaS → Sip ∧ Sop.
Gemäss 1.14, 1.15, 0.32 daraus:

2.21 ⊢ Mup ∧ MaS → Sup (Bucardu).
Dusamis und Bucardo stellen also Abschwächungen von Bucardu dar.
2.12 S/M, M/S, P/P' nach 0.35, 0.33, 0.15; 0.15:

2.22 ⊢ MaP ∧ Mus → SiP (Datusi).
2.22 P/P'; 0.14; 0.15, 0.18, 0.16:

2.23 ⊢ MeP ∧ Mus → SoP (Feruson).
2.12 S/P, P/S; 0.33; 0.11:

2.24 ⊢ PuM ∧ MaS → SiP (Dumatis).
Aus 2.23, 0.11 ergibt sich:

2.25 ⊢ PeM ∧ Mus → SoP (Fresuson).

Wir haben also insgesamt 11 Syllogismen mit einer strikt partikulären Prämisse erhalten, darunter zwei abgeschwächte. Nur ein Syllogismus hat auch eine strikt partikuläre Konklusion, alle anderen haben eine Konklusion mit einem gewöhnlich partikulären Urteil. Es sei angemarkt, dass sich bei der Berücksichtigung von Inversen als Konklusionen 20 Syllogismen ergeben würden.

3. Wird nur das strikt partikuläre Urteil neben den beiden universellen verwandt, so erhalten wir ein dreiwertiges System:

3.11 $\vdash$ SaP v SeP v SuP.

Das ist der Satz vom ausgeschlossenen Vierten für die Syllogistik mit zwei universellen und dem einen strikt partikulären Urteil.

Bezeichnet *N* die Postnegation, so gilt:

3.12 $\vdash$ NSaP $\leftrightarrow$ SeP

3.13 $\vdash$ NSeP $\leftrightarrow$ SaP

3.14 $\vdash$ NSuP $\leftrightarrow$ SuP.

Die Gesetze des logischen Quadrates schrumpfen zusammen auf die konträren Oppositionen:

3.15 $\vdash$ SaP/SeP.

3.16 $\vdash$ SaP/SuP.

3.17 $\vdash$ SeP/SuP.

Es gibt nur den einen echten Syllogismus Bucardu mit partikulärer Prämisse:

3.18 $\vdash$ MuP $\wedge$ Mas $\rightarrow$ SuP.

Daneben gelten die fünf Syllogismen mit universellen Prämissen: Barbara, Celarent, Cesare, Camestres, Camenes.

Ausserdem könnte man alle übrigen Syllogismen, auch die mit strikt partikulären Prämissen, als "unechte" Syllogismen konstruieren, in denen die Stelle des partikulären Urteils gewöhnlicher Art durch eine Disjunktion aus einem universellen und dem strikt partikulären Urteil ersetzt wird. Als Beispiele seien geboten:

3.21 $\vdash$ MaP $\wedge$ SaM v SuM $\rightarrow$ SaP v SuP (Darii)

3.22 $\vdash$ MaP $\wedge$ SuM $\rightarrow$ SaP v SuP (Darui)

3.23 $\vdash$ MeP $\wedge$ SaM v SuM $\rightarrow$ SeP v SuP (Ferio)

3.24 $\vdash$ MeP $\wedge$ SuM $\rightarrow$ SeP v SuP (Feruo).

4. Zusammenfassend darf festgestellt werden:

4.1 Das strikt partikuläre Urteil lässt sich im Rahmen der klassischen Urteilslehre ohne Benutzung neuer Grundbegriffe definieren.

4.2 Die für die klassische Syllogistik benötigten drei Axiome reichen aus, um auch alle Gesetze mit strikt partikulären Urteilen herzuleiten.

4.3 Es werden dabei allerdings drei zusätzliche Regeln aus dem Aussagenkalkül benutzt, die sich dort als allgemeingültig erweisen lassen. (1.21, 1.24, 2.18)

4.4 Bei Einführung des strikt partikulären Urteils werden die beiden gewöhnlichen partikulären Urteile entbehrlich. (Ihre Einführung kann gelegentlich zu Abkürzungen nützlich sein.)

4.5 Da das strikt partikuläre Urteil invariant gegen die konträre Negation ist, ergibt es zusammen mit dem allgemein bejahenden und allgemein verneinenden Urteil ein dreiwertiges System von Klassenaussagen: jeder Klassenaussage kommt einer der konträren Werte zu " allgemein wahr", " allgemein falsch", " strikt partikulär wahr". ("Strikt partikulär falsch" wäre äquivalent mit " strikt partikulär wahr".) Beispiel: Die Aussage " Logiker sind Raucher" kann allgemein wahr, allgemein falsch, oder partikulär wahr sein.

4.6 Die Gesetze eines Systems mit Urteilen der Art A, E, U sind entschieden weniger als in der üblichen Syllogistik: Die Kontraponibilität von A und die Konvertibilität von E bleiben erhalten. In dem entstehenden " Logischen Dreieck" gelten drei konträre Oppositionen. Es gibt genau sechs Syllogismen.

4.7 Da die gewöhnlichen partikulären Urteile sich durch Disjunktionen darstellen lassen, kann unter deren Verwendung das System 4.6 erheblich erweitert werden, so dass es alle klassischen Gesetze und viele neue enthält.

Universität Hamburg, Deutschland

CHAPTER 6

BANKS AB OMNI NAEVO VINDICATUS

E. W. BETH

In a recent book [1] a German philosopher, G. Jacoby, makes an attempt to defend logic and its history against the pretensions of those scholars which he denotes as logisticians. His argument has the dismal quality of a voice from the past. It recalls the endless polemics of earlier generations of philosophers against such new disciplines as non-euclidean geometry or the theory of relativity.

It would be out of place to follow Jacoby's requisitory in every detail and to try to put right all the misunderstandings of which it gives proof. But it is, perhaps, not altogether useless to discuss at least a few samples of the way of proceeding that is characteristic of the book. Let us start with the following quotation.[2]

> Ein gewisser Banks erdachte 1950 ein Propagandagespräch. In ihm wirbt ein Neuerer, lies Logistiker, "Neo" sofort einen Aristoteliker, lies Scholastiker, für Logistik an und schliesslich auch einen Ignoranten "Paläo", lies Logiker. So Banks. Anders in gleichem Jahre die wirklichen Bremer Gespräche mit von Freytag. Trotzdem beginnt 1952 Clark unter Quines Auspizien sein Buch und 1959 Menne unter Bochenskis ein anderes mit Banks' Machwerk. Dagegen zitiert kein Logistiker die realen Bremer Gespräche. Wissenschaftsbetrieb der Logistik.

Poor Mr Banks! His "Aristotelian dialogue" *On the Philosophical Interpretation of Logic* [3] is concerned, as A. Menne aptly ex-

[1] G. Jacoby, *Die Ansprüche der Logistiker auf die Logik und ihre Geschichtschreibung* (Stuttgart 1962).

[2] G. Jacoby, *op. cit.*, p. 157.

[3] P. Banks, On the philosophical interpretation of logic: an Aristotelian dialogue, Dominican Studies **3** (1950); reprinted in: A. Menne (ed.), *Logico-*

plains,[4] with clarifying "without formal means the relation between classical, traditional and symbolic logic." Having gained, like the early Wittgenstein, notoriety by one single publication, he has been compelled, if I am not mistaken, to remain silent ever since. He is now blamed with having contrived a propaganda discourse which has kept logisticians from reading and quoting the real discussions that took place in 1950 in Bremen, an accusation to which he is hardly in a position to reply. I should wish here to plead his case.

My defense will be based upon a close examination of the following points:

1° The reason why logisticians refrain from quoting the "Bremer Gespräche",

2° Jacoby's reproduction of Banks's dialogue,

3° Jacoby's moral right to blame Banks for making propaganda.

In connection with point 1°, it will be sufficient to observe that in *Symphilosophein* [5] a slip of paper is pasted which contains the following announcement:

> *Zu: Symphilosophein. Bericht über den Dritten Deutschen Kongress für Philosophie. Bremen 1950.*
>
> Infolge einer erst nachträglich bemerkten Verwechslung ist der Bericht über das Symposion "Philosophische Grundfragen der Logistik" bedauerlicherweise nicht, wie angegeben, in der von Professor Dr. Arnold Schmidt revidierten Fassung zum Druck gekommen; es ist geplant, die Ausführungen an anderer Stelle in berichtigter Form zu veröffentlichen.

Unfortunately, the revised version of Schmidt's report has, to the best of my knowledge, never been given to print. It is fully understandable that, in the absence of a reliable report, logisticians prefer not to quote the "Bremer Gespräche". I may add that I agree with Jacoby to this extent that I regret the fact that, owing to the unsatisfactory way in which they have been published, the

Philosophical Studies (Dordrecht 1962) German translation in: I. M. Bochenski, *Logisch-philosophische Studien*, übers. von A. Menne (Freiburg/München 1959).

[4] A. Menne, *op. cit.*, Preface.

[5] *Symphilosophein*, Bericht über den Dritten Deutschen Kongress für Philosophie (Bremen 1950) herausgeg. von H. Plessner (München 1952).

"Bremer Gespräche" have not received all the attention to which otherwise they would have been entitled; but this is no reason to offer Banks as a scapegoat.

Turning now to point 2°, I must first note that the above quotation is not the only place where Jacoby refers to Banks's dialogue. Elsewhere [6] he reports that "to regard the syllogism as indispensable, or as reasoning *par excellence*," is called by C. I. Lewis [7] "the apotheosis of stupidity" and by Banks, "a completely mistaken view."

It will be convenient here to offer a very brief summary of Banks's dialogue. The *dramatis personae* are: Paleo, an old-fashioned scholastic who, on the basis of Aristotelian logic as he understands it, rejects modern symbolic logic; Neo, a logistician with nominalist and operationist leanings, who rejects Aristotelian logic as he understands it; and the Aristotelian, who speaks for Banks himself. So, when the curtain rises, the Aristotelian is already a partisan of mathematical logic, and therefore Neo has no need at all to "convert" him; and, when the curtain falls, Paleo is still stubbornly holding his ground. The words "a completely mistaken view" occur in a passage in which Neo points out the incompatibility of mathematical logic with Aristotelianism; this argument is applauded by Paleo but firmly contradicted by the Aristotelian; and so most clearly the words quoted by Jacoby do *not* express Banks's own opinion, as suggested.

The Aristotelian argues that both Paleo and Neo misinterpret Aristotle's doctrines and that, if only these doctrines are correctly understood, they are in perfect harmony with the principles of symbolic logic. This, and *not* the conversion to symbolic logic of the Aristotelian (who is already an adept) or of Paleo (who does not give up his opposition), is the point that Banks wants to make.

This may suffice to complete the discussion of point 2°. I feel confident that I have shown Jacoby's reproduction of Banks's dialogue to be altogether inacceptable. It is grossly misleading and must be considered a most rejectable falsification.

Before turning to point 3°, I wish to discuss another example of

[6] G. Jacoby, *op. cit.*, pp. 128–129, p. 133.
[7] C. I. Lewis, *A Survey of Symbolic Logic* (Berkeley 1918).

the manner in which Jacoby thinks fit to handle the ideas of other persons. And, since after all we are not actually in court, I believe that I may be allowed to present my own case.

At the very beginning of Part II of his book, which is devoted to historical investigations, Jacoby [8] quotes the last of the "Thesen" which, in reply to an invitation, I had submitted to the Organizing Committee of the Congress in Bremen,[9] namely:

> 5. Da andererseits die erwähnte Entwicklung von Logik und Mathematik die Erfüllung derjenigen Aspirationen darstellt, von denen letzten Endes auch die traditionelle abendländische Spekulation (Platon, Aristoteles, Descartes, Leibniz, und sogar Hegel) ihren Ausgang nahm, verfügt die spekulative Philosophie überhaupt nicht über irgendwelche Mittel, den Folgen dieser Entwicklung vorzubeugen.

In addition, he quotes similar statements made by Bernays and others and then goes on to discuss a certain number of historical data which contradict, or seem to contradict, the thesis which has just been quoted. The following lines conclude this discussion and introduce the next one.

> .... Und nie hat Mathematisierung der Logik zu Aristoteles' "Aspirationen" gehört. So weit die These Beths.
>
> Ich führe nun konkrete, mir bekannt gewordene Argumente von Logistikern über Beziehungen der Aristotelischen Logik zu Mathematik vor.

It is clearly suggested here, by implication, that I have never presented concrete arguments in support of the views that are summarized in my above thesis. And this is again a gross misrepresentation of the facts. For I have published, both before 1950 and later on, a number of books and papers which contain a wealth of facts in support of my views. Since many of these publications are mentioned in Bocheński's *Formale Logik* [10] which is frequently quoted by Jacoby, their existence cannot possibly have escaped his attention.

Jacoby's line of conduct in this connection is again an unfair

[8] G. Jacoby, *op. cit.*, pp. 65–66.
[9] *Symphilosophein*, p. 171.
[10] I. M. Bocheński, *Formale Logik* (Freiburg/München 1956).

one since it tends to restrain his readers from checking his arguments with mine and thus from living up to the ancient maxim: *audi et alteram partem.*

Turning finally to point 3°, I observe that it splits up into three points, namely:

3a The presence of propaganda elements in Banks's dialogue,

3b The objectionable character of such propaganda elements as Banks's dialogue may happen to contain,

3c Jacoby's moral right to blame Banks even for the presence of objectionable propaganda elements.

As to point 3a, it is appropriate first to take the notion of propaganda in a rather large sense. From this viewpoint, anything in an utterance that goes beyond a sober description of real facts may be considered propaganda. Even so, the notion does not fully exhaust the field of application of the word, since even a sober statement of real facts may become a piece of propaganda because of the context in which it is placed; but this possibility need not detain us here.

It cannot be denied that, in the larger sense of the word, Banks's dialogue contains a few elements of propaganda; the two which I detected on a rather close inspection of the text are the following:

(i) The Aristotelian's words: "Aristotelianism is just commonsense and scientific spirit."

(ij) The dialogue form itself; if Banks should have wished to avoid every element of propaganda, he ought to have been content with stating the relation between classical, traditional and symbolic logic in the familiar forms of scientific discourse.

It will be clearly necessary now to distinguish the milder forms of propaganda from those which are objectionable; this brings us to point 3b. Propaganda becomes objectionable as soon as it appeals to some intellectual or moral weakness in the reader or in some other manner impairs his freedom to judge of what is written; this may occur, for example, in the form of an appeal to authority or to feelings of shame or fear, or by taking advantage of the reader's lack of information.

If this criterion is accepted, then the elements of propaganda detected in Banks's dialogue cannot reasonably be said to be objectionable; for they do not, in any way, interfere with the reader's freedom of judgement.

So we now come to point 3c: Jacoby's moral right to blame Banks even for the presence of objectionable propaganda elements. In spite of Jacoby's emphatic statement to the contrary: [11]

> Das Buch ist keine Streitschrift. Es streitet nicht. Es prüft Tatbestände sachlich und zieht die Folgerungen. Sein Inhalt gleicht eher den Untersuchungsakten eines Rechtsverfahrens.

but in accordance with the specific purpose of his book, its character is most strongly polemic. And therefore it is not surprising that it contains propaganda in various forms.

As relatively inoffensive – though already less so than the above propaganda elements in Banks's dialogue – I mention such slogans as:

> *Logiker denken. Logistiker rechnen.*

or:

> *Der Zusammenbruch der Logistik in der Logik und ihrer Geschichtschreibung.*

Much more serious are Jacoby's grossly misleading reproduction of Banks's dialogue and his misrepresentation of the facts in connection with his discussion of my thesis; it will not be necessary to add a word to what already has been said about these matters.

I contend, however, that Jacoby has forsaken every moral right to blame Banks for any propaganda elements whatsoever which his dialogue may contain by using the following arguments:[12]

> Zu den Propagandamotiven der Logistiker gehört ihr Verbandbewusstsein. Nach Einstellung, Ziel, Marschroute, Wortschatz, stereotypen Wendungen und Argumenten sprechen sie als Exponenten der Ideologie eines unsichtbaren internationalen Konzerns. Dessen Macht weiss jeder Logistiker hinter sich. Sie hebt und schützt ihn nach Massgabe der Verbandinteressen in allem, was er, Logistik preisend, Logik diffamierend, über beide schreibt. In anderen Wissenschaften sprechen Selbstdenker. In logistischer Propaganda spricht mit Einheits-

[11] G. Jacoby, *op. cit.*, p. 6.
[12] G. Jacoby, *op. cit.*, p. 152.

vokabeln der Verband. Vieles stammt, oft ohne Quellenangabe, aus Scholz, Łukasiewicz, Carnap, Bocheński.

This is propaganda of a most condemnable kind, characterized by what may be called the *conspiracy argument*. This type of argument is familiar from the propaganda made by agressive groups: fascists, McCarthyists, nazis, stalinists and the like against such minorities as capitalists, freemasons, intellectuals, Jews, and liberals.

The accusation of having laid a plot against the community, the state, or the workers is spread among the public in order to arouse or reinforce feelings of fear, and then of repulsion and of hatred, against the minority that is to be attacked. If only a satisfactory state of excitement can be achieved, the mob can be trusted to take over and settle the "problem", thus creating the atmosphere of unrest and confusion that is to enable the agressors to seize the power.

Even though, unfortunately, this scheme has in several cases proved successful, it will not work unless certain conditions are fulfilled. In the first place, there must be a certain amount of discontent, caused, for example, by a bad harvest or a lost war. Next, some minority group must be available that is sufficiently exposed to feelings of jealousy and of distrust in large groups among the population to be offered as a scapegoat. And, finally, some mastermind (not necessarily a member of an agressive group, but in general a man of authority and influence) is required to contrive the paranoid idea that the bad harvest or the lost war is the outcome of a conspiracy of that particular minority and that from that conspiracy still more disaster is to be expected.

This brief digression into the domain of the theory of propaganda may suffice to show that the conspiracy argument, used by Jacoby, is characteristic of a most condemnable kind of propaganda. Moreover it may be observed that both in the above quotation and elsewhere Jacoby also uses the phraseology that is typical of all propaganda of this kind.

By way of conclusion, I may state that on the one hand Banks has been disculpated from using any except the very mildest kind of propaganda methods, and that on the other Jacoby has forsaken every moral right to blame Banks for using such methods, since he was found guilty of making propaganda of a most condemnable kind.

Jacoby [13] falls in with Scholz's observation: "Die Qualität einer Logik ist nur an ihren Früchten erkennbar." Now if this criterion is to be applied to what is proudly offered by Jacoby as a "*philosophical logic*", then it seems only reasonable to demand that we find at least some wisdom among its fruits. It is disappointing that of this wisdom Jacoby's book gives so little proof.

Epilogue

It is, perhaps, not fully satisfactory, even though it is justified with a view to the facts, that in the preceding pages we have been concerned only with details. Therefore, I may still devote a few lines to a discussion of Jacoby's argument as a whole.

The reactions of some philosophers to the rise of mathematical logic show a striking similarity to the reactions of certain people to the introduction of motorized traction in transportation.

Among these latter, I recall that of an oriental ruler, as reported by O. Neurath.[14] This prince had acquired a beautiful automobile. At his request, he was given a detailed explanation of the operation of the engine as well as an opportunity to submit the interior of the car to a close inspection. Finally, he was taught to drive the car and even to carry out small repair jobs. Nevertheless, he felt deeply puzzled when, in spite of all this, he was still unable to locate the horse.

Jacoby's attitude, however, rather resembles that of an old army general who deeply resents the introduction of motorized traction into military transportation. Such a man may argue that, even though for civilian life motorized transportation is excellent, military transportation has special demands that can only be met by horse traction. He will ascribe the supersession of the horse by the motor to a plot laid by an international concern of automobile manufacturers, civilians lacking all understanding for military affairs. This accusation he will emphasize by using slogans such as:

[13] G. JACOBY, *op. cit.*, p. 163.
[14] O. NEURATH, *Einheitswissenschaft und Psychologie* (Wien 1933).

Soldiers fight. Civilians think.

He may even argue that a specialist in motorized transportation is not qualified ever to become a historiographer of transportation, as follows. In former times, only horse traction was available and so, since no civilian can ever be trusted to treat horses properly, even civilian transportation had to be controlled by military men and therefore was organized on military principles. On the other hand, a specialist in motorized transportation, even though he may be given a military rank, remains forever at bottom a civilian and hence uncapable to grasp the workings of the military mind.

Even though, of course, it would not do to carry the matter too far, I feel that the above parable provides a fair epitome of Jacoby's train of thought; it clearly demonstrates its inherent weakness. If the motor tends to supersede the horse both in civilian and in military transportation, this is not due to a sinister plot laid by automobile manufacturers; it is a result of the fact that, for transportation of almost any kind, motorized traction has proved vastly superior to horse traction. And if at present automobile manufacturers carry more prestige than retired generals or horse-breeders, it is because they produce a commodity that is very much in demand. Moreover, since the major problems of transportation remain forever basically the same but stand out much more clearly in the present than in the past because of the ever growing variety, amount and speed of transportation, an expert in motorized transportation, if historically minded, is in a very good position to become a successful historian of his field as well.

University of Amsterdam, The Netherlands

Editor's note: We deeply regret that Professor Beth, who took an active interest in the publication of this volume, did not live to see it appear in print. His contribution was completed just before the illness which caused his untimely death. As a token of his friendship for Professor Bocheński this defense of the pseudonymous P. Banks is particularly appropriate!

CHAPTER 7

METHOD AND LOGIC IN PRESOCRATIC EXPLANATION

JERRY STANNARD

If there is one thing that students of the presocratics agree upon, it is that disagreement, ranging from textual emendations to wide sweeping differences of interpretation, characterizes modern research. Within recent years, we have studied major contributions of such scholars as Cherniss, Jaeger, Solmsen, Vlastos, von Fritz, and many others; each of whom has added much that is new and valuable to our understanding of the presocratics. Rather than attempt to assess these rival claims, I intend to draw a moral from this state of affairs. The absence of agreement, sometimes on basic matters, is a sign that further questions legitimately may be raised as to the meaning and intent of the presocratics.

The Anglo-American tradition of presocratic scholarship goes back, in large measure, to Burnet's now-classical *Early Greek Philosophy*. I do not mean by this, that Burnet's interpretation enjoys canonical status, for his views today are as often criticized as they are accepted. But the fact that scholars still feel it necessary to mention Burnet, if only to take issue with him, suggests that whatever his errors or defects may have been, they are, nevertheless, still important. For, as historians of science are fond of remarking, errors are instructive if they stimulate further inquiry. "A false theory may be as great an achievement as a true one," Karl Popper recently wrote and singled out as an instance of this, Thales' floating earth.[1]

Time and again have I pondered a passage that enunciates one of Burnet's main theses:

[1] KARL POPPER, *Back to the Pre-Socratics*. Proceedings of the Aristotelian Society **59** (1958–59) 1–24 (p. 8).

> My aim has been to show that a new thing came into the world with the early Ionian teachers – the thing we call science – and that they first pointed the way which Europe has followed ever since, so that ... it is an adequate description of science to say that it is "thinking about the world in the Greek way." That is why science has never existed except among peoples who have come under the influence of Greece.[2]

Most scholars today have reservations about accepting Burnet's enthusiastic identification of presocratic inquiry with science.[3] Yet, as easy as it may be to find flaws in his overstatement of the case, I am not prepared to dismiss it with a *non probatur*.[4]

I shall suggest, therefore, in the following exploratory sketch, that Burnet's thesis may be defended with the proviso that two conditions be satisfied. The first condition is that by "science" we understand "the formulation of an explanatory method." The second condition, one that has received considerable attention in recent years, is that the sharp distinction between "science"

[2] JOHN BURNET, *Early Greek Philosophy*. 4th ed. (London 1948) Preface p.V. Cf. POPPER *op. cit.* p. 3 "... it is good to remember ... that our Western science – and there seems to be no other – did not start with collecting observations of oranges, but with bold theories about the world."

[3] It is quite probable that Cornford is referring to Burnet when he asks, "Is the Milesian cosmogony the work of rational inference based on observation and checked by at least rudimentary methods of inference? Or are its features to be referred to an attitude of mind uncongenial to natural science as we understand it?", F. M. CORNFORD, *Principium Sapientiae* (Cambridge 1952) p. 186. As has often been pointed out, Cornford devoted his life to proving the latter alternative. Cf. W. J. MATSON, Cornford on the Birth of Metaphysics, Rev. of Metaph. **8** (1955) 443–454. WERNER JAEGER likewise singled out Burnet as representing the "perspective that emphasizes their [sic the presocratics] achievements as natural scientists." The Theology of the Early Greek Philosophers (Oxford, 1948) pp. 7, 195. On the other hand, S. SAMBURSKY, *The Physical World of the Greeks* (London 1952) has attempted to restate the presocratic contributions in terms of contemporary science, a method which is not altogether satisfactory. Cf. my remarks in Philosophy of Science **26** (1959) 155–57.

[4] Burnet's contention that φύσις meant "stuff" or Urstoff when used by the Ionians generally has been rejected. Cf. G. S. KIRK, *Heraclitus, The Cosmic Fragments* (Cambridge 1954) p. 229 and JAEGER, *op. cit.* p. 198. For Cornford's explicit criticism of Burnet, cf. Princ. Sap. pp. 4–6 and infra n. 34.

and "philosophy" does injustice to presocratic inquiry by insisting on a distinction which is anachronistic.[5]

The concept of explanatory method need not be restricted to what has come to be the paradigm of explanatory method – modern science. There are several reasons for insisting upon this. The most obvious one is that philosophy always has claimed that it is her role, as much as it is the role of science, to furnish explanations. Even granting that a distinction can be made in presocratic times between philosophy and science, it is evident that this distinction cannot be maintained on the grounds that their respective explanations apply to different sets of phenomena. And that brings up the second point, that the distinction between philosophy and science is not a presocratic distinction but one that modern scholars have imposed on the presocratic fragments. Scholars may differ as to the debt owed by Plato and Aristotle to their predecessors, but all will agree that *our* distinction between philosophy and science, in so far as it applies to the presocratics, is as much a falsification of history as those falsifications for which Plato and Aristotle have been criticized.

Today, the phrase "explanatory method" is widely used and there appears to be some agreement that the essential ingredients are three:

I. The explicit formulation of specified techniques,
II. To be followed in a stipulated sequence,
III. For the solution of a circumscribed problem.

While there are, doubtless, many possible addenda to the above, it would not be too difficult to argue that the denial of any one of the three ingredients is tantamount to saying that the remaining

[5] The attempt to soften this harsh dualism, and at the same time to emphasize a transition from mythopoesis to presocratic inquiry is one of the many notable contributions of G. S. Kirk and J. E. Raven, *The Presocratic Philosophers* (Cambridge 1957) esp. pp. 8–72. Cf. also Jaeger, *op .cit.* pp. 55–72 on Orphic theogonies. It is going too far, however, to say that "the early philosophers like Thales, Heraclitus, and Anaxagoras tried to explain the physical world in terms of its own phenomena without having recourse to philosophy or religion." R. J. Forbes, *Man the Maker* (London 1958) p. 60.

two do not satisfy the conditions generally required of an explanatory method.[6]

In order to explain a set of facts, whether or not by the conscious use of an explicitly-formulated method, certain preliminary stages are required. We would all agree, I think, that to speak of a method in the absence of specifying the content or subject matter is somehow inadequate. But that specification alone is not sufficient to describe an explanatory method. Professor Philip Wiener has recently written, in conjunction with specifying the nature of what is commonly called the history of ideas, that "No discussion of problems and methods can mean much apart from the subject-matter, interests, and general basic assumptions of the investigators".[7]

The application, if not the explicit formulation, of an explanatory method thus presupposes a prior stage which, however dimly it may be known to historians, supplied the content to which the method later applied.[8] The almost simultaneous emergence of problems and difficulties will, moreover, contribute to the limits of the method's application and to further refinements in the method itself.

For a variety of reasons it is not possible to describe the earliest stages of Greek thought, i.e., that period which preceded the application of an explanatory method. In order to do so, with some show of completeness, another *Paideia* or *Glaube der Hellenen* would be

[6] Cf. George Boas, Some Assumptions of Aristotle. Trans. Amer. Phil. Soc. n.s. vol. **49**, part 6 (1959) esp. pp. 9–11.

[7] Philip P. Wiener, Problems and Methods in the History of Ideas. J.H.I. **22** (1961) 531–48 (p. 538).

[8] This is especially true of the gradual "rationalization" of the mythological cosmogonies (cf. n. 5). This process is well summed up by Kirk and Raven *op. cit.* p. 72, "What gave these [sc. Thales and the first Ionian Philosophers] the title of philosopher was their abandonment of mythopoeic forms of thought, of personification and anthropomorphic theistic explanations, and their attempt to explain the seen world in terms of its seen constituents." This is not to say, however, that the Ionians borrowed nothing for as Burnet recognized, and has been followed by the majority of scholars, there was a continual interplay between Ionian speculation and Hippocratic medicine. Cf. infra n. 34.

required.[9] But it is possible to sketch some parts of the next stage, i.e., that period characterized by the application of an explanatory method. There is, to be certain, a third stage in which the method and its application are themselves the object of inquiry. However, this stage, generally characterized by its concern with methodology, will be ignored in the following discussion.[10]

I shall attempt to localize some of the components of presocratic inquiry that have entered into the mainstream of later, more sophisticated types of explanation. Despite the fact that the presocratics rarely spoke of method per se, there are scattered indications that there was, in fact, an *hodos* in the sort of inquiry then prevailing and the belief that, were this *hodos* to be laid out in advance, and then followed, an entrance would be made on yet uncharted territory.[11] Heraclitus' recommendation that to know many things one must be a lover of wisdom (lit. *philosophos*) announces that to know those many things, certain techniques must be adopted.[12] Diogenes of Apollonia states, in the most general terms, one requirement, but that is sufficient to indicate by its very generality, that method was indispensable, "In starting any thesis, ... one should put forward as one's point of departure something incontrovertible ..." [13] In the same spirit, Xenophanes

[9] In recent years two such attempts are noteworthy. BRUNO SNELL, *The Discovery of the Mind*, trans. T. G. Rosenmeyer (Harvard U. P., 1953) and B. A. VAN GRONINGEN, *La Composition littéraire archaïque grecque* (Amsterdam 1958).

[10] Loenen is almost alone in suggesting a methodological interpretation of the presocratics, J. H. M. M. LOENEN, *Parmenides, Melissus, Gorgias* (Assen 1959). Cf. my review, Philosophical Review **70** (1961) 423–25. SNELL, *op. cit.* 221 and others have, however, emphasized analogical reasoning as an important part of presocratic method.

[11] For the use of *ὁδός* as method, see the index to Diels-Kranz, Vol. III 302b22–35. All references to the presocratic fragments are to HERMANN DIELS, *Die Fragmente der Vorsokratiker*. 5. Aufl. edited by Walther Kranz (Berlin 1934).

[12] Heraclitus B 35. Cf. Cornford, Princ. Sap. p. 115 (where the fragment is misnumbered as B 34) and for the Pythagorean background of *philosophia*, J. S. MORRISON, The Origins of Plato's Philosopher-Statesman, Cl. Quart. **52** (1958) 198–218 esp. 208.

[13] Diog. Apoll. B.1. Although this passage has sometimes been interpreted

said "Truly the gods have not revealed to mortals all things from the beginning; but mortals by long seeking discover what is better."[14]

We can abstract from presocratic explanations and reconstruct the components which such explanations presupposed.[15] Whether the presocratics had a name to describe such a component as, say, inference, is of less consequence than the fact they made use of inferential reasoning in a wide variety of explanatory contexts. We are, accordingly, entitled to speak of inference as a component of presocratic explanatory method.[16]

Having now characterized explanatory method, let us use it as a guide by which the components of such a method may be more precisely delineated. I do not claim that the following list exhausts all of the components of an explanation – presocratic or otherwise. On the other hand, each of the following can, I think, be defended as essential to those types of explanation which, as Burnet would say, have Ionian antecedents.

1. Inference

It is not my intention in discussing this or succeeding points, to enumerate all of the instances which could serve as evidence of the point in question. Since the presocratic fragments are readily accessible, it seems more important to discuss a few fragments representative of each of the following components of explanatory method.

Inference or inferential reasoning is an essential ingredient in all forms of explanation. Obviously, inference takes many forms and at various points abuts upon other equally-essential components

in the light of ancient rhetoric, e.g., by INGEMAR DÜRING, *Aristotelis De Partibus Animalium* (Göteborg 1943) p. 71, n. 1, I agree with GUNNAR RUDBERG, Zum Stil des Diogenes von Apollonia, Symbolae Osloenses **22** (1942) 1–7 that Diogenes must be understood in terms of "ionische Wissenschaft" (p. 7).

14 Xenophanes B. 18 (Trans. Freeman).

15 Cf. CHARLES E. KAHN, *Anaximander and the Origins of Greek Cosmology* (New York 1960) pp. 5–6 for a discussion of what is involved in reconstructing presocratic thought.

16 W. A. HEIDEL has written on inference in *The Heroic Age of Science* (Baltimore 1933) pp. 92–117.

of an explanation, for example, evidence, causal connection, prediction, etc. Any explanation, moreover, is characterized by the fact that one set of data becomes more intelligible when it is brought into relation with another set of data. The connection between these two sets varies considerably; in some cases it is immediately obvious, while in other cases, the connection may demand further inquiry. A mixed type is that known as analogy.[17] The explicans must be immediately obvious, otherwise the explicandum can not be said to have been explained. When Musaeus sought to explain the cycle of birth and death, he chose to illustrate it by drawing an analogy between it and the seasonal cycle of vegetation.[18] Empedocles, too, made use of common knowledge when he likened the sea to the sweat of the earth.[19]

It was probably very early in man's intellectual career that the defects of analogical reasoning became apparent. When Anaximander likened the earth to a stone pillar he was calling attention to the limitations of analogy as a form of explanation.[20] True as it may be that the earth's fixed position in a geocentric universe may be better understood by arguing that it, like a stone pillar, has a fixed and rigid position, it cannot be denied that in many other respects there is little similarity.[21] When analogies generate more

[17] The presocratic use of analogy and analogical reasoning has been the subject of many investigations. Cf. esp. H. C. BALDRY, Embryological Analogies in Pre-Socratic Cosmogony, Cl. Quart. **26** (1932) 27–34; W. KRANZ, Gleichnis und Vergleich in der frühgriechischen Philosophie, Hermes **73** (1938) 99–122 and HEIDEL, *op. cit.* pp. 139–152.

[18] Musaeus B. 5. On the seasonal cycle of vegetation, cf. the parallels adduced by KAHN, *op. cit.* pp. 175–6.

[19] Empedocles B. 55. For other examples of what may be supposed to have been then-current knowledge and its transformation by presocratic writers, see OTTO GILBERT, Spekulation und Volksglaube in der ionischen Philosophie. Arch. f. Religionswiss. **13** (1910) 306–332.

[20] Anaximander B. 5. Cf. KAHN, *op. cit.* pp. 76–84.

[21] This is evident in Pherecydes' allegorical interpretation of Anaximander's cylindrical-shaped earth as a winged oak (Phercyd. B. 2). JAEGER *op. cit.* p. 70 follows Diels' interpretation in claiming that this allegorical account presupposes a knowledge of Anaximander's cosmology. KURT VON FRITZ s.v. Pherekydes, PAULY-WISSOWA, Real-Encyclopädie d. classischen Altertumswissenschaft Bd. XIX/2 (Stuttgart 1938) columns 2025–2033,

difficulties than they explain, another pattern of inferential reasoning is required if explanation is to continue.

A more sophisticated form of inferential reasoning is characterized by the schema, "If P then Q." Interest in this form of reasoning lies not only in its increased explanatory powers, but also in the assumption in back of it. The derivation of one statement from another rests on the assumption that the grammatical connection between statements represents a causal connection between events in the physical world. Although this assumption was not made explicit in presocratic times, the explanatory force of such an inference was recognized. On the authority of Aristotle, Thales is said to have believed that the soul was a moving force. Aristotle repeats, or at least paraphrases, the main point of Thales' arguments, "... the lodestone possesses soul because it moves iron".[22] Both the inferential form of this argument and its explanatory function are preserved when it is recast into the form, "If the lodestone moves iron, then the lodestone possesses a soul." It should be observed that the explanatory role of this inference depends upon acceptance of a causal connection between the stone's possession of a soul and its ability to attract pieces of iron.[23] For otherwise, the stone's property is left unexplained and no progress has been made, that is, the juxtaposition of the two statements is a mere irrelevancy.

It is not always easy to catch the force of ancient arguments because what has been preserved is not labelled as "premise" or as

denies any allegorizing and dismisses Pherecydes' attempts at philosophizing as characteristic of Märchenstil. (col. 2030).

[22] Thales A. 22. JAEGER, *op. cit.* p. 21 connects this line with another line from the same fragment (cf. infra n. 54) and suggests that Thales' recognition of magnetic attraction served "as a basis for a generalization about the nature of the so-called inorganic world... Thales would thus have made his observation of magnetism a premise for inferring the Oneness of all reality as something alive." It is instructive to note Burnet's opposite conclusion, "to say that the magnet and amber are alive is to imply, if anything, that other things are not," *op. cit.* p. 50.

[23] Aristotle, like Plato (cf. Ion 533D for Plato's reference to the magnet) believed the possession of soul supplied an explanation for its possessor's overt behavior. Cf. the definition of soul as οὐσία τοῦ ἐμψύχου, Ar. metaph. Z 10, 1035b15, cf. also de anima I 2,403b29.

"conclusion." [24] In other cases, however, the inferential force of the argument is self-contained as in a contrary-to-fact conditional; for example, Heraclitus' statement. "If there were no sun, the other stars notwithstanding, it would be night." [25] Whatever was Heraclitus' evidence, other references to eclipses and to the reflected light of the sun, make it clear that a causal relation was recognized between certain types of astronomical phenomena.[26] How much of this was due to observation and how much was due to speculation is a vexed question. But, for our purposes, it is important to recognise that in inferential reasoning the two coalesce. The solution as to whether Thales predicted an eclipse is hampered by our inability to separate out the observational data from the inferences, or speculations, founded on them.[27]

2. Evidence

The foregoing examples bring out a further point concerning the development of an explanatory method. By accepting the assumption which, as we have seen, permits a passage from the connection

[24] E.g., Alcmaeon's statement, "Men perish because they cannot join the beginning to the end" (B.2) could function as either a premise or conclusion. One may compare an apophthegm attributed to Thales, "The divine has neither beginning or end" (A. 1.36) Cf. also Diogenes' statement, "But this seems to me to be clear, that it is great and strong, everlasting and deathless, and multiform (B. 8). A somewhat different meaning is, I think, conveyed by Kranz' translation *ad loc*, "Doch dies ist, scheint mir, klar, dass *der Urstoff* gross und gewaltig, ewig und unsterblich und vielwissend ist."

[25] Heraclitus B. 99. For other examples of contrary to fact conditionals, see Xenophanes B. 15 and B. 38. Hippolytus reports that Anaximines (A 7.2) argued that bodies are in constant movement, "for they would not undergo change if they were not in motion."

[26] For eclipses, cf. Emped. B. 42, Anaximander A. 21. For reflected light of the sun, Parm. B. 14, Emped. B. 43–45; Anaxagoras B. 18. Anaximander A 1.1.

[27] Thales A. 1.23. The matter is sympathetically discussed by STEFAN OŚWIECIMSKI in *Charisteria Thaddaeo Sinko ... Oblata* (Varsaviae 1951) pp. 229–253. For a hypercritical interpretation which denies Thales any speculative insight whatsoever, cf. D. R. DICKS, Thales, Cl. Quart. **53** (1959) 294–309.

between statements to a connection between events in the physical world, the gap is widened between two types of evidence. That is, an explanation may employ as evidence further grammatical or logical connections uncovered, so to speak, by analysis. One such species of this is commonly known as deductive reasoning.[28] Or, the evidence may be based on sensory reports. Examples of evidence in this second sense are empirical evidence and, at a slightly further remove, prediction.[29]

Two examples will make clear the important differences between these two types of evidence, both of which are integral to an explanatory method.

When Parmenides argued that Being was One and that it was impossible to speak of Non-Being, he was employing a type of argument that contained an example in miniature of how evidence is used in a philosophic explanation. First he ruled out sensory evidence as relevant to deciding the merits of a certain type of statement. Then he explored what consequences would follow if his hypothesis that Being is One were (a) True or (b) False. If his hypothesis were true, he could consistently explain further phenomena, or further properties of Being. If, on the other hand, it were

[28] I do not mean that the presocratics had a notion of a formalized deductive system. I have discussed this matter in the paper cited infra n. 30. For this reason, KIRK, *Heraclitus, The Cosmic Fragments* p. 16 goes too far when he accuses Heraclitus, following Aristotle (cf. Heracl. B. 58 and 102), of violating the law of contradiction. There was no "law of contradiction" prior to Aristotle's formulation of logical rules. For the pre-formalized stages of Greek thought, cf. E. HOFFMANN, *Die Sprache und die archaische Logik* (Heidelberg 1925) and for what can be said of pre-aristotelian formal logic, I. M. BOCHEŃSKI, *Ancient Formal Logic* (Amsterdam 1951) pp. 14–18.

[29] In addition to Thales' alleged prediction of an eclipse (supra n. 27), there are other examples of the same type. E.g., Anaximander (A 5a) and Pherecydes (A 1.116) are supposed to have predicted earthquakes while Anaxagoras (A 11–12) is credited with predicting the spectacular meteorite which fell at Aegospotami ca. 467. Cf. F. S. KIRK, Some Problems in Anaximander Cl. Quart. **48** (1955) 21–38 esp. p. 30. For empirical evidence in presocratic cosmology cf. F. SOLMSEN, Aristotle and Presocratic Cosmogony, H.S.C.P. **63** (1958) 265–82 esp. 274 n. 54 and the controversial claims made by OLAF GIGON, *Untersuchungen zu Heraklit* (Basle 1935) concerning Heraclitus' use of empirical evidence.

false, then the truth-value of other statements must be revised. But these, commonly accepted as false, could not, without serious misgivings, be supposed to be true. From this it followed, claimed Parmenides, that his initial supposition must be true.[30]

For evidence in the second sense, we shall turn to Empedocles. While it cannot seriously be maintained that he was an evolutionist, still less a Darwinist, the very fancifulness of this *fin de siècle* comparison, draws attention to an important component of explanatory method.[31] The enormous difference between Empedocles' and Darwin's method must not obscure one point of resemblance. Although rather trivial by modern standards, this one point is of the utmost historical importance. The resemblance concerns their use of evidence and the consequences which each drew from that evidence. It is idle to pretend that Empedocles' evidence was of the same nature as Darwin's or that each obtained his evidence in the same manner. Nevertheless, the fact remains that in each case their respective theories and generalizations were supported by evidence.

In several cases, Empedocles cites the evidence that supports his claims, for example, his belief that, in some sense, hair, leaves, feathers, and scales were comparable.[32] Whether or not this permits the historian of biology to attribute to Empedocles a knowledge

[30] Cf. my paper, Parmenidean Logic. Philos. Rev. **69** (1960) 526–33.

[31] The older literature is vitiated by failure to distinguish the important methodological differences between Greek biological speculation and the conceptual refinements and testable consequences of Darwinian evolutionary theory. Rádl summed up the matter succinctly when he wrote, "In almost every philosophical writer from Empedocles and Aristotle to Goethe, the inquiring eye of faith of some historian would discover a masked Darwin," EMANUEL RÁDL, *The History of Biological Theories*. Trans. E. J. Hatfield (London 1930) p. 7. None the less, the belief that the presocratics were "masked Darwins" continues unabated. E.g., the well-known biologist Ludwig von Bertalanffy recently stated, "The first Darwinist to explain organic 'fitness' on the basis of events at random was the pre-Socratic Empedocles". LUDWIG VON BERTALANFFY, *Problems of Life* (New York 1952) p. 106. A detailed investigation of the entire problem is nearing completion and will appear elsewhere.

[32] Emped. B. 82. For an attempt to place this fragment within the Empedoclean context, cf. F. SOLMSEN, *Aristotle's System of the Physical World*, (Ithaca 1960) p. 113[39].

of homologous structure, the mere possibility of such an attribution depends on the fact that Empedocles has cited precisely the evidence that would justify the attribution. His famous clepsydra experiment, like that of Anaxagoras', provides another example.[33] In these cases, leaving aside the vexed question of experimentation, we learn something further of ancient explanatory method. For in these cases, not only is their evidence cited, thus permitting a duplication, but the theories based on that evidence are communicated as well.[34]

Today, as well as yesterday, the distinction between description and explanation is sometimes blurred. Despite the excellent admonition of Julius Sachs, "Was man nicht gezeichnet hat, hat man nicht gesehen", in fields as widely separated as astrophysics and genetics, it is difficult to avoid imposing explanatory or interpretative categories when describing a complex process.[35] In presocratic times it was not easy to avoid blurring the distinction between description and explanation.[36] Nor is it easy for us to determine

[33] Empedocles B. 100, Anaxagoras A. 69. The old belief that the clepsydra in Empedocles meant "water clock" (oddly still retained by GEORGE SARTON, *Introduction to the History of Science*, (Baltimore 1927) I, 87) was corrected by HUGH LAST, Empedocles and his Clepsydra again. Cl. Quart. **18** (1924) 169–73. Cf. the article by Furley cited infra n. 58.

[34] On the controversial question of experimentation in the presocratics cf. Burnet *op. cit.* p. 27 and Cornford's criticism there of, cited supra n. 4. Much valuable material on the presocratics is found in G. SENN, Über Herkunft und Stil der Beschreibungen von Experimenten im Corpus Hippocraticum. Arch. Gesch. Med. **22** (1929) 217–89 esp. pp. 269, 274–81. Senn's work, like Heidel's discussion of experiments, *op. cit.* pp. 153–194, emphasizes the close relationship between presocratic and Hippocratic patterns of inquiry. Cornford's last word on the subject, his posthumously published, Was the Ionian Philosophy Scientific?, J.H.S. **62** (1942) 1–7 makes one concession – for he admits (p. 3) that "Alcmaeon even tried, by dissection, to trace the 'pores' leading from the sense organs to the brain."

[35] Quoted from AGNES ARBER, *The Mind and the Eye* (Cambridge 1954) p. 121.

[36] Parmenides' statement "(I will describe) how earth and sun and moon, and the heavens common to all, and the Milky Way in the heavens, and outermost Olympus, and the hot power of the stars, hastened to come into being" B. 11 (trans. Freeman) may be taken as a description of astronomical phenomena, so KAHN *op. cit.* p. 148 or as an explanation. For a good discussion of this and related matters, cf. W. J. VERDENIUS, Parmenides' Conception of Light, Mnemosyne **2** (1949) 116–131.

what theories were based on relatively pure descriptive evidence.[37] It is, therefore, to miss the point of, say, Empedocles' clepsydra experiment if it be said that the experiment was inconclusive or that experimental controls were wanting. It is no more just to label as false Xenophanes' assertion that "We all have our origin from earth and water."[38] The very fact that the truth-claim of an assertion, offered as an explanation, depends on evidence, is a noteworthy step forward. No less important is the fact that this evidence must be selected out of a much larger collection of possible data. How the presocratics faced this issue is our next point.

3. Classification

Once evidence becomes important in the support or substantiation of an explanation, a methodological problem arises. Assuming that much of the evidence available to the presocratics was of the sort that could be obtained with a bare minimum of specialized equipment or instrumentation, there remains the problem of using it effectively. What, for example, was Xenophanes to do with the mass of fossilized remains he observed on Paros and elsewhere? [39] Classification first arose in the attempt to catalogue such accumulated evidence. By "classification," one must not think of the elaborate

[37] Despite an abundant literature, it is still uncertain what theoretical foundation Diogenes of Apollonia's description of the human vascular system was meant to serve (B. 6). Aristotle, our source for the passage, added no comments and apparently understood it as a description. Later, Simplicius (ad B. 6) took the passage to refer to Diogenes' theory of ἀήρ as νόησις (cf. B. 5), neither of which is mentioned in B. 6. Later Greek physicians, according to Vindicianus, adopted Diogenes' description without assenting to his physiological or psychological theories. Cf. MAX WELLMANN, *Die Fragmente der sikelischen Aerzte* (Berlin 1901) pp. 51–52.

[38] Xenophanes B. 33. Cf. B. 29 "All things that are generated and grow are earth and water." Statements of this form, a "rudimentary physical theory", as KIRK and RAVEN *op. cit.* pp. 176–77 term it, probably represent an attempt to explain the fact that growth is always accompanied by moisture and nourishment. Cf. JOHN McDIARMID, Theophrastus on the Presocratic Causes, H.S.C.P. **61** (1953) 85–156 esp. pp. 135–36.

[39] Xenophanes A. 33. Xenophanes was not the only one in antiquity to recognize fossils. Cf. the references assembled by A. S. PEASE, Fossil Fishes Again, Isis **33** (1942) 689–90.

structures current in the heyday of the descriptive sciences and illustrated by Linnaeus' binomial classification. Rather, one must look to the earliest forms of classification – dividing, and collecting, or, as Plato later will term them, *diairesis* and *synagōgē*.[40] These techniques, as simple as they may be, contain the essentials of classification and can thus claim a place in the development of explanatory method. The recognition of a criterion, whether it is made explicit or not, by which one divides particulars into several groups or collects discrete particulars into a group, is the minimal requirement for a classification.[41] An elementary example, in the sense that the criteria are not explicit, and that no clear distinction between collecting and dividing is discernible, is the fragment of Democritus' almanac-like classification of weather-signs.[42]

In antiquity, the search for a criterion often took the form of a search for some factor common to a group of particulars which for some implicit reason, were roughly lumped together, for example, Heraclitus' catalogue of "Night-ramblers, magicians, Bacchants, Maenads, Mystics".[43] It makes no difference whether this common-factor was imposed in an arbitrary fashion, for as logicians have taught us, any property can serve to define a set. Again, Heraclitus

[40] Cf. my paper, Socratic Eros and Platonic Dialectic, Phronesis **4** (1959) 120–34.

[41] For a classification based on several explicit criteria, see the Hippocratic treatise De Victu esp. II cc. 39–49. This has been carefully studied by Robert Joly, *Recherches sur le traité pseudo-hippocratique Du Régime* (Paris 1960) pp. 93–121. For a different interpretation see my paper, Hippocratic Pharmacology, Bull. Hist. Med. **35** (1961) 497–518.

[42] Democritus B. 14. Although Democritus emphasized the need of "finding a causal explanation" (lit. *aitiologia*, B. 118) the widespread failure in antiquity to distinguish between causal connection and correlation (cf., e.g., Hippocr. de aere 32; humor. 20) was one of the factors which hindered progress in meteorology.

[43] Heraclitus B. 14. The passage continues "the rites accepted by mankind in the Mysteries are an unholy performance" (trans. Freeman). Rather than the somewhat envolved religious interpretation given by Wilhelm Nestle, *Vom Mythos zum Logos*, 2. Aufl. (Stuttgart 1942) pp. 99–100, I prefer to agree with Gregory Vlastos, Equality and Justice in Early Greek Cosmologies, Cl. Philol. **42** (1947) 156–178, that Heraclitus is taking such groups as representing the "many" who are "philosophically benighted" (p. 166).

provides an example by his assertion that "Reason is common to all", that is, with respect to the possession of reason, there is a property common to members of a certain set and that this property serves to distinguish that set from other sets not so characterized.[44]

Thus, the search was on in presocratic writers to find the common factor which by its presence or absence was a sufficient condition to collect or divide particulars, with respect to that criterion, into their respective classes. Instances of the attempt to classify and thus explain in a unified fashion the set so defined occur in the early stages of presocratic meteorology. The common factor sought after may take a variety of forms, but it will suffice to mention one representative type of classification.

Anaximines' primal element, air, unlike Anaximanders' *apeiron* was determinate. Of this material, it was said that "it differs in different things according to its rarity or density. In its rare form it gives rise to fire and in its dense form it gives rise to wind from which come clouds and when more dense water; in turn, comes earth, then stones, and from them everything else."[45] The importance of this passage is twofold. It is a classification of meteorological phenomena in terms of the criteria "rare" and "dense."[46] But more than that, in so classifying a diverse group of meteorological phenomena, Anaximines has also supplied an explanation of their origin

[44] Heraclitus B. 113. For a proper understanding of the epistemic vocabulary of the presocratics, cf. KURT VON FRITZ, ΝΟΥΣ, ΝΟΕΙΝ, and their Derivatives in Pre-Socratic Philosophy. Cl. Philol. **40** (1945) 223–42.

[45] Anaximines A. 5. JAEGER *op. cit.* p. 36 compares B. 2, "As our soul, being air, holds us together, so do breath and air surround the whole universe" (trans. Freeman) and notes, "In his attempt to identify Anaximander's infinite first principle with air and to derive everything from the metamorphoses which air undergoes, Anaximines has undoubtedly been guided also by a feeling that the first principle ought to explain the presence of life in the world. . . ."

[46] On rarefaction-condensation and related meteorological phenomena cf. KAHN *op. cit.* p. 160 and WILHELM CAPELLE, s.v. Meteorologie, PAULY-WISSOWA, *Realencyclopädie* ... Suppl. Bd. VI (Stuttgart 1935) esp. coll. 327–338. Cherniss, in referring to Anaximines' "mechanism of condensation-rarefaction" adds that this mechanism is "in modern terms ... a function of the variation of density," HAROLD CHERNISS, The Characteristics and Effects of Presocratic Philosophy, J.H.I. **12** (1951) 319–45 (p. 328).

and nature. In a similar fashion, modern phylogenetic taxonomies not only classify, but in the process of classifying also explain the ancestry of orders and families within the larger taxonomical groupings.

4. Hypothesis

The fragment of Anaximines, just discussed, will serve to illustrate another step in the development of explanatory method. "Rare" and "dense" have been rejected as criteria for classifying and explaining meteorological phenomena, but it does not follow that such an attempt at classification was lacking in explanatory power.[47] On the basis of these criteria, hitherto unrelated meteorological phenomena were unified and given a consistent explanation. Although the explanation turned out to be factually false, the fact remains that it was internally consistent because of the rigor in which these, and no other criteria, were applied in order to explain the data in question.[48] The adoption of a criterion on the strength that it will unify and explain, plus the fact that the resulting explanation will be, at least with respect to that criterion, internally consistent, may be termed a hypothesis. At the moment of its adoption a hypothesis is neither true nor false. It is, rather, a well-phrased question or a poorly-phrased question. "The method of approach to any scientific problem," Julian Huxley cone wrote, "is itself largely dictated by the type of answer you want to obtain, it is, in fact a kind of question".[49] It may turn out that the hypothesis is confirmed, in which case it is in line for promotion to a theory or even a law, or it is rejected. In either case, it directs attention to

[47] JAEGER, *op. cit.* p. 123 contrasts Heraclitus' "cosmology" with the Milesian philosophers who "in line with their basic assumptions, tried to explain the emergence of the world from the one primal ground by resorting to purely physical hypotheses such as separating-out or rarefaction and condensation." In a note ad loc. Anaximander A. 9 and Anaximines A. 5 are cited as examples.

[48] Cf. MATSON *op. cit.* pp. 445, 450, 452 and CHERNISS *op. cit.* pp. 328, 336 for different accounts of what constitute rigor and consistency.

[49] JULIAN HUXLEY, *Knowledge, Morality and Destiny* (New York 1960) p. 68.

one problem or a certain class of data, for which better answers are required than those hitherto available.

The ascription of hypotheses to the presocratics – from vague suggestions to what we might call theory formation – is a difficult matter to sum up adequately. This is due, in large part, to the many cross-currents that entered, at a very early time, into the formation of the pattern of presocratic inquiry. It appears that while there are many such strands, two deserve special notice: (i) pure speculation and (ii) the extension of analogies, drawn from a biological, moral or political domain, to cosmological dimensions.[50]

Of the presocratic doctrines falling within these two classes, some may be called hypotheses in the sense that they functioned as questions. That a concept served as a question need not preclude the inclusion of an answer in its explication. For hypotheses, in the sense of questions as well as answers to those questions, have as their purpose the demand for increasingly better explanations of data.[51] The data may have been localized by some previous step in the attempt to explain, e.g. by classification, but this is by no means always the case. It is probably more frequent that it is a well-directed question which isolates a certain class of data out of a larger set that, up to the time of the question, was either taken for granted or given a mythological explanation.

Like hypotheses in the modern sense, Alcmaeon's *isonomia*, Heraclitus' *logos* or even Thales' floating earth, served their purpose by unifying data not otherwise or not hithertofore amenable to inquiry, and hence explaining them. Although fragmentary evidence prevents us from judging, in systematic terms, presocratic writings, there is some evidence that Empedocles and Anaxagoras attempted

[50] The best statement of "the naturalization of justice" is Vlastos (cited n. 43) to whom I owe this useful phrase. For the generalization of biological analogies, cf. supra n. 17 and the recent study by H. A. T. Reiche, *Empedocles' Mixture, Eudoxan Astronomy and Aristotle's Connate Pneuma* (Amsterdam 1960).

[51] Cherniss *op. cit.* p. 329 describes Anaximander's physical system as the "ultimate achievement" of Milesian philosophy. He continues by noting that the answers given by Anaximander were such as to call attention to the "limitations which were to become important and stimulating problems for subsequent thinkers."

to explain, in a systematic manner, a wide range of data.[52] It would appear that their efforts at a system depended upon the explanatory usefulness of their hypotheses. Because Anaxagoras' *nous* and Empedocles' *philia kai neikos* have been superseded as explanatory categories, their role as hypotheses is seen more clearly. By its very nature, an hypothesis is provisional and its replacement by a more adequate or fertile suggestion reinforces the claim that a hypothesis serves its purpose best when it initiates further inquiry.[53]

5. Generalization

As indicated previously, one way be which a hypothesis is tested in preparation for its eventual acceptance or rejection, is the degree to which it can be generalized. If it will explain, when applied to data not specifically included in its initial formulation, it may be termed a generalization. Thus, whatever we may think about Thales' statement that "all things are full of Gods," it is clearly a generalization and, perhaps for that reason, it was the more easily found to be lacking in explanatory power.[54] Generalizations are, however, not restricted to the testing of hypotheses for, as Beveridge has recently noted, "Generalizations can never be *proved* [but] they

[52] The belief in the presocratics as systematic thinkers is well-stated by BURNET *op. cit.* p. 6. Hesioid's Theogony, he writes, "is an attempt to reduce all the stories about the gods into a single system, and system is fatal to so wayward a thing as mythology." For Empedocles as a system-builder, cf. CHARLES E. KAHN, Religion and Natural philosophy in Empedocles' Doctrine of the Soul, Arch. Gesch. Phil. **42** (1960) 3–35; for Anaxagoras, cf. GREGORY VLASTOS, The Physical Theory of Anaxagoras, Philos. Rev. **59** (1950) 31–57.

[53] This is well illustrated by Solmsen's contention that Parmenides' denial of change and plurality caused a crisis in Greek philosophical thinking. After him, "the future actually unfolds by positing now this and now that concept that he had rejected (and rejecting a corresponding antiphysical concept that he had posited) . . . .", Aristotle's System . . . p. 5.

[54] Thales A. 22. Cf. supra n. 22 and CHERNISS, *op. cit.* p. 322, "To say that all things are full of gods, for example, may be to mean that all things are divine in a mystical or religious sense or equally to mean that *nothing* is, in the way that the author of the Hippocratic essay, 'On the Sacred Disease,' asserts that all diseases are divine and all human, meaning that no disease is sacred in the religious sense at all."

can be tested..." [55] Once a hypothesis has been accepted, generalization supplies the means of extending its range of application to the point that it becomes, in its own right, an essential component of explanatory method. Presumably Heraclitus' statement that "Every creature is driven to pasture with a blow," is a generalization which either supports his system as a premise or is derivable as a conclusion from other statements.[56] This relation of generalization to the other portions of a system comes out more clearly in Empedocles' observation. "Thus all (sc. creatures) have a share of breathing and smell."[57] The force of the particles (*hōde men oun*) suggests that this statement was a conclusion Empedocles derived from his observations on the physiology of respiration, fragments of which still remain.[58]

Much has been written about the origin of Natural Law and Laws of Nature though it is difficult to determine when the idea first became crystallized in a form which we would recognize today. [59] While it is true that the earliest of the presocratics did not use the phrase *nomos tēs physeōs*, it seems probable that they viewed the succession of events and natural cycles in law-like terms.[60] For

[55] W. I. B. BEVERIDGE, *The Art of Scientific Investigation* (New York 1957) p. 118.

[56] Heraclitus B. 11. However, KIRK, *Heraclitus, The Cosmic Fragments* pp. 258–59 states that although the context of this fragment "asserts that all things in nature, plants and animals as well as the heavenly bodies, behave as parts of a single organism", it tells us "nothing of what Heraclitus intended."

[57] Empedocles B. 102.

[58] Empedocles B. 100–101. For Empedocles' theory of respiration cf. D. J. FURLEY, Empedocles and the Clepsydra. J. Hellenic Studies **77** Pt. 1 (1957) 31–34.

[59] JAEGER *op. cit.* p. 16 finds "Natural law" as early as Hesiod, but the more common view is that natural law, if it occurs in the presocratics at all, is to be found first in Anaximander, cf. KAHN *op. cit.* p. 191 and CHERNISS *op. cit.* p. 327. SAMBURSKY *op. cit.* p. 161 by interpreting natural law as causal law argues that it was first introduced by Democritus.

[60] The phrase occurs first in Plato, Gorgias 483 E. E. R. DODDS, ed. *Plato Gorgias* (Oxford 1959) states (*ad. loc.*) that this phrase is "not to be confused ... with the modern scientist's 'laws of Nature' which are simply observed uniformities ... [it] is not a generalization about Nature but a rule

example, the so-called "Attraction of Likes", despite its limited application, is capable of being stated in law-like terms which apply to the whole of nature.[61] In the same manner, both the earlier doctrine of opposites and the later, primarily atomistic account of mechanical regularity, can be interpreted as attempts to explain the order (*taxis*) of the cosmos.[62] This law-like behavior of natural phenomena was quickly seized upon as part of the explanation of those phenomena. One has only to think of the famous first fragment of Anaximander to note the explanatory force of a generalization. Whether Anaximander generalized from the primitive *lex talionis* or whether the pregnant phrase *kata to chreōn* was originally formulated with reference to cosmological explanation is a matter of conjecture.[63] None the less, any interpretation of Anaximander's role in philosophy and/or science must make room for the generalized, and at the same time, the explanatory force of his claim that natural phenomena "give justice and make reparation to one another for their injustice, according to the arrangement of Time." [64] We do not know what Anaximander would have accepted

of conduct based on the analogy of 'natural' behaviour." This explanation, however, only doubtfully applies to Plato's later usage in Timaeus 83E. It is the latter passage, not the former, that Aristotle seems to refer to in Rhet. I. 13, 1373b6.

[61] Cf. Emped. B. 109 and KURT VON FRITZ, *Democritus' Theory of Vision*, *in Science, Medicine and History.* Essays on the Evolution of Scientific Thought and Medical Practice Written in Honour of Charles Singer (Oxford 1953) pp. 83–99. Von Fritz has studied carefully the use of the doctrine of "Like to Like" in the explanation of physical and cosmic questions.

[62] A typical statement of the use of opposites in cosmology is Anaximander A. 16. Cf. KAHN *op.* pp. *cit.* 119–165 on "Elements and Opposites." Leucippus B. 2 sums up the atomistic belief in mechanical regularity, "Nothing happens at random; everything happens out of reason and necessity" (trans. Freeman). CYRIL BAILEY, *The Greek Atomists and Epicurus* (Oxford 1928) 90–101 emphasizes Leucippus' dependence on Anaximander and Anaxagoras and his attempt to give "coherence and a scientific character" to cosmology (p. 97). For *taxis*, cf. infra nn. 64–65.

[63] For a resumé of older interpretations, cf. Vlastos (cited n. 43) pp. 169–70 and for the numerous conceptual and textual difficulties, cf. KAHN, *op. cit.* pp. 166–196.

[64] Anaximander B. 1 (trans. Freeman). The word "arrangement" is a translation of *taxis*, which JAEGER, *Paideia* I (New York 1945) 159 would

as a legitimate deduction, but that he intended his statement to be generalized seems clear from the last clause concerning the universality of time. For time is one of the *metra* by which the dimensions of a generalization are determined.[65]

6. Guarantee

The ultimate test of any method is its effectiveness. But, as we all know, this an uncertain guarantee, for success in one or a few instances does not necessarily entail success in all cases. Perhaps one of the most striking differences between ancient and modern explanatory methods lies at precisely this point. When we have reason to believe that an explanatory method will always prove to be reliable, we are assuming that the correct application of the method, with all that it entails in the way of controlling external conditions, parameters, etc., is in itself its own guarantee. That is, the gurantee, such as it is, is internal to the method. It is for this reason that some scientists speak of their method as "self-correcting." Thus, Louis Pasteur defended "this marvelous experimental method" on the grounds, "not that it is sufficient for every purpose, but that it rarely leads astray, and then only those who do not use it well." [66]

It is otherwise with those methods which have been rejected on the grounds that their results are not always reliable and that no margin of error or limits of application are included in their formulation. A major reason why these methods have been rejected lies in the nature of their guarantee of reliability. Generally, a method is found to be objectionable, or at least highly suspect, when the guarantee of its reliability is external to it. As Carl Becker

prefer to translate as "ordinance" in order to emphasize the legal metaphor. This claim has been contested by McDiarmid, cf. next note.

[65] Cf. Diog. Apoll. B. 3. McDiarmid *op. cit.* p. 141 has argued against Jaeger's interpretation of *taxis* in Anaximander B. 1 on the grounds that the phrase, κατὰ τὸ χρεών, paraphrased by Theophrastus as κατὰ τὴν τοῦ χρόνου τάξιν emphasizes a temporal rather than a juridical meaning. For time as a *metron* cf. Vlastos, (cited n. 43) 161 and for *taxis*, G. S. Kirk, Natural Change in Heraclitus, Mind **60** (1951) 35–42 esp. p. 36.

[66] Quoted from René Dubos, *Pasteur and Modern Science* (New York 1960) p. 22.

put it, "No serious scholar would now postulate the existence and goodness of God as a point of departure for explaining the quantum theory or the French Revolution." [67] An extra-methodological guarantee is a not uncommon device in presocratic writings. One thinks of Empedocles' invocation to the gods as providing him with a guarantee or of Heraclitus' emphatic distinction between human fallibility and divine truth.[68] In both cases, the feeling of certainty that permeates their assertions depends as much on a prior belief in the nature of the guarantee or guarantor as in the testable consequences of the method itself. When Philolaus stated that "there are certain thoughts which are stronger than ourselves," [69] this certainty, like that felt by Parmenides [70] is no different psychologically from that felt by Archytas in arriving at a mathematical proof;[71] the difference lies in the location of the guarantee.

The fact that ancient and modern explanatory methods differ on the location of their respective guarantees, does not mean that the ancient demand for an extra-methodological guarantee is historically unimportant. For if one compares the type of expla-

[67] CARL L. BECKER, *The Heavenly City of the Eighteenth-Century Philosophers* (New Haven 1932) p. 16.

[68] Empedocles B. 3, Heraclitus B. 78. The belief that man is inferior to the devine is common in presocratic fragments, e.g., Xenophanes B. 23, Epicharmus B. 23, etc., and possibly is part of the epic tradition which underlies many presocratic thought-patterns. Cf. C. M. BOWRA, The Proem of Parmenides, Cl. Philol. **32** (1937) 97–112. E. R. DODDS, *The Greeks and the Irrational* (Berkeley 1951) p. 51, n. 6 minimizes the point of Heraclitus' distinction when he states that B. 78 represents "man's lack of insight into his own situation."

[69] Philolaus B. 16.

[70] Parmenides B. 2.1. W. R. CHALMERS, Parmenides and the Beliefs of Mortals, Phronesis **5** (1960) 5–22 suggests that *pistis* (B. 1.30) is more adequately rendered by "reliability", "evidence," or "genuine conviction" than by the more common "belief."

[71] Several examples of Archytas' ability in mathematics and geometry still survive. A. 14 is a solution in "three dimensions" of the celebrated problem of the duplication of the cube while. A. 19 contains a theorem for a superparticular ratio. Cf. IVO THOMAS, *Greek Mathematical Works I* (London 1957) pp. 284, 130 for the above fragments and Archytas B. 4. Whether the latter fragment is genuine or not, it is a summary statement of the logical powerfulness of a deductive proof.

nation preceeding from a mythopoetic cosmogony, as Hesiod's *Theogony*, with a presocratic cosmological explanation, it will be observed that one of the fundamental differences is the latter's concern that there be a guarantee. It matters not, at this point, whether the guarantee is internal or external to the method. For the belief that the purpose of a method is to provide a consistent explanation is in sharp contrast to the inconsistencies permitted in mythological explanations. Inconsistencies, to say nothing of factual errors and conceptual extravaganzas as the Orphic World Egg, were apparently not felt to be defects in mythologies because there was no standard or *logos* by which they might be judged, as there would have been had a guarantee been included.[72]

The frequent occurrence of the word "Truth" in presocratic writings testifies to the care they took lest they be confused with their mythologizing predecessors.[73] Even the equally-frequent denial that truth can be attained points in the same direction for, in no case in presocratic literature, does the denial of the attainability of truth lead to a suspension of the method which permits that assertion. Neither Xenophanes nor Democritus had any doubts about the compatibility of the usefulness and reliability of his method with the claim that truth cannot be attained.[74]

Although truth is only one form of a guarantee, it cannot be doubted that historically, any method which claimed to lead to, or was supported by, truth had an initial advantage over a method which made no such claims.[75] For, as Epicharmus said, "No sooner

[72] Cf. MATSON, *op. cit.* p. 447, "The criteria of philosophers and shamans have nothing in common, for the reason that the shamans had no criteria."

[73] The index in Diels-Kranz (Vol. III pp. 32–34) s.v. alētheia, alēthēs and other cognates, occupies over 3 columns. JAEGER *op. cit.* p. 94 stated that the presocratic emphasis on truth goes back to Hesiod who gave to the word "truth" an "almost philosophical sense."

[74] Xenophanes (B. 34) stated roundly "... as for certain truth, no man has seen it, nor will there ever be a man who knows about the gods and about all the things I mention" (trans. Freeman). Cf. Democritus B. 7, 8, and 10 for a similar sentiment.

[75] Democritus' methodological denial of attaining truth reaches its climax in the statement, "We know nothing in reality; for truth lies in an abyss" B. 117 (trans. Freeman).

are the words spoken than the fault appears." [76] Probabilism, and the relativism described by Xenophanes and Heraclitus, are, like truth, forms of a guarantee.[77] While it is probably hyperbolic to see in Protagoras' *homo mensura* argument the direct ancestor to the modern disenchantment of what Dewey called "The Quest for Certainty," there are some grounds for believing that a healthy scepticism lies in back of the modest claim that some method is better than no method.[78] For, it is no method when it is claimed before that method is tried, that all its results are vouchsafed by revelation or fiat.

I am aware that in characterizing explanatory method as I have, certain steps were omitted which have a strong claim to being essential in any such method. Today, logical rigor, semantic precision, and an historical review of the subject are as much parts of our working vocabulary as *physis* or *doxa* were to the presocratics. With some effort, passages could be cited to indicate that the presocratics were not totally ignorant of procedures such as these. But, on the whole, the adoption of these procedures was far from being a widespread practice. Other components of explanatory method could, perhaps, be extracted from presocratic writings; for example, prediction, measurement, controlled experiments, experimental confirmation, quantification of observational data, etc. But, even in their simplest forms, these later procedures require apparatus or equipment of the sort that was not readily obtainable in antiquity.[79] Therefore, I have concentrated my remarks on the

[76] Epicharmus B. 14.

[77] For typical relativistic statements, cf. Heraclitus B. 82 and Xenophanes B. 16.

[78] KAHN, *op. cit.* p. 117 writes approvingly of the presocratics' "severely skeptical common sense."

[79] In addition to the clepsydra (supra n. 32) several types of mechanical devices are referred to in presocratic writings. Pending a fuller statement elsewhere, I note that they fall largely into 2 classes, (i) apparatus designed for further application, e.g., tools, the *gnomon*, the *polos*, etc., and (ii) devices whose construction, apart from any application they may have had, indicates a knowledge of mechanical principles and technological practices, e.g., tooling, etc. In general, cf. PAUL TANNERY, *Pour l'histoire de la science hellène*, 2nd ed. (Paris 1930) p. 85.

conceptual apparatus rather than on the technological apparatus in the belief that there were no physical barriers to prevent their widespread adoption in a variety of explanatory contexts. Why ancient science failed and ancient philosophy prospered is a topic I leave for another occasion.[80] But, in closing, I think that the answer is not to be found by projecting our compartmentalization of disciplines back to the presocratics. That they tried to explain is, I trust, admitted on all hands. To describe how they did it, and not how far they succeeded, has been my aim in this paper.

Rutgers University, Newark, New Jersey, USA

[80] The most balanced account of the various theories advanced for the decline of ancient science is LUDWIG EDELSTEIN, Recent Trends in the Interpretation of Ancient Science, J.H.I. **13** (1952) 573–604 esp. pp. 598–601.

CHAPTER 8

PSEUDO-SCOTUS ON THE SOUNDNESS OF *CONSEQUENTIAE*

BENSON MATES

In a pair of articles published twenty-five years ago [1] Professor Bocheński first drew attention to the remarkable logical acumen of an unknown Scotist who wrote a commentary on Aristotle's *Prior Analytics* [2] and whom, in accord with an established convention, we shall call "Pseudo-Scotus".

Some of the most interesting parts of the commentary by Pseudo-Scotus are found in his discussion of the *quaestio* "whether in every sound *consequentia* the negation of the antecedent follows from the negation of the consequent".[3] In the course of his treatment of this topic he is quite naturally led to a more fundamental question: what, after all, are the necessary and sufficient conditions for the soundness of a *consequentia?* He considers various possibilities and in a subtle analysis disposes of nearly all of them by means of counterexamples. Then, after presenting an illuminating classification of *consequentiae*, he applies his distinctions in answering the principal *quaestio* itself.

Now since the whole of Pseudo-Scotus' analysis has been admirably set forth in an exegetical article by J. Bendiek [4] it does not

[1] De consequentiis scholasticorum earumque origine, Angelicum **15** (1938) 92–109, especially 93–96; Notes historiques sur les propositiones modales, Revue des sciences philosophiques et théologiques **26** (1937) 673–99, especially 687–90.

[2] *Ioannis Duns Scoti Opera Omnia*, ed. Wadding (Paris, L. Vivès 1891–95) volume 2, pp. 81–197. Citations of "Pseudo-Scotus, *op. cit.*" will refer to this edition.

[3] Pseudo-Scotus, *op. cit.*, pp. 103 ff.

[4] Die Lehre von den Konsequenzen bei Pseudo-Scotus, Franziskanische Studien **34** (1952), 205–34.

need to be recounted here. Instead we shall concentrate upon a few points that plainly merit further comment. For the most part these concern the ingenious counterexamples that our author brings against various plausible possibilities for a satisfactory definition of soundness.

The term *consequentia*, as used by Pseudo-Scotus, refers to something that seems to be intermediate between an argument and a conditional. According to the definition he gives, "a *consequentia* is a hypothetical proposition composed of an antecedent and a consequent, joined by a conditional conjunction".[5] Typical examples found in his text are:

Omnis homo est animal; igitur omne animal est homo.

and

Socrates est et Socrates non est; igitur Socrates non est.

From his explanations and practice it is clear, however, that Pseudo-Scotus, like many of the other scholastics and like Aristotle himself, was not much concerned to distinguish between propositions and arguments. But this really makes little difference to his work, for it is equally clear that in effect he is endeavoring to explicate the intuitive concept of *logical consequence*, in the sense in which one sentence or proposition may be said to be a logical consequence of another. It will be seen that the points he makes concerning the soundness of *consequentiae* are immediately applicable to the validity of arguments.

Pseudo-Scotus takes up and rejects each of three possible definitions of soundness as applied to *consequentiae*. Although the last of the three, with its corresponding counterexample, is by far the most interesting, there are reasons for following the author's own order.

I. "For the soundness of a *consequentia* it is necessary and sufficient that it be impossible for the antecedent to be true and the consequent false." [6]

[5] Pseudo-Scotus, *op. cit.*, pp. 104–5.

[6] *Ibid.*, p. 103: "Primus modus est, quod ad bonitatem consequentiae requiritur, et sufficit, quod impossibile est antecedens esse verum, et consequens falsum".

Against this proposal Pseudo-Scotus brings as a counterexample the following *consequentia*:

Every proposition is affirmative; therefore no proposition is negative.[7]

He argues 1) that the given *consequentia* as a whole is sound, 2) that it is possible for the antecedent to be true, and 3) that it is not possible for the consequent to be true. From 1)–3) he concludes that the condition mentioned in *I* is not necessary to the soundness of a *consequentia*. Unfortunately the reason that he gives for 1), namely, that the negation of the antecedent follows from the negation of the consequent, seems question-begging in view of the principal *quaestio* under discussion, but in any case we can see that he considers the *consequentia* intuitively sound. In support of 2) he observes that the antecedent would be true if all negative propositions were destroyed, and to establish 3) he notes that the consequent is itself a negative proposition and thus will be false if it exists at all (so it cannot be true).

While giving due credit to the author for his refreshingly unmystical attitude toward the existence of propositions – despite his association with realist metaphysicians he seems to have had what Russell calls "a healthy sense of reality" – and for his willingness to make a serious application of a self-referential proposition, we must note that his argumentation does not hang together very well. He appears to be employing the modal principle

$$CKPpNPqPKpNq,$$

which is good enough, but if he is going to argue that

Every proposition is affirmative.

would be true if all negative propositions were destroyed, he must grant that under the same condition the proposition

No proposition is negative.

would not exist and hence would not be false. Thus he has not shown that it is possible for the antecedent to be true and the consequent false. At best his counterexample applies not to *I* but to

I'. For the soundness of a *consequentia* it is necessary

[7] *Ibid*., p. 104: "Omnis propositio est affirmativa; igitur nulla propositio est negativa".

and sufficient that it be impossible for the antecedent to be true without the consequent also being true.

Since his point 3), which is crucial for the argument, is directly relevant to *I'* but not to *I*, we may conjecture that the text has been somehow corrupted.[8]

Pseudo-Scotus returns later to an amended version of *I*, but first he tries another approach.

> *II*. "For the soundness of a *consequentia* it is necessary and sufficient that it be impossible for things to be as signified by the antecedent without also being as signified by the consequent".[9]

The counterexample in this case is even more curious:

> No chimaera is a goat-stag; therefore a man is an ass.[10]

and Pseudo-Scotus' comments about it are also in certain respects puzzling. After noting that "for the truth of a negative proposition it is not required that anything be in this or that state, but only that things should not be as signified by its affirmative contradictory", he states that accordingly the antecedent is true. Thus, taking the consequent as obviously false, he pronounces the given *consequentia* unsound. But, he goes on to say, according to the criterion under consideration it would have to be sound, since it is impossible for things to be as signified by the antecedent of this *consequentia* without also being as signified by the consequent. Hence, he concludes, the proposed criterion must be rejected.

Now although the text of Wadding's edition, upon which we have to rely, is not entirely satisfactory here, we can see fairly clearly that the reason why Pseudo-Scotus thinks that the given *consequentia* satisfies the condition of *II* is that he considers it

[8] Bendiek (*op. cit.*, p. 209) considers that the author takes *I* in the sense "For the soundness of a *consequentia* it is necessary and sufficient that it be impossible for the antecedent to be true and that it be impossible for the consequent to be false". To me the text does not seem to require this (logically unsatisfactory) interpretation.

[9] Pseudo-Scotus, *op. cit.*, p. 104: 'Secundus modus dicendi est, quod ad bonitatem consequentiae requiritur, et sufficit, quod impossibile est sic esse, sicut significatur per antecedens, quin sic sit, sicut significatur per consequens".

[10] *Ibid.*: "Nulla chimaera est hircocervus; igitur homo est asinus".

impossible for things to be as signified by the proposition

No chimaera is a goat-stag,

even though at the same time he regards this proposition as *true*.[11] This, as the Kneales say, "is very strange indeed".[12]

But Bendiek has given what must be the explanation.[13] He notes that in discussing an earlier *quaestio* Pseudo-Scotus has explicitly rejected the idea that the significatum of the sentence "God exists" is a *significabile complexe*, such as might be referred to by the clause "That God exists" (in Latin, *Deum esse*); instead it is God himself. Similarly, the significatum corresponding to "Socrates has a shape" is said to be Socrates; that corresponding to "Socrates is not an ass" is again Socrates; and that corresponding to "Man is an animal" is mankind. But, he says, the significatum corresponding to "a chimaera is not a goat-stag" does not exist (and presumably *could* not exist). In general, as concerns the sorts of propositions here considered, the view of Pseudo-Scotus seems to be that the significatum corresponding to an affirmative proposition is the same as the significatum of its subject term, while

[11] Wadding's text is defective here. He has "Nulla chimaera est hircocervus; igitur homo est asinus, quod est falsum; quia antecedens est verum, et consequens falsum; quia impossibile est sic esse, sicut significatur per antecedens, quin ita sit sicut significatur per consequens; et hoc per se sufficit ad bonitatem consequentiae: igitur consequentia dicta fuit bona". The sense requires something like "... quia impossibile est sic esse sicut significatur per antecedens, impossibile est sic esse sicut significatur per antecedens quin ita sit sicut significatur per consequens ...". It is easy to see how such a seeming reduplication could have been edited out. Interestingly, a rare 1520 edition in the University of California library, Berkeley, *Quaestiones utiles subtilissimi doctoris Ioannis Scoti super libros Priorum*, 6 (verso) has a text which, though full of mistakes, is better at this point: "Nulla chimera est hircocervus. Igitur homo est asinus, quod est falsum quia antecedens est verum et consequens falsum; quia impossibile est sic esse sicut significatur per antecedens; igitur impossibile est sic esse sicut significatur per antecedens quin ita sit sicut significatur per consequens; et hoc per se sufficit ad bonitatem consequentiae; igitur consequentia dicta fuit bona".

[12] WILLIAM and MARTHA KNEALE, *The Development of Logic* (Oxford, Clarendon Press 1962) p. 287.

[13] BENDIEK, *op. cit.*, pp. 211 ff.

that corresponding to a negative proposition is the same as that of the contradictory affirmative. Applied to the present case, this yields the conclusion that it is impossible for things to be as signified by "No chimaera is a goat-stag". (Here we assume that the phrase "things are as signified by φ" means approximately the same as "the significatum of φ is as described by φ"). On the other hand, Pseudo-Scotus holds that although the truth of an affirmative proposition requires the existence of its significatum, a negative proposition will be true provided only that its contradictory affirmative is false. Thus our same proposition "No chimaera is a goat-stag" will be true, since "Some chimaera is not a goat-stag" is false. It is a true proposition, and yet there can be no fact or state-of-affairs corresponding to it.

The upshot of all this, I would say, is as follows. Confronted by the suggestion that a *consequentia* is sound if and only if it is impossible for the significatum of the antecedent (i.e., what the antecedent is about) to be as described in the antecedent, without the significatum of the consequent being as described in the consequent, Pseudo-Scotus constructs his counterexample by utilizing a true proposition that neither has nor could have a significatum. Perhaps a mathematical example, such as

The greatest integer is not even; therefore 2 is not even.

would have been clearer. Applied to this example Pseudo-Scotus' argument would be that the antecedent is true (since "the greatest integer is even" is false), and yet that it is not possible for the significatum of "the greatest integer" to be as there described (because it is not possible for such a significatum to exist at all). In effect, what is here involved is the Theory of Descriptions, including the problem of scope. The question with which Pseudo-Scotus is occupied, namely, whether there can be a true proposition with a subject term that does not – or even cannot – denote anything, was not really clarified until the time of Frege and Russell.

The next definition considered is the result of amending *I* so as to get around the objection there raised.

III. "For the soundness of a *consequentia* it is necessary and sufficient that it be impossible for the antecedent and consequent, when formulated at the same time, to be respectively true and false[14]".

Against this definition our author proposes to give an example of a *consequentia* that has a necessary antecedent and a necessary consequent and yet is not sound. Such a *consequentia* will satisfy the condition of *III*, he points out, since "when the antecedent and the consequent are both necessary, it is impossible for the antecedent and consequent, formulated at the same time, to be respectively true and false". Thus the condition of *III* will be shown not sufficient for soundness. Pseudo-Scotus then comes up with the following *consequentia*:

God exists; therefore this *consequentia* is not sound.[15]

Of this he says, "The *consequentia* is surely unsound, for it cannot be sound since in that case there would be a sound *consequentia* with a true antecedent and a false consequent. And that the antecedent is necessary, is acknowledged. But that the consequent is necessary I am (now) establishing, since it is impossible for the said *consequentia* to be sound."

Thus we have what appears to be a clear counterexample to one of the most common ways of explaining informally the validity of arguments. Our author has concocted an argument which is invalid and which nevertheless satisfies the condition that it is impossible for its premises to be true and its conclusion false. Note that any other necessary proposition could have served instead of "God exists", but note also that necessity, and not just truth, is essential.

Pseudo-Scotus regards his argument as showing that *I* and *III* are defective. If, however, we accept *I* or *III* as logically true, his argument becomes an antinomy. For let "*A*" denote the following *consequentia*:

God exists; therefore *A* is not sound.

Then *A* both is and is not sound. *A* is not sound, since if it were sound it would have a true antecedent and a false consequent,

[14] Pseudo-Scotus, *op. cit.*, p. 104: "Tertius modus est, quod ad bonitatem consequentiae requiritur, et sufficit, quod impossibile est antecedente, et consequente simul formatis, antecedens esse verum, et consequens falsum."

[15] *Ibid.*: "Deus est; igitur ista consequentia non valet".

which is impossible in a sound *consequentia*. But on the other hand A is sound, for, since "God exists" is counted as a necessary truth, we have just shown that the consequent of A is a necessary truth, and by *I* or *III* this suffices for the soundness of A.

One can consider a necessary truth as a sound *consequentia* with no antecedent (i.e., as the conclusion of a valid argument from the empty set of premises). From this point of view, the following is an analogue to Pseudo-Scotus' antinomy. Let "A" denote the sentence

A is not a necessary truth.

Then A both is and is not a necessary truth. It is not, for if it were, it would be true and hence would not be a necessary truth. But we have just proved A, and so A is a necessary truth after all.

Two close relatives of the foregoing antinomy immediately spring to mind. One, of course, is the patriarch of all antinomies, The Liar (Rüstow's history of which ought to be read by every logician).[16] The other is the undecidable sentence of K. Gödel's famous incompleteness proof.[17] As will be recalled, in the course of his proof Gödel constructs a sentence that asserts its own unprovability. With certain important qualifications that are irrelevant to the present discussion, an essential portion of his argument may be set forth as follows.[18] Let "A" denote the sentence

A is not provable.

Now suppose that A is provable. Then, assuming that whatever is provable is true, A is true. But if A is true, it is not provable. Hence A is not provable. On the other hand, suppose that the negation of A is provable. Then the negation of A is true. But in that case A is provable and true, so that both A and its negation are true, which is impossible. Therefore, neither A nor its negation is provable (although, as noted earlier, A is true).

If we regard an argument as an ordered pair, the first term of which is a set of sentences (the premises) and the second term of

[16] RÜSTOW, A., *Der Lügner* (Leipzig, Teubner 1910).

[17] GÖDEL, K., Ueber formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme I, Monatshefte für Mathematik und Physik **38** (1931) 173–198.

[18] Cp. *op. cit.*, pp. 175–6.

which is a single sentence (the conclusion), we get a somewhat more general form of Pseudo-Scotus' antinomy by letting Γ be any set of necessary truths and letting "A" denote the argument with premises Γ and conclusion "A is not valid". Then A will be an invalid argument with a necessary truth as conclusion.

Similarly, if Γ is a set of true sentences and "A" denotes the argument with premises Γ and conclusion "the conclusion of A is not derivable (in a system in which derivation preserves truth) from the premises of A", then we find that the conclusion of A is a true sentence not derivable from the set of true sentences Γ. Thus, for any set of true sentences there is a true sentence that is not derivable from it; in particular, if Γ is the set of *all* true sentences, it follows that A is not derivable from Γ although A belongs to Γ, which on any plausible notion of derivability is an antinomy.

When we consider the relation of logical consequence instead of derivability, and again let Γ be the set of all true sentences, the foregoing form of Pseudo-Scotus' antinomy reduces to The Liar. For if "A" denotes the argument with premises Γ and conclusion "the conclusion of A is not a logical consequence of the premises of A", it is clear that the conclusion of A asserts its own falsehood and thus is both true and false.

There may be some interest in the fact that similar lines of thought lead to paradoxes for other modalities. As an example, let "A" denote now the sentence

> Nobody knows that A is true.

Suppose that A is not true. Then somebody knows that A is true, and hence A is true after all. But now we have proved the truth of A, even though, oddly enough, we have also shown that nobody knows that A is true.

Or, if the reader feels no dismay at the above established limitation on his knowledge, he may wish to consider the following. This time let "A" denote the sentence

> Either the reader has no slightest doubt that A is true, or A is false.

Suppose that A is false. Then A is true. Therefore, A is indeed true. But further, since A is a true disjunction and its right member is false, the left member must be true; hence, however he may protest to the contrary, the reader has no slightest doubt that A is true.

In a collection honoring a modern scholastic who has written a nice essay on the advantages of *Spitzfindigkeit* [19] it is probably unnecessary to apologize for spending some time in the consideration of paradoxes and antinomies. From the earliest times puzzles of this sort have appealed to some minds and repelled others, and of course there has never been a shortage of critics to complain about "triviality", "quibbling", "superficiality", and so on. But although the scoffers have always been numerous and confident, it is worth noting that they have been remarkably unable to agree with one another upon the solutions which they individually found so obvious. This by itself suggests that there may be a few more things to learn from The Liar and its associates before they are shelved "as of historical interest only". Especially in logic, as Russell has observed, one cannot afford to overlook the antinomies:

> A logical theory may be tested by its capacity for dealing with puzzles, and it is a wholesome plan, in thinking about logic, to stock the mind with as many puzzles as possible, since these serve much the same purpose as is served by experiments in physical science. [20]

And to this may be added the fact that, as Gödel has amply shown, some of the deepest results are obtainable by arguments that closely skirt antinomies. Whether the contribution of Pseudo-Scotus will give anyone similarly good ideas, remains to be seen.[21]

University of California, Berkeley, Calif., USA

[19] BOCHEŃSKI, I. M., Spitzfindigkeit, in: *Festgabe an die Schweizerkatholiken* (Freiburg, Schweiz, Universitätsverlag 1954) pp. 334–356.

[20] RUSSELL, B., On Denoting, Mind **14** (1905) pp. 484–5.

[21] Research for this paper was aided by the National Science Foundation.

CHAPTER 9

THE LATER HISTORY OF THE *PONS ASINORUM*

IVO THOMAS O.P.

Some essential stages in the history of the syllogistic diagram known as the *pons asinorum* or *umbelicus* have been enumerated by Bocheński.[1] Those data should be supplemented by notes of L. Minio-Paluello [2] in which he remarks for instance that Alexander of Aphrodisias refers to a diagram at the relevant place in his commentary on the *Prior Analytics*, that the figure is given in some Greek and at least a hundred Latin MSS, and that mnemonic verses had already been devised to accompany it in the 13th century. A magnificent printed version, illustrated with the crescent and triangular diagrams which were also of Greek origin, occurs in the rare Latin Averroes of 1489.[3] As was frequently the case, it is not free from misprints and errors. A treatise was devoted to all these diagrams by Joannes Albanus: *Apparatus syllogistici ad Aristotelis mentem synopsis*, Bononia, 1620. Throughout this tradition the Latin letters A through H were used to parallel the Greek letters Alpha through Theta, and the exemplifying terms inherited from Philoponus remained unchanged. After such detailed commentaries as those of Thomas Bricot, John Dorp and others [4] it might be thought that there was little room for further development of the

[1] *Formale Logik* (1956).
A History of Formal Logic (1961).

[2] Aristoteles Latinus III. 1–4, Analytica Priora, pp. 384–6. A Latin Commentary (? translated by Boethius) on the *Prior Analytics, and its Greek Sources*. Journal of Hellenic Studies **72** (part 1), pp. 97, n. 7.

[3] *Omnia Aris. opera cum commento Averrois*; Bernardinus de Tridino de Monteferato (1489).

[4] Cf. K. PRANTL, *Geschichte der Logik*, vol. IV.

theme. However, three writers from the 16th century deserve some notice.

I

Ratio inveniendi medium terminum in syllogismo categorico, ab Aristotele tradita, et per Christophorum Cornerum ex Fagis diligenter explicata. Una cum tractatu ipso περὶ εὐπορίας προτάσεων, ex Aristotele, ad finem adiecto. Addita etiam est praefatio, de usu huius doctrinae. Basel, 1549; 9.5 × 16 cm.

The contents are: p. 1 title, 2–12 dedication, 13–28 preface, 29–133 treatise, 134–142 text of the so-called τμῆμα δεύτερον from *An. Pr.* The preface, part of which we quote, shows that the *pons* was by no means as well known as one might suppose, and also evidences a concern with educational theory such as occupied John Sturm and Peter Ramus about this same time. Corner stresses the need to make things easy for *pueri educandi*, and considers that he is the first to treat this particular matter from that point of view. He himself, a pupil of Melancthon, had come on it only after he had thought his knowledge of logic was complete and was about to proceed to other subjects.

> Nec est alia causa cur hactenus haec doctrina sit omissa, quam quod sine praeceptore difficulter possit intelligi: et quod, qui priori aetate dialecticen Aristotelis enarrarunt, ut caetera omnia, ita neque hanc partem non modo non illustrarint, sed etiam exemplorum obscuritate et praecipiendi difficultate totam e manibus discentium excusserint.

His method is to translate the text of Aristotle and comment on it, section by section, before giving the entire Greek text, and he thinks this whole proceeding sufficiently novel to need detailed justification.

> Non dubitat et ipse Melanthon, illis qui perfecte cupiant hanc artem cognoscere, et veros fontes eius intueri, et quasi exhaurire, esse author, ut postquam haec elementa perdidicerint, legant quoque Aristotelem ipsum. ...Multa ... sunt quae non cum rivulis his, quos pueri degustant, profluunt,

> sed in vivo fonte necessario conservantur. Etsi non sunt pauci, qui credant ignoratione et desidia falsi, nihil posse ostendi in Aristotele, quod non in vulgaribus libellis abunde satis explicatum sit. His ego non inscitiam obijcio, sed iubeo tantum vel a primo limine salutare Organum Aristotelicum: quod ubi fecerint, dabunt mihi cute manus, et se victos fatebuntur. Ut enim de me ipso dican, cum adolescens primum, nihil admonitus a praeceptoribus, incidissem in hoc caput, ... cogitavi ita mecum, Quo nunc quaeso, in quos labyrinthos et monstrosas figuras vocor? putabam equidem me totam artem probe cognovisse, et constitueram ad graviora studia progressus facere: nunc vero cum huc verto oculos, et in haec scripta inspicio, intelligo quam procul adhuc absim ab artis huius perfectione. ... Videri possum recessisse ab Aristotelis inscriptione, at revera mea non est alia quam Aristotelis inscriptio: sed est, ut mihi quidem videtur, apertior, et adolescentibus magis nota atque grata.

Finally he confesses that if circumstances had allowed, he would have preferred to comment directly on the Greek text, but has to be content with appending it, a proceeding which by itself renders the book a monument of scholarship. The diagram of the *pons* is given twice, on p. 94 in Latin, and p. 138 in Greek.

II

Paralipomena dialectices. Libellus lectu dignissimus, et ad dialecticam demonstrationem certius cognoscendam, cuius etiam in praefatione prima quaedam principia proponuntur, apprime utilis. Basel, 1558; 8.5 × 14 cm.

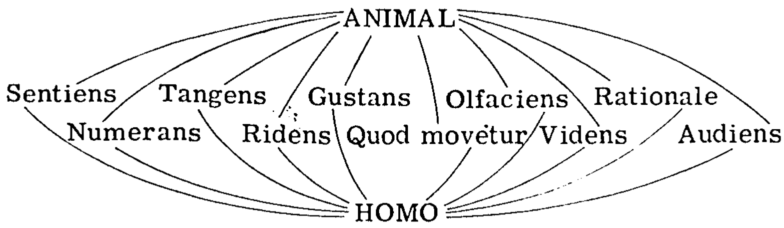
The contents are: pp. 3–11 epistula nuncupatoria, 15–28 praefatio, 29–78 liber primus, de inveniendo medio, 79–183 liber secundus, de ordine, (184–192) index. The epistle is signed by Mathias Flacius Illyricus, who says that the book was written "by someone", presumably himself, in the long and dangerous siege of Magdeburg (1550). (His son, Mathias Flacius Junior, wrote an *Opus logicum* in Organon Aristotelis . . . constans libris xiiii, Frankfurt, 1593.) In the preface he remarks that no-one has yet written a satisfactory treatise on Order, one of the two principal parts of dialectic, the other being *de instrumentis*. This last has been satisfactorily covered by many, with the exception of the *ratio inveniendi medii*, generally neglected and little known.

The emphasis on order is consonant with that of so many of Flacius's contemporaries on method, and the spatial imagery noted by Ong [5] in connexion with this post-Agricolan school of writers is very evident in the first chapter, where Flacius distinguishes between the *inventio medii*, *loci*, and *formae argumentorum*.

> posset haec fortasse ei similitudo (quanquam iusto crassior sit) recte proponi, quod topica seu argumentorum loci sint veluti vascula quaedam, in quibus metalla resoluta contineantur. Formae argumentorum sint veluti typi et formae quaedam, in quas formanda materia infundatur. Doctrina vero de inventione medii sit veluti instrumentum quoddam, quo hauriamus ex locis tantum materiae et eam materiam, quae instituto operi apta est, eamque in suos typos industrie infundamus, atque ita destinatum opus efficiamus. Hec similitudo satis apte rem declarare videtur.

The kind of instruction given for finding particular kinds of syllogistic argument can be exemplified from the third chapter, *de inquisitione medii, in probanda universali affirmativa*.

> Cum habes propositionem disputandam, quae sit universaliter et affirmative ponenda, pone in tabula summo loco eius maiorem terminum seu praedicatum, minimum in infimo. In medium congere voces, quae praedicta ratione supremo, sed per subiectionem, et infimo termino per praedicationem coniungi possunt, ut typus ostendit . . .



> Iam accipe maiorem, Omne sentiens est animal. Et minorem, Omnis homo est sentiens. Ergo &c. Vel sume aliud quocunque medium: ut, omne ratiocinans, omne videns, omne loquens &c. Observandum autem in delectu medii, diligenter Aristotelis praeceptum est: qui iubet, ut praecedentes sint notiores et firmiores conclusione, seu ut notius sit medium terminum maiori et minori.

The early part of the book is distinguished by illustrative diagrams such as that given above. The *pons* itself comes on p. 52 and is commented on thus:

> Typum praecedentem, veteres philosophi umbelicum appellarunt: recentiores pontem asini. Illi quod iudicabant esse mirabile quoddam invemtum, quo positis in parva tabella cuiusvis sententiae terminorum circumstantibus vocibus, facile iudicari posset, quid de ea, et quomodo vel posset probari, vel non posset. Adhaec et mirificam quandam perfacilemque rationem contineret, protinus inveniendi de quavis re: cuius modo sis peritus, argumenta, eaque non ex trivio sumpta, sed ex ipsius rei natura plerunque petita.
>
> Sophistae porro novi, ideo Pontem asini nominarunt, quod etiam imperitioribus, minusque in argumentando expeditis facilem ostendat rationem, de quavis propositione affirmativa, negativa, particulari, et universali argumenta inveniendi.
>
> Non dubito tamen, dubitaturos primo aspectu rudiores, qui nam sibi velit totus typus, vel singulae rotulae, earum connexiones et litterae,

and an explanation follows. The ninth chapter repeats the whole doctrine in the form of a catechism, with the pupil asking the questions, e.g., p. 71:

> Nihilne habes amplius?
>
> ¶ Nihil. Nisi cognoscera velis ea quae hac de re certis quibusdam vocabulis ad hunc usum confictis, tradiderunt scholastici, seu Sophistae recentiores.
>
> Quae sunt illa vocabula?

[5] WALTER J. ONG, S. J., *Ramus, Method and the Decay of Dialectic* (Harvard U.P. 1958) Cf. esp. pp. 116 ff.

> ¶ Duobus his versiculis, comprehensam totam rationem inquirendi Medii indicant. Fecana, Cageti, Daphenes (*sic*) Hebare, Gedaco, Gebali stant: sed non stant Febas, Hedas et Hecas.
>
> Quae tu mihi monstra refers?
>
> ¶ Voces sunt durae quidem et inauditae Latinis hominibus: sed . . . valde ad confirmandam huius doctrinae memoriam utiles.

On p. 78 another *pons* is given, partly in Greek, but with the mnemonic words written on the lines.

III

L'ORGANE c'est à dire L'Instrument du Discours Divisé en deux parties, sçavoir est, L'Analytique, pour discourir veritablement, et La Dialectique, pour discourir probablement. Le Tout puisé de l'Organe d'Aristote, et dedié au Roy Treschrestien, par M. Philippes Canaye, Sieur de Fresnes, Conseiller de sa Majesté en Soy Grand Conseil. MDLXXXIX par Jean de Tournes Imprimeur du Roy. 16.5 × 34 cm.

Pp. 135–270 are devoted to covering the ground of the *Prior Analytics*. The whole book deserves some special study as being outside the usual categories of logical writing, though not uninfluenced by contemporary trends. It is an elegantly written and very discursive exposition rather than a commentary, marked by enormous enthusiasm for its subject with strong emphasis on what the author considers to be its practical nature. The preface opens:

> M'Estant proposé de declarer l'usage de l'Organe d'Aristote, non point pour entreprendre sa defense contre ceux qui se sont vainement efforcés de la reprendre et de le supprimer, que pour proffiter a ceux qui désirent d'apprendre le divin art du discours . . . . cest admirable instrument lequel iusques icy a esté si mal manié par la barbarie pedantesque, que la plus part des beaux esprits de nostre temps l'ont estimé du tout inutile et de nul usage, combien que ce soit l'œuvre le plus

> necessaire et le plus accompli de tous les liures humain est, qu'il ne puisse estre aboli, sans la perte et ruine de toutes les sciences. .... Et neantmoins, d'autant que l'Organe est fort incõgnu en France, et principalement en la langue françoise ... toute la beauté de l'Organe gist en la prattique, et que plus un instrument est portatif, plus il est estimé, il l'ayt voulu reduire au plus petit volume qu'il a peut.

We have already seen Flacius juxtaposing a treatise on the fashionable theme of Method under the name of Order and one on the purely Aristotelian *pons*. When Canaye reaches the latter subject (p. 167) he unites these two matters. For him the *inventio medii is* a main part of Aristotelian method, the other being contained in the Topics. Some of his headings to this section show this clearly:

Louange de l'Invention Analytique. De la Methode tant Analytique que Topique. Pourquoy la Methode du discours consiste plus en l'Invention qu'en l'Artifice du Syllogisme. ... Pourquoy l'Invention qu'il enseigne icy ne peut estre commune à l'Analytique & aux Topiques. Division de la Methode Analytique.

A brief extract from the first of the fore-going sections gives a good sample of the hyperbolic language he uses about the *pons*:

> Le secret qu'Aristote enseigne icy, enrichit l'ame, & non pas la bourse: car à iceluy est attaché la clef de toutes sciences, & quiconque l'aura trouvé ne craindra point le larron ny le soldat. Il ne craindra point de le prattiquer de peur d'estre enfermé en quelque tour, & en enrichir autruy. Il ne luy faudra ny fourneau ny poudre ny racine pour faire ses operations.[6] Il travaillera de iour & de nuict, par les champs, & à la maison, & plus il travaillera, plus il prendra de plaisir en son travail: lequel rapportãt à son droit usage, il acquerra ce thresor tant estimé par le plus riche Roy qui fut onques, a sçavoir la sapience, au prix de laquelle toute autre chose n'est que vanité.

When it comes to giving the *pons* itself, Canaye makes some unique innovations. He prints two figures, one to show the conclusive combinations, the other the useless ones; he changes the traditional lettering, and introduces curved lines.

[6] The reference is to a long alchemical simile that precedes this passage.

.... et pource que la figure, qui a esté mise cy deuant, pourroit estre difficile à cause de ses entrelassements, en voycy une autre qui sera plus facile à mon iugement.

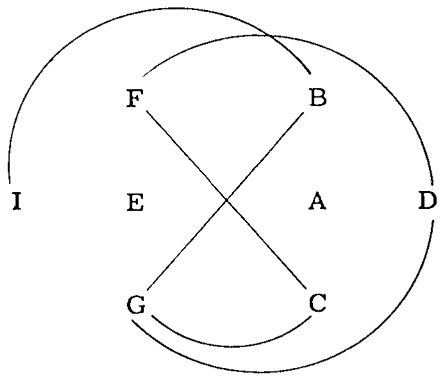


Figure de l'Invention Analytique.

A Attribué.
B Consequent de l'Attribué.
C Antecedent de l'Attribué.
D Repugnant de l'Attribué.
E Subiect, duquel il faut demonstrer l'Attribué.
F Consequent du Subiect.
G Antecedent du Subiect.
I Repugnant du Subiect.

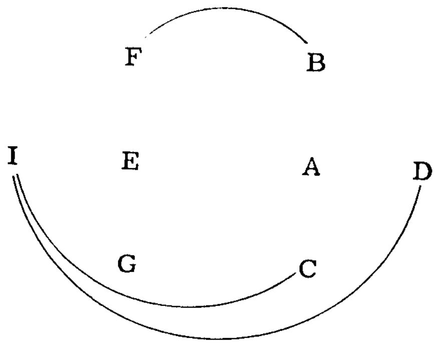


Figure des compositions inutiles des principes.

The essential doctrine is then tabulated in verbal form, and an elaborate example of the whole figure given in religious terms, on which he comments:

> Cest exemple suffira pour monstrer comme il faut faire, et comme il faut avant toutes choses chercher les consequents, antecedents, et repugnans, qui est un étude le plus utile et qui apporte autant de contentement à un gentil esprit, que nul autre.

After this wild enthusiasm of the greatest devotee the *pons* ever had, it seems to have gradually passed out of fashion, though Robert Sanderson's *Logicae Artis Compendium* (various editions from 1615 to 1841) preserved its memory at Oxford. The second edition (1618) contains a number of misprints which must have made the figure more than usually puzzling to the student.

University of Notre Dame, Notre Dame, Indiana, U.S.A

CHAPTER 10

REIFICATION, QUOTATION AND NOMINALIZATION

J. F. STAAL

> Do we know, then, that there will prove to be any ultimate boundary between "logical grammar" and a revised and enlarged *Grammar*? In the history of human enquiry, philosophy has the place of the initial central sun, seminal and tumultuous: from time to time it throws off some portion of itself to take station as a science, a planet, cool and well regulated, progressing steadily towards a distant final state. This happened long ago at the birth of mathematics, and again at the birth of physics: only in the last century we have witnessed the same process once again, slow and at the time almost imperceptible, in the birth of the science of mathematical logic, through the joint labours of philosophers and mathematicians. Is it not possible that the next century may see the birth, through the joint labours of philosophers, grammarians, and numerous other students of language, of a true and comprehensive *science of language*? Then we shall have rid ourselves of one more part of philosophy (there will still be plenty left) in the only way we can ever get rid of philosophy, by kicking it upstairs.[1]

1. Introductory

The three concepts mentioned in the title of this paper are often considered to belong to ontology, semantics and linguistics respectively.[2] A study concerned with the interrelations between

[1] J. L. Austin, *Philosophical Papers* (Oxford 1961) p. 180.

[2] I am grateful to N. Chomsky, J. C. Heesterman, A. Kraak, R. B. Lees, P. A. M. Seuren and especially to the late E. W. Beth for valuable suggestions.

these concepts cannot be easily classified under any of these fields of research. This paper attempts to study these interrelationships and to unearthen the roots of some philosophic perplexities by discussing cases mostly taken from Indian logic and Sanskrit grammar. Since logic in some respects can be thought of as a bridge between philosophy and linguistics, it will be appropriate to consider what follows a study in logic. In so far as it contributes to the understanding of Indian logic, this paper will not be out of place in a volume in honour of Professor Bocheński, whose *Formale Logik* is the first comprehensive history of formal logic which takes into account Indian material. However, I shall not confine myself to Indian logic but shall follow a suggestion implicit in Bocheński's remark on the importance of its study: "denn sie – und sie allein – bietet dem Historiker eine Möglichkeit von höchster Bedeutung, nämlich die des Vergleichs".[3] In this respect this paper may also be considered a comparative study.

The picture of some structures of Indian logic which will emerge, will look quite different from the characteristics given by Bocheński. This is neither due to differences of interpretation, nor to different selections from the source material. Undoubtedly the latter is unavoidable to some extent when one tries to refer in a few pages to a vast and heterogeneous field. Moreover the term Indian logic is taken as including not only Indian systems of logic, but also logical doctrines propounded by Indian scholars in other fields, in particular the Indian grammarians, who often use more logic than professional logicians.[4] The principal difference rather results from a particular point of view concerning the position of logic. On the one hand logical techniques and formalizations are often motivated by considerations which are best described as philosophic in nature. On the other hand logical expressions are closely related to the structure of languages in which they occur. Here the latter point of view will be emphasized.

I am especially indebted to Peter Hartmann who has placed in an additional note (see below, Annex), some of the facts studied here in a wider linguistic perspective.

[3] I. M. Bocheński, *Formale Logik* (Freiburg/München 1961) p. 180.

[4] This perspective is adopted in an article "Indian logic", *Encyclopedia of Philosophy* (forthcoming).

It does not only apply to natural languages in which logical structures may be initially embodied, but also to artificial and formal languages which developed in India as well as in the West. This point of view will result in particular comments on the relations between natural and artificial languages.

Sanskrit, the natural language in which Indian logic developed, may at first appear abstruse and exotic, but reveals on closer inspection Indo-European structures which also belong (in varying degrees) to languages in which Western logic developed. In both cultures logicians may feel that their constructions and discoveries are merely expressed by means of language, but have remained independent of this instrument. In how far can this feeling be said to be appropriate? The minimum hypothesis we can initially adopt is that a formal system has at least some structures in common with some natural languages, while the meta-language in which the formal system is presented or studied is in general much closer to a natural language.

In the first place the following will therefore differ from Bocheński's "*indische Gestalt der Logik*" as more attention will be paid to structures of the language in which logical expressions are formulated. In particular, the doctrines described by Bocheński as dealing with "*mehrstufige Abstrakta*"[5] will be studied here. In the second place observations on the relations between natural and artificial languages in India will be shown to apply in part to Western logic as well. In this respect this paper attempts to throw some light not only on Indian logical structures, but also on relations between logical structures and linguistic expressions in general.

The paper consists of three parts. The first deals primarily with reification, but also treats quotation and nominalization; the second deals primarily with quotation, but is also concerned with reification and nominalization; and the third deals primarily with nominalization, and to a far less extent with the remaining two.

2. Reification

At the end of this section, the philosophical problem of reification will be disposed of rather quickly, not because it is so transparent,

[5] *Op. cit.* p. 517.

but because the Indian and Western predicaments appear largely similar. The battle between realism and nominalism is Indian as well as Western. The general background of the issue may be stated as follows. Some parts of sentences in natural languages are thought of as referring to things. For example, the referent of the fourth word of the sentence "he smoked his pipe with relish" is a thing. Here three observations may be made. First it is not immediately obvious than any other word of this sentence refers to a thing or refers in the manner in which "pipe" refers to anything at all. Secondly not only words, but also smaller or larger units may refer to things, e.g., the italicized portion of the sentences: "he *paint*ed his room pink" and "I don't like *this particular one*"[6]. Lastly, if words refer to things, these words need not necessarily be nouns.[7] The first word of a particular statement [8] "this is what you were looking for" may refer to a thing as well.

We may speak of reification in general if parts of sentences which do not obviously refer to things are considered as referring to things. For example, if "with" in the first sentence mentioned above is said to refer to a with, we may speak of the reification of "with". However, this assumption appears improbable and the example seems far-fetched. Reification takes place more readily when it is applied to parts of sentences which often refer to things. Since many nouns and pronouns refer to things it is not always clear whether a particular noun or pronoun does. For example, in "she was struck

[6] Cf. also the examples quoted in: E. SAPIR, *Language* (New York 1949) p. 30; B. L. WHORF, *Language, Thought and Reality* (New York 1958) pp. 210, 243.

[7] The fallacy has been frequently exposed by WITTGENSTEIN, e.g.: "We are up against one of the great sources of philosophical bewilderment: a substantive makes us look for a thing that corresponds to it" (*The Blue and Brown Books* (Oxford 1958) p. 1).

[8] The first word of each sentence of the form "this is what you were looking for" need not refer to a thing; but we can easily find a *statement* of the same form for which this holds. The distinction between *statement* and *sentence* occurs in Austin, *op. cit.* pp. 87–88. From a very similar distinction Ziff derives (P. ZIFF, *Semantic Analysis* (Ithaca 1960) p. 115) the distinction between *meaning* and *referent*. His demonstration can be simplified as follows: the meaning of *this* is always the same, but not its referent; the meanings of *this* and *that* are always different, but not their referents.

by his expression" the last word can be easily thought of as referring to a thing in the same way as the last word of "she was struck by his stick". Similarly, the first word of "this is what you were thinking of" can be easily considered to refer to a thing in the same way as the first word of "this is what you were looking for". In both cases confusion about the meaning of an expression or object of thought may occur at this stage.

A particular class of nouns has for its meaning (not necessarily for its referent) universals, and a particularly important kind of reification is the reification of universals.[9] Linguistically speaking, universals are often expressed by nouns which end in abstract forming suffixes such as Greek -*της*, Latin -*tas*, French -*té*, German -*heit*, English -*ness*, -*ty*, -*ion*, etc.

In all these respects ordinary Sanskrit is basically similar, but philosophic Sanskrit and especially the technical Sanskrit used in Indian logic goes much further. Sanskrit nouns are easily formally recognized because of their declension and terminations. On account of distinctive nominal declensions, nouns are to a far greater extent similar to each other and dissimilar e.g., to verbs than in English, where a man not only blows bubbles but also comes to blows. Two important abstract forming suffixes in Sanskrit are -*tva* and -*tā* (cf. -*της*, -*tas*, -*té*, -*ty*). These are used with greater ease in Sanskrit than their cognates are in other Indo-European languages. Already in the earlier stages of the language it was common usage to speak of *aśvānām aśvatvam* "the horseness of horses" [10] and the like. These suffixes can be liberally attached to various forms. They also occur combined (e.g., -*tvatā*), not only in the technical language of Indian logic but also in the oldest religious hymns of the Vedas.[11]

A widespread logical expression for a subject-predicate sentence could be described as semi-artificial. In ordinary "natural" Sanskrit,

[9] Cf. W. V. O. QUINE, *From a Logical Point of View* (Cambridge Mass. 1961) pp. 102–129.

[10] L. RENOU, *Histoire de la langue sanskrite* (Lyon/Paris 1956) p. 44.

[11] D. H. H. INGALLS, *Materials for the Study of Navya-nyāya Logic* (Cambridge Mass. 1951) p. 45; L. RENOU, *Etudes védiques et Pāṇinéennes*, I (Paris 1955) p. 54.

"the pot is blue" is expressed by *ghaṭo nīlaḥ* [12] (as isolated forms, *ghaṭaḥ* is "pot" and *nīlaḥ* "blue"). Logicians, however, prefer to say *ghaṭasya nīlatvam*. Here *-tvam* is attached to the stem *nīla-*, while *ghaṭaḥ* is replaced by its genitive *ghaṭasya*. The result may be somewhat more litterally translated by either "there is blueness of the pot" or by "blueness of the pot". In the first interpretation the expression continues to function as a sentence, in the second it is replaced by a nominalized sentence part. The latter can become part of a sentence, but in many such cases it is customary to go a step further and form one nominal compound, *ghaṭanīlatvam* litterally "pot-blueness" or, rarely, "there is pot's-blueness". This can be incorporated in a sentence in the same way nouns are incorporated in sentences. It is declined like other nouns ending in *-am*. Logicians express "since the pot is blue, the flowers look beautiful" by an expression which may be litterally rendered by something like "because of pot-blueness the flowers look beautiful".

This development:

ghaṭo nīlaḥ	(1)
ghaṭasya nīlatvam	(2)
ghaṭanīlatvam	(3)

may be compared to Western expressions for the subject-predicate type of sentence. In the predicate calculus any of the expressions (1)-(3) can be represented by "$F(x)$". However, if predicates are treated as propositions in the propositional calculus, i.e. occur in expressions such as "$F(x) \supset G(x)$", Indian logicians are inclined to use (2) and especially (3) rather than (1). Predicates are thereby incorporated in other sentences. Any of the expressions (1)–(3) can be incorporated in another sentence if (1)–(3) are considered expressions of an object-language which are quoted in a metalanguage. In a Western terminology, a metalinguistic expression corresponding to the expression of the object-language "$F(x) \supset G(x)$", is:

"'$F(x)$' entails '$G(x)$'".

[12] The nominative case ending *-aḥ* of *ghaṭaḥ* is replaced by *-o* when *n-* follows.

In an Indian terminology, quotation can be effectuated by relatively similar means, as will be shown in the next section. Linguistically speaking, an expression, if quoted, assumes the status of a noun. It can be introduced into a sentence in the place of a noun. Since the expressions (2) and (3) can be considered nouns already, the need to quote them explicitly is hardly felt.

In Sanskrit it is possible to quote any of (1) – (3), for example in: "'the pot is blue' entails 'the flowers are beautiful'".

Without quotation, (2) and (3) can be directly incorporated in another sentence as follows. Since one of the functions of the Sanskrit ablative case is to express a reason, and since the ablative case termination of words ending in *-am* is *-āt*, the sentence part "since the pot is blue" can be expressed by *ghaṭanīlatvāt*, litterally "because of pot-blueness". Expressions ending in *-tvam* play a similar role in Indian logic as expressions of the form "$F(x)$" in the Western predicate calculus. Similarly, declined forms of expressions ending in *-tvam*, in particular forms ending in *-tvāt*, function in a similar way as occurrences of "$F(x)$" in larger expressions, e.g., "$F(x) \supset G(x)$". In Sanskrit reasons are often given at the end of sentences: a stock example in logic is *parvato vahnimān dhūmavattvāt* "the mountain possesses fire because it possesses smoke". The philosopher Sureśvara (ca. 800 A.D.) characterized this kind of artificial Sanskrit, when he described scholars as "trying to catch each other in webs of words, culminating in-*tvāt*"[13]. At paṇḍits' debates in India, arguments continue to be driven home in this resounding manner.

From a grammatical point of view the abstraction expressed by attaching *-tvam* to various linguistic forms, is nominalization. Since we can talk about things by expressing them as nouns or nominal forms ("about" is also followed by nouns or nominal forms[14]), nominalization increases the descriptive power of language. As

[13] *Naiṣkarmyasiddhi*, ed. M. Hiriyanna (Poona 1925) p. 80, quoted in: P. HACKER, *Untersuchungen über Texte des frühen Advaitavāda*, I (Mainz 1951) p. 1923.

[14] The literature about *about* is mentioned in the first note of: N.GOODMAN, About, Mind **70** (1961) 1–24. The last but one section of this paper is called *Nominalization*, a term which is not used in the text and which should probably be read either as *nominalism* or as *a transition to nominalism*.

we shall see later, quotation can be said to provide within a limited context a more general method for introducing topics into our discourse. The use of nominalization in natural languages too can be further extended, unified and formalized. It may lead to artificial expressions such as "$F(x)$". The mere occurrence of such expressions in formal systems may appear rather elementary from a logical point of view. The interesting feature is that these expressions and their occurrences are based upon a process which in natural languages is expressed by nominalization.

In "$F(x)$" reification is applied to the individual x. Similarly in (3) the element *ghaṭa-*, though hardly describable as a word, is thought of as referring to a thing. In general, if "x" in a given language is used as a variable, reification applies to the values "x" may assume within the language. By this generalization "$F(x)$" becomes an open sentence which can be closed by prefixing quantifiers and the like. As Quine remarked, in expressions such as "$(x)F(x)$", "x" is rather like a pronoun.[15] The variable "x" can refer to various constants just as a pronoun can stand for (refer to) various nouns. If the variable "x" ranges over the set of individuals $\{x_1, \ldots, x_n, \ldots\}$, the quantified expression "$(x)F(x)$" will accordingly express "$F(x_1) \wedge \ldots \wedge F(x_n) \wedge \ldots$", while "$(Ex)F(x)$" will express "$F(x_1) \vee \ldots \vee F(x_n) \vee \ldots$".

Speaking about quantified artificial languages Quine has proposed the following criterion for ontological (later called: ontic) commitment: *entities of a given sort are assumed by a theory if and only if some of them must be counted among the values of the variables in order that the statements affirmed in the theory be true*".[16] Bocheński criticized this view and said that, for properties, "to be is not to be a value of a variable but *to qualify the value of a variable*".[17] This requires "to be" to have different meanings in its application to things and properties. Once we write "$(Ex)F(x)$", says Bocheński, "we turn the property into a thing". But this is exactly what reification means. When properties are introduced, the values of varia-

[15] *Op. cit.* pp. 102–103.

[16] *Op. cit.* p. 103; W. V. O. QUINE, *Word and object* (New York/London 1960) p. 120.

[17] *Logico-philosophical Studies*, ed. A. Menne (Dordrecht 1962) p. 131.

bles are always qualified. In the controversy between realists and nominalists the referent of "qualify" is under discussion. If "to be" had different meanings when applied to things and properties, the problem of universals would vanish [18] and be replaced by an *analogia entis*. While Bocheński expresses, in this context, a critique of the reification of predicates which, from a realistic point of view, appears nominalistic, Quine has merely tried to "point to certain forms of discourse as *explicitly* presupposing entities of one or another given kind" without settling the issue.[19] This discussion is revelant here since this paper studies the possible implications of certain expressions without trying to settle the issue either.

In a natural language ontological commitment is not determined, but merely suggested by the kinds of expression utilized. In Sanskrit entities mentioned by names which can be substituted for *nīlatvam* in any expression of the form (2) or (3) can be said to be assumed by the language, when the reifications suggested by these expressions are accepted.

According to Quine's criterion, "$F(x) \supset G(x)$" does not require that there are entities "F" and "G", though "$(F)\,(G)\,(F(x) \supset G(x))$", where "$F$" and "$G$" are variables, does. Following Quine, quantification of the predicate can be avoided [20] by treating "F" and "G" as schematic letters. This convention can be applied to such expressions as (3), since (3), though it can be replaced by

[18] "in which case we need not concern ourselves": QUINE *op. cit.* p. 105. A criticism in this respect similar to Bocheński's, was given by B. MATES in: Synonymity, University of California Publications in Philosophy **25** (1950) 222–223. The idea of different meanings of "there is" is treated in: M. WHITE, *Toward Reunion in Philosophy* (Cambridge Mass. 1956) pp. 60–80. It is repudiated again by QUINE in *Word and object*, pp. 241–242.

[19] QUINE *op. cit.* p. 102. Towards the end of the essay Quine appears to take sides. Nominalism is finally rebuked pp. 173–174 and in *Word and object*, p. 243, note 5; cf. pp. 266–270.

[20] Quantification of the predicate (as in "$(F)\ F\,(x)$" or "God possesses all attributes") is different from propositions such as "every man is every animal", frowned upon since Aristotle (cf. J. F. STAAL, The Construction of Formal Definitions of Subject and Predicate, Transactions of the Philological Society (1960) pp. 95–97). The latter type of proposition is more adequately expressed by "$(x)\ (F(x) \leftrightarrow G(x))$".

similar nominal compounds, need not commit us to entities such as *nīlatvam* "blueness". The need does not arise if (3) is merely considered another way of saying (1), i.e., a synonym of (1). This reflects the nominalist point of view.

However, in Sanskrit realistic suggestions are strong: (3) is generally considered as of higher level, in some sense or other, than (1). As we shall see, there are good syntactical reasons for this view. An excessive use of abstract forming suffixes in general did not commit all Indians to realism, but suggested Platonic interpretations to an even larger extent than was the case in Europe. Still, nominalistic reactions did appear soon on the Indian philosophic scene as well. The discussion between realists (notably the Hindu logicians and the followers of Mīmāṃsā, the most orthodox of the six main philosophical schools of Hinduism) and nominalists (notably the heterodox Buddhists) continued for centuries. I shall not enter these philosophic discussions, which often have a familiar flavour, but only quote a fragment ascribed to the Buddhist logician Dignāga (VIth century A. D.). Dignāga ridiculed the realists' belief in universals by drawing attention to the incompatible functions universals would be required to perform:

> "Great is the skill of that which should, while residing in one place without moving from there, yet reside in what comes to exist in another place.
>
> This is connected with what is there and yet does not fail to pervade what occupies this place: a great miracle!
>
> It does not go, it does not stay, it does not exist afterwards, it has no parts, it does not leave its former receptacle – what a series of difficulties!"[21].

[21] *anyatra vartamānasya tato'nyasthānajanmani*
tasmād acalataḥ sthānād vṛttir ity atiyuktatā
yatrāsau vartate bhāvas tena saṃbadhyate na tu
taddeśinaṃ na vyāpnoti kim apy etan mahādbhutam
na yāti na ca tatrāste na paścād asti nāṃśavat
jahāti pūrvaṃ nādhāram aho vyasanasaṃtatiḥ (H. N. Randle, *Fragments from Diṅnāga* (London 1926) pp. 56–59.) I have adopted de la Vallée Poussin's reading *na* (for *ca*) in the fourth line and replaced the last two lines from the *Sarvadarśanasaṃgraha* (ed. V. S. Abhyankar (Poona 1951) pp. 27, 139–140) by two lines from the *Nyāyadīpikā* (quoted by Randle from: S. C. Vidya-

3. Quotation

We have seen that any expression can be converted into a noun by quoting it. Quoting, then, is a kind of nominalization. Often both are on a par with each other, e.g., English "he thought of coming" and "he thought 'I'll come'"; or Sanskrit *tamaso daśamadravyatvaṃ siddham* "it has been established that darkness is the tenth substance" and *tamo daśamadravyam iti siddham* "'darkness is the tenth substance' has been established"; or "artificial", "$F(x) \supset G(x)$" and "'$F(x)$' entails '$G(x)$'". In artificial languages the difference between quoting and merely containing is a difference of level (as between entailment and implication, rule and theorem), which can be made unsurmountable if the construction of the language demands it (e.g., in order to avoid paradoxes). In natural languages the difference is at the most gradual; more often, the two expressions are interreplaceable and can be described as synonyms.

In translating the second Sanskrit expression mentioned above, use is made of single quotes. These are absent in the Sanskrit original. Before studying the method used for expressing quotation in Sanskrit some general remarks must be made. From some examples it might appear that quotation is a much more general method for introducing expressions into sentences, than nominalization. Though we can find a parallel nominalization for the quotation of "he thought 'I'll come'" in: "he thought of coming", we cannot do this in such cases as: "he said 'I'll come'", "she thought 'how marvellous!'", "he cried 'I am lost'" and the like. In some of these cases there is a parallel construction in English by means of a dependent clause: "he said (that) he would come", etc. In Latin *accusativus cum infinitivo* can be used. In Sanskrit there is no such construction though there is something similar in the occurrence of the accusative of the present participle in the widespread construction *praviśantam mām apaśyat* "he saw me entering".

However, quotation can be said to provide a more general method for introducing expressions into our discourse only within

BHUSANA, *A History of Indian Logic* (Calcutta 1921) pp. 273–274 note). – Cf. OCKHAM, *Summa totius logicae*, ed. and tr. P. BOEHNER in: *Philosophical Writings* (Edinburgh etc. 1957) pp. 32–40; see also M. H. CARRÉ, *Realists and Nominalists* (Oxford 1946) p. 108.

a limited context. While nominalized expressions can be substituted for almost any noun occurring in a sentence, only some nouns could be replaced by quotations. In English quotations can occur: (1) after such verbs as *saying*, *shouting*, *dreaming*, *quoting*, ...(it is doubtful whether it occurs after *stating*, *expressing*, *believing*, ...); (2) after nouns such as *statement*, *expression*, *name*, *word*, *letter*, *rule*, *law*, *proverb*, etc.; and (3) in the same context as expressions such as "the statement '—'" mentioned under (2). As for the difference between (2) and (3) compare:

"the expression 'Damn it!' is to be avoided"
"'Damn it!' is to be avoided"
or: "the letter 'O' is not quite round"
"'O' is not quite round".

From a semantic point of view Austin is right when he says: "Inverted commas show that the words, though uttered (in writing), are not to be taken as a statement by the utterer".[22] He is only partly right when he continues: "This covers two possible cases, (i) where what is to be discussed is the sentence, (ii) where what is to be discussed is a statement made elsewhere in the words 'quoted'". For we may discuss not only sentences, but also words, case endings, letters, etc. Though words and sentences sometimes appear to have identical referents in Austin's work,[23] this is definitely not in accordance with ordinary English usage. Elsewhere Austin rightly emphasizes that a sentence is made up of words.[24]

[22] *Op. cit.* p. 88 note 3. The criticism in this footnote: "it is quite incorrect to say that 'The cat is on the mat' is the *name* of an English sentence", apparently directed against Russell and other logicians, is hardly convincing. Why is this *incorrect?* We have here a special use of the term *name* which coincides in some cases with ordinary usage, and is elsewhere artificial. The contention that it is dangerous to suppose that each name refers to a thing, is a different matter. – Tarski, who uses sentences, statements, etc. in the sense of Austin's *sentences*, has shown that the so-called *quotation-mark names* can be dispensed with when use is made of so-called *structural-descriptive names*. For this purpose we must have individual names of some sort for all signs of which the expressions of a language are composed (A. Tarski, *Logic, Semantics, Metamathematics* (Oxford 1956) pp. 156–157). In this connexion Quine speaks of *spelling* (*Word and object* pp. 143, 190).

[23] *Op. cit.* pp. 89–90.

[24] *Op. cit.* p. 88.

Now let us look at Sanskrit quotations. Instead of using left hand quotes to the left and right hand quotes to the right of written expressions, Sanskrit uses *iti* after the quoted expression, whether in speech or in writing, i.e.:

English "'—'" corresponds to Sanskrit "—*iti*".

Accordingly there may be uncertainty about the beginning of a quotation. In practice ambiguity is rare, partly on account of the context and partly because there are additional conventions.[25]

In this system of quoting, parallel uses to the three kinds of English quotation described above are common. Thus quotation can occur in Sanskrit: (1) before verbs such as *saying*, etc. e.g., *agaccham ity* [26] *āha* "he said 'I have come'"; (2) before nouns such as *statement*, etc., e.g., *tattvamasīti mahāvākyam* "the Great Statement '*tattvamasi*'"; *dravyam iti padam* "the word '*dravyam*'" (note how sometimes what is quoted can not be translated); and (3) as in (2), omitting the relevant noun, e.g., *tattvamasīty arthaḥ* "the meaning is '*tattvamasi*'"; —*iti tathā karoti* "after (the words) '—' he acts thus" (stage direction).

There are in Sanskrit, however, many more uses of *iti*, which are generally comparable to nominalization (*the fact that, as for*) and need not be connected with what in English could be called verbs of quoting. A few examples will be given here of such un-English usage.[27] For the sake of clarity, the Sanskrit clause which is bracketed by *iti* will be written in Roman while the corresponding part of the English translation will be italicized: *na* dharmaśāstraṃ paṭhatīti *kāraṇam* "*the fact that he reads law books* is not a reason (for confiding in him)";[28] śīghram iti *sukaram* nibhṛtam iti *cintanīyaṃ bhavet* "*as for doing it quickly*, it would be easy, *as for*

[25] See e.g. below note 28. Cf. Renou *op. cit.* p. 142 for special conventions.

[26] Final *-i* is replaced by *-y* before some vowels. The reader will have noticed and will continue to notice similar applications of so-called *sandhi* rules.

[27] See V. S. Apte, *Sanskrit-English Dictionary*, I (Poona 1957) pp. 381–2; A. A. Macdonell, *A Sanskrit Grammar for Students* (London 1950) pp. 148–149.

[28] Here another interpretation is excluded by ethical considerations: na dharmaśāstraṃ paṭhatīti *kāraṇam* "*the fact that he does not read lawbooks* is a reason (for confiding in him)".

doing it secretly, it would require consideration"; purāṇam ity *eva na sādhu sarvam* "everything is not good just *because it is old*"; brāhmaṇam iti *māṃ vidhi* "know me *to be a Brahman*". In these and similar cases, expressions terminated by *iti* are incorporated into sentences in the same way in which nominalized expressions are incorporated in sentences. In most cases it is possible to replace the quoted forms by nominalized forms.

These uses occur in ordinary Sanskrit. The Sanskrit grammarians classified some of them under the headings *śabdasvarūpadyotaka*, *prātipadikārthadyotaka* and *vākyārthadyotaka*.[29] The first, *śabdasvarūpadyotaka* "referring to a sound's own form" (corresponding to Austin's "sentences") is exemplified by grammatical expressions, e.g., *śatam iti pade śakāraḥ* "the *ś*-sound occurring in the word '*śatam*'". The second, *prātipadikārthadyotaka* "referring to the meaning of a nominal base" is exemplified by *dilīpa iti rājenduḥ* "the meaning of (the name) Dilipa is moon amongs kings". The third, *vākyārthadyotaka* "referring to the meaning of a sentence" corresponds to Austin's quoted statements. The general distinction between *arthadyotaka* "referring to its meaning" and *śabdasvarūpadyotaka* "referring to its own form" corresponds to the Western distinction between "use" and "mention".[30]

As is well known, the Indian grammarians attempted to arrive at the shortest possible description of linguistic phenomena.[31] They generally confined themselves to forms and excluded meaning. Since in ordinary language *iti* is used whenever a form is to be mentioned, grammatical descriptions should abound in occurrences of *iti* and this should conflict with the economy criterion that all superfluous elements ought to be deleted. The grammarian Pāṇini

[29] Apte *loc. cit.*

[30] See e.g. W. V. O. Quine, *Mathematical Logic* (Cambridge Mass. 1951) pp. 23–26. For the medieval *suppositio formalis* and *suppositio materialis* see Bocheński, *Formale Logik*, pp. 188–193, 277. Cf. also P. A. Verburg, *Taal en functionaliteit* (Wageningen 1952) *Zaakregister* s.v. *suppositio*. See further J. F. Staal, Indian Semantics, Journal of the American Oriental Society (forthcoming).

[31] Applications of this principle in: J. F. Staal, A Method of Linguistic Description: the Order of Consonants according to Pāṇini, Language **38** (1962) 1–10.

(ca. 400 B. C.) therefore framed a meta-rule (*paribhāṣā*) [32] for the particular practice to be followed in his grammar: "a word (in a grammatical rule) which is not a technical term refers to its own form".[33] This rule, to which attention was drawn by Brough,[34] implies that when a grammatical rule says that *agni* "fire" has the suffix *-eya*, the suffix has to be attached to the form itself and not to its meaning or referent. We clearly cannot add the suffix to the embers, says Patañjali.[35] This convention conflicts with ordinary usage, which employs *iti* as in *agnīti* "'*agni*'". As for technical terms, for example *śa* referring to "the infix '*a*'", these should be replaced by their referent: the result of inserting *śa* between *tud-* and *-ti* is *tudati*, not * *tudśati*.[36] Similarly, when a rule of English grammar states that the plural of nouns is formed by adding a "suffix '*s*'", this implies that the plural of *horse* is *horses* but not **horsesuffixs*.

By means of this rule Pāṇini makes a distinction between the object language, to which Sanskrit forms which he describes belong, and the meta-language of description, to which not only technical terms belong, but also meta-rules such as the rule which lays down the distinction between object language and meta-language. One test for differentiating between mentioning forms of the object language and using forms of the meta-language is translation into another language: forms of the meta-language are replaced but forms of the object-language remain. However, Pāṇini and his immediate

[32] Cf. J. F. Staal, The Theory of Definition in Indian Logic, Journal of the American Oriental Society **81** (1961) 122–126; Negation and the Law of Contradiction in Indian Thought: a Comparative Study, Bulletin of the School of Oriental and African Studies **25** (1962) 52–71.

[33] Pāṇini 1.1.68: *svaṃ rūpaṃ śabdasyāśabdasaṃjñā*.

[34] J. Brough, Theories of General Linguistics in the Sanskrit Grammarians, Transactions of the Philological Society (1951) 27–46.

[35] Brough *op. cit.* p. 29. Since attention is confined to the expression (*agni*) and excludes the reference (embers), quotation becomes referentially opaque (*Word and object*, p. 144). (Patañjali is generally placed in the 2nd century B.C., but see E. Frauwallner in, Wiener Zeitschrift für die Kunde Süd- und Ostasiens und Archiv für indische Philosophie **4** (1960) 92–118.)

[36] For further particulars see J. F. Staal, review of B. Shefts, Grammatical Method in Pāṇini: his Treatment of Sanskrit Present Stems (New Haven 1961) in, Language **39** (1963) 483–488.

followers never considered translating their grammar into another language (the matter becomes different with the later Prakrit and Dravidian grammarians). Another test for differentiating both forms is substitution of synonymous expressions. For example, if *-eya* is attached to *agni* this does not imply that it should also be attached to the synonym *tejas*. On the other hand, if a synonym for *śa* were available, this could be substituted for *śa* in rules of the meta-language, since both would have to be replaced by their referent, i.e., *-a-*, if the rule were applied to the object-language. Similarly, in English the first word of: '"fire' consists of four letters" cannot be replaced by a synonym (unless it happens to consist of four letters too), but the first word of "fire is hot" can.[37]

In general, Pāṇini's meta-language is an artificial language which is highly formalized and not intelligible to ordinary Sanskrit speakers or students of natural Sanskrit. The practice of quoting forms without *iti* is equally artificial. It could be compared to avoidance of quotes in Western philology and linguistics by italicizing forms of the object-language (this is often done, e.g., in this paper, when italicized forms of the object-language are accompanied by translations written in Roman and placed within quotes). In Sanskrit Pāṇini's convention is the opposite (*viparyāsa*) of ordinary usage. In order to make the reversal complete and the result consistent, Indian grammarians have also introduced the convention, that forms which are quoted in the grammar (i.e., followed by *iti*), refer to their meaning. This is a reversal of ordinary usage as well, since using a natural language means in the first place using its elements and not merely mentioning them.[38] The technical use of *iti* for expressing use occurs for example in a meta-rule of Pāṇini which defines *option*, an important concept in linguistics, where many rules are optional. Pāṇini defines *vibhāṣā* "option" as follows:

[37] Cf. e.g. QUINE, *Mathematical Logic*, p. 26. This has been called the paradox of denotation: see E. W. BETH, *The Foundations of Mathematics* (Amsterdam 1959) esp. pp. 516–518.

[38] Cf. Mates' German soldier who said: "Es schneit". The interpreter Jones, asked for an interpretation, replied: "He uttered a German sentence consisting of two words, the first of which was 'es' and the second, 'schneit'" ("Synonymity", p. 206).

na veti vibhāṣā.[39] This means that *vibhāṣā* is *na* "not" and *vā* "or". The referents of *na* and *vā* are prohibition and alternative, respectively. Now in this rule must the definiens be understood as "not" and "or" or as not and or? The former is excluded, for rules about *na* and *vā* occur elsewhere in the grammar and the present rule does not mean that occurrences of the term *vibhāṣā* in other rules should be replaced by occurrences of *na* and *vā*. The latter is correct, for the meta-rule gives the meanings of *na* and *vā* in order to explain how *vibhāṣā* is used in the grammar: if this term occurs in a rule it implies that first a prohibition is given and next an alternative. For example, if there is option with regard to the form *śuśāva* "it has swollen", the form can be prohibited and replaced by another form derived in accordance with other rules of the grammar, e.g., *śiśvāya*. The option obtains between *śuśāva* and *śiśvāya*. In brief, since the expressions *na* and *vā* are marked by *iti*, they are not mentioned but used.

The fact that Pāṇini and his followers refer to forms of the object-language without making use of *iti* has some unexpected consequences. In ordinary Sanskrit a sentence can mention a form by quoting it; hence this form can occupy the place of a noun in a sentence. When referring to the spelling of *asti* "he is", the Sanskrit man in the street will say *astīti padam* "the word '*asti*'" or *astīti* "'*asti*'" (the Western man in the street could at the most *write* the last expression). If somebody says about a man "he is on holiday", while in fact the person he refers to has died, the Sanskrit man in the street could say: "you should have said 'he was' in the place of 'he is'". For "in the place of 'he is'" he will use the expression *astīti padasya sthāne* (where *sthāne* "in the place of" is the locative case of *sthānam* "place", and *padasya* "of the word" the genitive of *padam* "word").

Linguists are always engaged in substitutions and Pāṇini has constantly to say "in the place of". For the sake of simplicity he therefore deletes *sthāne* and retains only the genitive of the form in the place of which something else should be substituted.[40]

[39] Pāṇini 1.1.44. For what follows cf. Y. OJIHARA and L. RENOU, *La Kāśikā-vṛtti*, I (Paris 1960) pp. 120–121.

[40] Pāṇini 1.1.49: *ṣaṣṭhī sthāneyogā* "the (technical) genitive expresses

This cannot easily produce ambiguity, since non-technical uses of the genitive are always perspicuous. Had Pāṇini further adopted ordinary usage, he could have said *astīti padasya* for "in the place of '*asti*'". But now the general convention can be applied, i.e. *asti* can be referred to without being quoted. To put this to practice another artifice is introduced. The expression *asti* "he is" is a conjugated verbal form but has now entered a sentence where it is going to function as a noun. It can therefore be declined as a noun and treated like nouns ending in *-i*. Since the genitive case termination of nouns ending in *-i* is *-eḥ*, Pāṇini expresses "in the place of '*asti*'" by means of a nominalization of a verbal form: *asteḥ*.

As in many other languages, the verb "to be" is rather irregular in Sanskrit. Some of its forms can be described as derived from *asti*, others as derived from a root *bhū*. This root, when referred to, need not be quoted but can be treated as a noun ending in *-ū*. The nominative case ending of nouns ending in *-ū* is *-ūḥ*. Pāṇini treats some irregularities of the verb "to be" by specifying conditions when *asti* should be replaced by *bhū*. He therefore needs the expression "'*bhū*' (should be substituted) in the place of '*asti*'". This job can be done by *asteḥ bhūḥ*, which then on phonemic grounds is replaced by *aster bhūḥ*.[41]

To explain this single rule a relatively long analysis has been required. The result, though concise, may appear neither practical, not sufficiently lucid. But this is only one example. Such expressions become both useful and perspicuous by their incorporation in a system. In this way the Indian grammarians describe linguistic facts in a concise and consistent way. Such expressions are different from natural Sanskrit and therefore artificial, but no more artificial than expressions such as "$F(x) \supset G(x)$". Their significance is fully understood only when the total system in which they are used is studied in greater detail than is possible here. Consider the opposite case: explaining to an Indian grammarian (1) the meaning of "if I sign the test ban treaty, I recognize East Germany", and (2) the advantage of representing this by "$F(x) \supset G(x)$".

the relation in the place of". Cf. J. F. STAAL, Context-sensitive rules in Pāṇini, Foundations of Language **1** (1965) 62–71.

[41] Pāṇini 2.4.53; cf. 8.2.73.

Actually similar artificial uses occur in Aristotle who speaks of τὸ ἔστι "the is", and the schoolmen who added *quidditas*, *quodditas*, and the like. In English such expressions are not absent either, though they seldom occur and are never directly incorporated in a scientific language.[42] When Austin calls one of his essays "Ifs and cans"[43], the title is, though artificial, immediately intelligible: no philosopher who adheres to ordinary usage has objected to it on behalf of ordinary usage. Austin uses *if* and *can* as if they were nouns and forms their plural as if he were treating of horses and cows. Of course, the essay drives home that these words do not function like nouns and can never be reified. Nevertheless the title exemplifies reification suggested by nominalization.[44]

Even this usage may appear less extraordinary when compared to an expression which is quite natural: though English balloons which fly up and up to the skies are never said to rise ups, the scholar who is puzzled by these limitations of language may very well have his ups and downs.[45]

[42] They are indirectly incorporated in formal systems in the sense dealt with here (above p. 157).

[43] *Op. cit.* pp. 153–180. In the first line of this essay: "Are *cans* constitutionally iffy?" *can* is italicized, but *if* is not. Does this mean that *can* is more canny than if?

[44] Titles need not be nominalized, though they often are: the treatment of a subject suggests slight reification. Ryle, when first writing about *about*, called his paper "About" (G. Ryle, "About", Analysis **1** (1933) 1011), but Goodman is content with About (above note 14), reserving *aboutness* for the text.

[45] In the above mentioned review (note 36), where some Latin examples of artificial nominalization have been constructed, italicization in translations of technical treatises of Sanskrit grammarians is discussed. – If ifs and cans are acceptable, and can can be said to be iffy, the criticism that Carnap and Ayer directed against Heidegger's *das Nichts nichtet* applies to nothing, but does not affect nothings once it is taken for granted that nothing is meaningful (in this sentence quotes are optional). In all such cases the parts of speech are no longer used as disjunct classes: conjunctions are nominalized or adjectivized and nouns are verbalized. In historical linguistics, this may have happened in, e.g., English or Chinese. In philosophy the need was felt, not only of neologisms, but also of neogrammaticalisms (cf. M. Heidegger, *Sein und Zeit* (Halle 1931) p. 39: "für die letztgenannte Aufgabe (i.e., Seiendes in seinem Sein zu fassen) fehlen nicht nur die Worte, sondern

4. Nominalization

In order to characterize nominalization as used in the artificial Sanskrit of Indian logic and philosophy, a kind of model [46] and an outline of its construction will be given. This model is the result of a generalization from some important features of this nominalization. It will consist of rules which exhibit methods by which artificial nominalized expressions can be derived from sentences in natural Sanskrit. Some sentences which can be derived from the model may not be idiomatic, while many others, though probably idiomatic, have not been found in existing texts.[47] Illustrations will partly

vor allem die 'Grammatik'"). Aristotle and the schoolmen did not shrink from such artificialities, as we saw before. The problem is what use is made of such jargon and, in particular, whether such expressions are incorporated in a system. Isolated examples of artificial extensions of grammatical possibilities are also found in Indian philosophy. Sureśvara uses the Sanskrit personal pronoun *aham* "I" to denote the Self as well as the ego. In the first case he forms a regular genitive *mama* "of the Self", in the second het treates *aham* as a noun and forms the artificial genitive *ahamaḥ* "of the ego" (Hacker, *op. cit.* p. 1952 note 2).

[46] The term "model" is used here in a general sense and without specific reference to model theory.

[47] The apparent discrepancy between an actually available corpus of linguistic utterances and the probably idiomatic results of constructions is none other than the distinction Chomsky made between linguistic *data* and linguistic *facts*: see N. CHOMSKY, Some Methodological Remarks on Generative Grammar, Word **17** (1961) 219–239. The identification of utterances as belonging to a corpus through checking with informants (cf. Z. S. HARRIS, *Structural Linguistics* (Chicago 1960) p. 12 note 12) is no easy matter when dealing with a language such as Sanskrit which, though not dead, is still mainly available in a large corpus of texts. Such a corpus rarely provides the exact answer to a particular question: this was another reason for proposing a model instead of a description in the present paper. Though Harris' methodological requirements may need some modification in view of languages like Sanskrit, it is imperative that a modern linguist should study Sanskrit with the help of a paṇḍit who speaks the language fluently (fortunately, they are not rare in India). Apart from the use that can be made of the work done by Indian grammarians, an advantage of Sanskrit as an object of structural research is that we can confine ourselves to the language (whether spoken or written) as it is spelled, for the spelling is largely phonetic (Cf. W. S. ALLEN, *Phonetics in Ancient India* (London 1953) and the article quoted above note 31).

be taken from studies on Sanskrit artificial and semi-artificial nominalization by Jacobi, Hartmann and Renou.[48] In order to arrive at an adequate and formal description which is satisfactory from a linguistic point of view, a much larger body of texts will have to be examined than is possible here. There is no lack of such texts, even in Western libraries. I therefore hope that linguists who know Sanskrit will be prepared to undertake this job. The following sketch may have heuristic value for them. Actually Sanskrit nominalization, natural as well as artificial, should prove an exciting topic for structural linguists. The reaction against Latin grammar, an important factor in the establishment of modern linguistics, has had its salutary effects. Linguistic methods, successfully applied to English and numerous "exotic" languages, appear now powerful enough to tackle the language which has always been the cornerstone of Indo-European philology. Moreover there are structures in Sanskrit which a student of Latin could hardly have dreamt of, let alone appreciated. Nominalization is one of these.[49] It is imperative that modern linguistic research on Sanskrit should follow the lead of the Indian grammarians, especially of Pāṇini. Western grammars may be consulted if necessary. Structural linguists who study Pāṇini may well experience a strangely familiar atmosphere and

[48] H. Jacobi, Über den nominalen Stil des wissenschaftlichen Sanskrits. Indogermanische Forschungen **14** (1903) 236–251; P. Hartmann, *Nominale Ausdrucksformen im wissenschaftlichen Sanskrit* (Heidelberg 1955) (cf. also the same, 'Lebendige' Sprachformen: Zur Frage des nominalen Ausdrucks. Sprachforum **1** (1955) 223–233, especially p. 229); L. Renou, *Histoire de la langue sanskrite*, and Recherches sur l'emploi du participe in, *Etudes de grammaire sanskrite*, I (Paris 1936). – Nominalization is also conspicuous in *kāvya* (erudite poetry).

[49] Another feature is nominal composition: cf. J. F. Staal in BSOAS **23** (1960) 110–113 and Synthese **12** (1960) 281. Cf. also Renou *op. cit.* p. 144: "En définitive, le *bhāṣya* à son apogée – surtout le *bhāṣya* philosophique – a été une réussite achevée dans la voie de l'abstraction, de la condensation, et cela dans des conditions d'autant plus extraordinaires que rien, dans le substrat linguistique élémentaire, n'y préparait". – (Added in the proof) Nominal composition is treated at length in R. Harweg, *Kompositum und Katalysationstext vornehmlich im späten Sanskrit* (The Hague 1964) which was published after the above article was sent to the press (August 1963) and will be reviewed in the Indo-Iranian Journal.

this is not surprising, since Pāṇini's grammar contributed, mainly through Bloomfield, to the foundations of modern linguistics.

To logicians the following sketch may seem to be primarily a linguistic affair. But which Indian logician would not at first sight consider an introduction to a Western formal system a special chapter of English syntax? [50] One cannot talk about artificial Sanskrit without talking about Sanskrit; this gives the exposition a linguistic flavour. Another factor will contribute to create a similar impression. In the following outline some ideas of Chomsky will be utilized, though the formalizations proposed here will be different from those used by Chomsky [51] and Lees [52]. Though Chomsky is regarded by some linguists as a kind of logician, most linguists and logicians rightly consider his work an attempt at linguistic description of natural languages, which has in common with logic – and with many of the sciences – a good amount of formalization.[53] One respect in which the model proposed here is different from a logical model, is that it explicitly starts from Sanskrit sentences. Two respects in which it is different from a linguistic description are that (1) its object consists of linguistic structures which are partly artificial and (2) it merely exhibits *some* structures of the object-language. Chomsky and Lees do not provide models in this sense, but attempt to construct adequate descriptions.

The peculiar situation we are faced with here is determined by the object of study: an artificial language constructed from and within a rather unfamiliar natural language. Since for this reason natural features of the artificial language will have to be studied as well, while on the other hand Western students of Western artificial

[50] Cf. RUSSELL's remarks on natural languages when introducing formal expressions in *Introduction to Mathematical Philosophy* (London 1948) esp. chapters XV–XVII.

[51] Especially: N. CHOMSKY, *Syntactic Structures* ('s-Gravenhage 1957) Appendix I *et passim*.

[52] R. B. LEES, *The grammar of English Nominalizations* (The Hague 1963).

[53] Of course there is more to it: see, e.g., N. CHOMSKY, Logical Syntax and Semantics: their Linguistic Relevance, Language **31** (1955) 36–45; E. W. BETH, Konstanten van het wiskundige denken. Med. Kon. Nederlandse Akademie v. Wetenschappen, Afd. Letterkunde, Nieuwe reeks **26**, 7 (1963) 231–256.

systems have often forgotten the structures of their own natural language, generally assimilated some decades earlier, conditions are created for a *coup d'œil* on both natural and artificial features of language. The results will probably suffice to show that the difference between natural and artificial language is relative (actually some features could be described as semi-artificial). This has nothing to do with the fact that Western artificial languages appear more artificial than Indian ones (an impression partly created by formalization); on the contrary, this fact points to the existence of degrees of artificiality. Some structures of Western logic could be looked upon as partial descriptions of the skeleton (in some sense or other) of a natural language. Other structures could be interpreted as constructions inspired or suggested by particular structures of natural languages.[54] The gap between natural and artificial languages appears unbridgeable mainly to those who neglect the intermediaries and whose image of the situation resembles a total blank with a play of Shakespeare at the one end and an article in the *Journal of Symbolic Logic* at the other. In fact, both scientific Sanskrit and Western formal systems can be described as specimens of artificial Indo-European.

The kind of nominalization studied here can be described as a development of the following, earlier mentioned, sequence of expressions:

ghaṭo nīlaḥ	(1)
ghaṭasya nīlatvam	(2)
ghaṭanīlatvam	(3)

In order to study the nominalizations in (2) and (3), let (1) be called a *source sentence* or *source* and let (2) and (3) be called *transforms* which can be derived from (1) by *transformational rules*, T1 and T2 respectively. In general, sources are expressions of natural Sanskrit whereas nominalized transforms are expressions of artificial Sanskrit. The further nominalization proceeds, the greater the degree of artificiality. The transformational rules can therefore be regarded as rules for the construction of an artificial language from a natural language. Transformational rules will be written

[54] Cf. the second article quoted above (note 32), esp. p. 71.

in the form $X \rightarrow Y$ "rewrite X as Y", where Y is a transform and X is a source, which itself may be a transform. The transformational rules T1 and T2 should therefore be of the form:

$$(1) \rightarrow (2) \qquad \text{T1}$$
$$(1) \rightarrow (3) \qquad \text{T2}$$

If required, a transformational rule may also describe:

$$(2) \rightarrow (3) \qquad \text{T3}$$

and thereby show that (3) can be described as a transform of the second order.

Sources such as (1), which are not transforms, will be considered as given: an analysis of transformational structure presupposes an analysis of phrase structure. *Phrase structure rules* will be required to produce sources along with specifications of their constituent structure. For the present purpose it will suffice to describe sources and transforms as strings, the elements of which are morphemes, words or larger units. The original and derived P-markers (structural descriptions) will not be given here but can be easily supplied. The phrase structure rules must be supplemented by *lexical rules*, by means of which specific morphemes, etc., are substituted for general expressions for morphemes, etc. To the resulting expressions *sandhi rules* (roughly: morphophonemic rules) are applied in order to produce grammatical sentences of the form (1). The transforms will undergo a similar treatment. Since attention will be mainly directed to transformational rules, the lexical and sandhi rules will be only illustrated. The notation used in these illustrations is clear from the context and will not be described. Adequate formulations of phrase structure and sandhi rules would require a restatement of a large part of Sanskrit grammar.

Since sources and transforms are described as strings consisting of elements, the only relation required for the description of strings is the relation of *concatenation* which holds between two elements which occur in a string in immediate succession. Concatenation will be symbolized by "–". Since word and sentence boundaries are presupposed, the open space between words and at the beginning and end of sentences is also an element of strings. This element will be symbolized by "$\neq$". This symbol will never be used, however, since "$- \neq -$" will be abbreviated as "+" and initial "$\neq -$" and final "$- \neq$" will be omitted.

A rough outline of the phrase structure analysis can be given along the following lines. Before stating the lexical rules, the elements of strings will be classified under the following types:

(i) $x, y, \ldots, N(x), N(y), \ldots, N(v), \ldots$ interpreted as *nominal stems;*

(ii) $v, \ldots$ interpreted as *verbal stems*;

(iii) 1, 2, . . ., 7 interpreted as case endings in Pāṇini's order:
1 nominative
2 accusative
3 instrumental
4 dative
5 ablative
6 genitive
7 locative;

(iv) $t, \ldots$ interpreted as *verbal endings;*

(v) $\neq$.[55]

The elements $N(x), N(y), \ldots, N(v), \ldots$ can be interpreted as nominal stems of nominalizations of elements of types (i) and (ii). Repeated nominalization can occur, as was noted before. The nominalization of sentences will be described by transformational rules. This implies that little attention will be paid to some parallels between nominalizations at different levels. Such considerations are nevertheless relevant, since use could be made of parallels to increase the degree of simplicity in a full-fledged transformational grammar of Sanskrit.

[55] For more adequate statements of phrase structure rules, the types of elements should be further subdivided and specified, other types should be added, and different types may be expressed with the help of different typographical means, subscripts, accents or the like. In this paper no ambiguity will arise though small Roman letters are used in most cases.

Lexical rules

$x, y, \ldots \rightarrow$ *ghaṭa, nīla, maṅgala, karaṇa*, ...
$v, \ldots \rightarrow$ *bhava, padya, sādhaya, kalpa*, ...
$1 \rightarrow$ 0(zero), *ḥ, m, s, ā, ī*, ...
$2 \rightarrow$ *m*, ...
$3 \rightarrow$ *ena, ā*, ...
$4 \rightarrow$ *āya*, ...
$5 \rightarrow$ *at, smāt, eḥ, yāḥ*, ...
$6 \rightarrow$ *sya, nām, eḥ, aḥ*, ...
$7 \rightarrow$ *e, ām*, ...
$t, \ldots \rightarrow$ *ti, te, nte*, ...
$N(x), \ldots \rightarrow$ x–*tva*, x–*ta*, x–*na*, x–*ti*, ...

Sandhi rules

tamas + 0 → *tamaḥ*
ghaṭa — *ḥ* + *nīla* — *ḥ* → *ghaṭo nīlaḥ*
kalpana –*yāḥ* → *kalpanāyāḥ*
rasa – *nām* → *rasānām*
jñāna – *āya* → *jñānāya*
bhakti – *eḥ* → *bhakteḥ*
bhaktiḥ jñānāya → *bhaktir jñānāya*
tat – *utpattiḥ* → *tadutpattiḥ*
nīla – *ḥ* + *iti* → *nīla iti*
utpadya – *te* + *iti* → *utpadyata iti*
etc.

Transformational rules

$x - 1 + y - 1 \rightarrow x - 6 + N(y) - 1$ T1
$x - 1 + y - 1 \rightarrow {}^{0}x - N(y) - 1$ T2

With the help of lexical rules these can be exemplified as:

ghaṭa — *ḥ* + *nīla* — *ḥ* → *ghaṭa* — *sya* + *nīla* — *tva* — *m* T1
→ *ghaṭa* — *nīla* — *tva* — *m* T2

With the help of sandhi rules this can be written as:

ghaṭo nīlaḥ → *ghaṭasya nīlatvam* T1
→ ^{0}*ghaṭanīlatvam* T2

Other substitutions can be similarly made, e.g.;

$x \rightarrow$ *tamas*
$1 \rightarrow 0$ (*tamaḥ* "darkness")
$y \rightarrow$ *daśamadravya*
$1 \rightarrow$ *m* (*daśamadravyam* "tenth substance")
$6 \rightarrow$ *aḥ*

After applying sandhi rules also the results are:

tamo daśamadravyam → *tamaso daśamadravyatvam* T1
→ °*tamodaśamadravyatvam* T2
"darkness is the tenth substance".[56]

Another example:

$x \rightarrow$ *karaṇa*
$1 \rightarrow$ *m*
$y \rightarrow$ *kāryaniyatapūrvavṛtti*
$1 \rightarrow 0$ (*kāryaniyatapūrvavṛttiḥ* "necessarily preceding the effect")
karaṇam kāryaniyatapūrvavṛtti → *karaṇasya kāryaniyata-pūrvavṛttitvam* T1
→ °*karaṇakāryaniyata-pūrvavṛttitvam* T2
"the cause necessarily precedes the effect".

So far sources have been representend by purely nominal sentences, where subject and predicate occur in the nominative. Next verbal sentences will be considered consisting of: (1) a noun in the nominative; (2) a noun in the accusative, instrumental, dative, ablative or locative;[57] and (3) a verb. The elements of the strings

[56] Quoted earlier (above p. 161).

[57] Deletion of the genitive corresponds to its so-called quasi-adjectival function. In Pāṇini this is expressed by deleting the genitive in the list of cases which can be used to express a relation between the referent of a noun and the referent of a verb (*kāraka*; exceptions are dealt with in Pāṇini 2.3.51–63). Philologists usually call these cases oblique. The genitive expresses a relation between two nouns. This can also be nominalized by the simple rule:

$$x - 6 + y - 1 \rightarrow x - y - 1$$

which yields a *tatpuruṣa* compound (cf. BSOAS **23** (1960) 111–112).

will be given in this order, though the order of nouns may be reversed.[58] Such sentences can be nominalized in accordance with the following transformations which hold for numerals i such that $i \neq 1$, $i \neq 6$, and (in the case of T5) $i \neq 2$:

$$\begin{aligned} x - 1 + y - i + v - t &\to x - 6 + y - N(v) - 1 && \text{T4}^{59} \\ &\to y - i + x - N(v) - 1 && \text{T5} \\ &\to {}^{0}y - x - N(v) - 1 && \text{T6} \end{aligned}$$

Note that T4 and T6 result in transforms which can be ambiguous, since i has disappeared.[60] Such cases of "nominalizational homo-

[58] In highly inflexional languages such as natural Sanskrit a problem is created by the optional character of word order, by which generally synonymous sentences are produced. This suggests a description in other terms than strings. A grammar constructed for the generation of strings should contain numerous additional rules to exhibit these options. It might be more economical to mention only the restrictions by which certain types of word order are excluded. Since the order of elements in nominalized expressions is relatively fixed (cf. note 60) a description in terms of strings is undoubtedly the most simple. See further H. B. CURRY and H. HIŻ in: *Structure of Language and its Mathematical Aspects* (Providence 1961) pp. 66 and 256.

[59] Note that T1 can be considered a special case of T4 if T4 is written as:

$$x - 1 + y - i + v - t \to x - 6 + N(y, v) - 1 \;(i \neq 6).$$

However the simplicity thus gained at the transformational level is probably lost at the phrase structure level, where additional rules will be required, e.g.:

$$\begin{aligned} N(y, v) &\to N(y) \text{ for } v = 0, \textit{as}, \textit{bhava}, \ldots \\ N(y, v) &\to y - N(v) \text{ for } v \neq 0 \end{aligned}$$

(In the last expression, *as*, *bhava*, ... need not be excluded since we may need $N(as) \to sat$, $N(bhava) \to bh\bar{a}va$, etc.) Attempts to find an *ad hoc* solution may await a general decision procedure for simplicity, covering all levels of grammatical description. As far as I know this has not been done (cf. N. GOODMAN, *The Structure of Appearance* (Cambridge Mass. 1951) pp. 59–85; J. G. KEMENY, The Use of Simplicity in Induction, Philosophical Review **62** (1953) 391–408; M. HALLE, On the Role of Simplicity in Linguistic Descriptions, Structure of language and its mathematical aspects (American Mathematical Society 1961) pp. 89–94).

[60] A generalization of this disappearance of i in nominalized expressions implies that the inflected system of natural Sanskrit is exchanged for an artificial system where the order of elements becomes increasingly important. At the same time word boundaries disappear. Hartmann has given a semantic analysis of this system and has shown how the result resembles the structure

nymy" are relatively common in Sanskrit. While the hearer or reader understands such sentences on account of the context, the different uses can be described without semantic analysis by mentioning the transformational history, i.e., by describing transforms in terms of transformations such as T4 and T6.

For each case one example will follow:

$i = 2$
$x \rightarrow$ *maṅgala*
$1 \rightarrow$ *m* (*maṅgalam* "introductory benediction")
$y \rightarrow$ *samāpti*
$2 \rightarrow$ *m* (*samāptiḥ* "completion")
$v \rightarrow$ *sādhaya*
$t \rightarrow$ *ti* (*sādhayati* "accomplishes")
N(*sādhaya*) $\rightarrow$ *sādhanatva*
maṅgalaṃ samāptiṃ sādhayati $\rightarrow$ *maṅgalasya samāptisādhanatvam* T4
$\rightarrow$ ^{0}*samāptimaṅgalasādhanatvam* T6
"the introductory benediction accomplishes the completion (of the work)"

$i = 3$
$x \rightarrow$ *rasa*
$1 \rightarrow$ *aḥ* (*rasaḥ* "sentiment")
$y \rightarrow$ *svaśabda*
$3 \rightarrow$ *ena* (*svaśabdaḥ* "(its) own name")
$v \rightarrow$ *nivedya*
$t \rightarrow$ *nte* (*nivedyante* "are called")
$6 \rightarrow$ *nām*
N(*nivedya*) $\rightarrow$ *niveditatva*
rasāḥ svaśabdena nivedyante $\rightarrow$ *rasānāṃ svaśabdaniveditatvam* T4
$\rightarrow$ *svaśabdena rasaniveditatvam* T5
$\rightarrow$ ^{0}*svaśabdarasaniveditatvam* T6
"feelings are called by their own name"

of systems like Chinese, where no inflexion exists and all units occur in juxtaposition so that order is a decisive factor (*op. cit.* p. 207 sq.).

$i = 4$

$x \to$ *bhakti*
$1 \to$ *ḥ* (*bhaktiḥ* "love")
$y \to$ *jñāna*
$4 \to$ *āya* (*jñānam* "knowledge")
$v \to$ *kalpa*
$t \to$ *te* (*kalpate* "conduces to")
$6 \to$ *eḥ*
N(*kalpa*) $\to$ *kalpana*

bhaktir jñānāya kalpate	$\to$ *bhakter jñānakalpanā*	T4
	$\to$ *jñānāya bhaktikalpanā*	T5
	$\to$ ^{0}*jñānabhaktikalpanā*	T6
	"love is conducive to knowledge"	

$i = 5$

$x \to$ *tat*
$1 \to 0$ (*tat* "that")
$y \to$ *anya*
$5 \to$ *smāt* (*anyaḥ* "other")
$v \to$ *utpadya*
$t \to$ *te* (*utpadyate* "arises")
N(*utpadya*) $\to$ *utpatti*

tad anyasmād utpadyate	$\to$ *tasya anyotpattiḥ*	T4
	$\to$ *anyasmāt tadutpattiḥ*	T5
	$\to$ ^{0}*anyatadutpattiḥ*	T6
	"that arises from something else"	

$i = 7$

$x \to$ *tamas*
$1 \to 0$ (*tamaḥ* "darkness")
$y \to$ *pṛthivi*
$7 \to$ *ām* (*pṛthivī* "earth")
$v \to$ *antarbhava*
$t \to$ *ti* (*antarbhavati* "exists in")
N(*antarbhava*) $\to$ *antarbhāva*

tamaḥ pṛthivyām antarbhavati	$\to$ *tamasaḥ pṛthivyantarbhāvaḥ*	T4
	$\to$ *pṛthivyāṃ tamontarbhāvaḥ*	T5

→ *pṛthivitamontarbhāvaḥ* T6
"darkness exists in earth"

From a logical point of view all transforms obtained by these transformations express propositions of the form "$F(x)$". Since the transforms are nominalized they can be introduced into sentences like nouns. The sources can be incorporated in a sentence only if quoted. The simplest kind of incorporation in a sentence is as of "x" in "$F(x)$" or as of a subject in a predicative sentence. In Sanskrit the predicate can be expressed by an adjective, the ending of which agrees with the ending of the subject (e.g., in gender). An example is the adjective *siddha*-"established". The masculine, feminine and neuter endings of *siddha*- are *ḥ*, *ā*, and *m*, respectively.

Let sources be denoted by "K", their nominalized transforms by "T", and masculine, feminine and neuter transforms by "T_m, "T_f" and "T_n" respectively, then:

"'K' is established" → "T is established"
"*K-iti siddham*" → "T_m *siddhaḥ*", "T_f *siddhā*" or "T_n *siddham*".

Accordingly we can construct:

ghaṭo nīla iti siddham → *ghaṭasya nīlatvaṃ siddham*
→ *ghaṭanīlatvaṃ siddham*
maṅgalaṃ samāptiṃ sādhayatīti siddham → *maṅgalasya samāpti-sādhanatvaṃ siddham*
→ *samāptimaṅgalasādhanatvaṃ siddham*
tad anyasmād utpadyata iti siddham → *tasya anyotpattiḥ siddhā*
→ *anyasmāt tadutpattiḥ siddhā*
→ *anyatadutpattiḥ siddhā*
etc.[61]

Readers who have been puzzled by the unexplained symbol ^{0} will be relieved by its absence here. While most nominalized transforms can occur as sentences or be incorporated in sentences, nominalized transforms marked with ^{0} may be incorporated in sentences, but rarely function as sentences themselves. Since they

[61] Another example occurred earlier (p. 161).

are sometimes incorporated they had to be first constructed. This explains the need for T2 and T6.

We have seen that quotation in English provides a general method for introducing expressions into our discourse within a limited context. In Sanskrit, *iti* can be used in many more contexts. The nominalized transforms constructed here can function in a still greater number of contexts. They can be substituted for almost any noun in a sentence. This partly explains their importance for artificial Sanskrit.

In general, all nominalized transforms function in the same way as the nominalized expressions (2) and (3) which were treated earlier. We have seen that a function of the Sanskrit ablative is to express a reason, so that *ghaṭanīlatvāt* expresses "since the pot is blue". As this holds for all nominalized transforms, it is possible to construct sentences which contain such expressions as:

kāraṇakāryaniyatapūrvavṛttitvāt "since the cause necessarily precedes the effect";
svaśabdarasaniveditatvāt "as feelings are called by their own name"
tasya anyotpatteḥ "because that arises from something else";
jñānāya bhaktikalpanāyāḥ "because love is conducive to knowledge".

The other cases will be considered next. The nominative and the ablative cases of nominalized transforms occupy the same place as "$G(x)$" and "$F(x)$", respectively, in "$F(x) \supset G(x)$". The locative expresses a conditional which is often counterfactual, e.g.:

maṅgalasamāptisādhanatve "if the introductory benediction realized the completion";
tamasaḥ pṛthivyantarbhāve "if darkness existed in earth";
anyasmāt tadutpattau "if that arose from something else".

If *api* is added to such locative expressions, the conditional becomes concessive:

jñānāya bhaktikalpanāyām api "even if love is conducive to knowledge";
svaśabdarasaniveditatve'pi "although feelings are called by their own name".

The dative expresses a purpose:

maṅgalasya samāptisādhanatvāya "in order that the introductory benediction may accomplish the completion"

and the instrumental expresses a reason or further specification of the reason, and is therefore generally used in combination with the ablative, e.g.,:

anyatadutpattyā maṅgalasamāptisādhanatvāt "since the introductory benediction realizes the completion in as far as that arises from something else".

Now let "Nom" denote any of the strings occurring to the right of the symbol "→" and to the left of the final elements "–1" in T1 – T6; let "S" denote any sentence and let "NP" and "VP" denote the noun and verb phrases respectively of "S"; then the previous results can be expressed by five transformational rules. These rules are of a type not considered before: the transforms are derived from more than one source. The following five expressions give only the transforms (the most common word order is exhibited: see footnote 58). For the first four, the sources are "S" and "Nom – 1"; for the last, "S", Nom – 1" and "Nom' – 1":

$$\text{Nom} - 1 + \text{VP} \qquad \text{T7}$$
$$\text{S} + \text{Nom} - 5 \qquad \text{T8}$$
$$\text{Nom} - i + \text{S} \quad (i = 4{,}7) \qquad \text{T9}$$
$$\text{Nom} - 7 + api + \text{S} \qquad \text{T10}$$
$$\text{S} + \text{Nom} - 3 + \text{Nom}' - 5 \qquad \text{T11}$$

Next the system will be extended in order to express negation and modality.[62] For negation the general rules are the following. It is assumed that the negative particle *na* "not" occurs in the source in concatenation with "VP" (this restriction can be removed if required: see footnote 58):

$$\text{NP} + na + \text{VP} \rightarrow \text{Nom} - 1 + n\bar{a}sti \qquad \text{T12}$$
$$\text{NP} + na + \text{VP} \rightarrow (a - \text{Nom}) - 1 \qquad \text{T13}$$

[62] Conjunction (by means of *ca* "and" or nominal composition (*dvandva*)) and disjunction (by means of *vā* "or") pose no special problem. For particular cases see Ingalls, *op. cit.* pp. 63–65.

where:

$(a - \text{Nom}) \rightarrow a - N(y), y - a - N(v), x - a - N(v),$
$y - x - a - N\ (v)$

and:

$$\text{NP} + na + \text{VP} \rightarrow \text{Nom} - abh\bar{a}va - 1 \qquad \text{T14}$$

Examples:

$x - 1 + na + y - 2 + v - t \rightarrow x - 6 + y - N(v) - 1 +$ *nāsti* T12

maṅgalaṃ na samāptiṃ sādhayati → *maṅgalasya samāptisādhanatvaṃ nāsti*

"the introductory benediction does not accomplish the completion"

$x - 1 + na + y - 4 + v - t \rightarrow y - x - a - N(v) - 1$ T13

bhaktir na jñānāya kalpate → *jñānabhaktyakalpanā*

"love is not conducive to knowledge"

$x - 1 + na + y - 1 \rightarrow x - 6 + N(y) - abh\bar{a}va - 7 + api$ T14

karaṇaṃ na kāryaniyatapūrvavṛtti → *karaṇasya kāryaniyatapūrvavṛttitvābhāve'pi*

"though the cause does not necessarily precede the effect"

Modality can be introduced into the system in a similar way as *abhāva* was introduced into T14. While various modal expressions can be variously introduced into the natural sources, there are only a few typical methods for introducing modality into artificial nominalized transforms. Modal functors are more easily incorporated in the artificial language than in natural Sanskrit. In order to avoid the complications of natural language, all modal sources will be denoted by "Mod(S)":

sambhava "possibility"

Mod(S) $\rightarrow y - i + x - N(v) - sambhava - 1$

anyasmād tadutpattisambhavaḥ

"that can arise from something else"

āvaśyakatva "necessity"

Mod(S) $\rightarrow x - 6 + y - N(v) - \bar{a}va\acute{s}yakatva - 1$

maṅgalasya samāptisādhanatvāvaśyakatvam "the introductory benediction must accomplish the completion"

anupapatti "impossibility"

Mod(S) $\rightarrow y - x - N(v) -$ *anupapatti* $- 5$

svaśabdarasaniveditatvānupapatteḥ

"since feelings cannot be called by their name".

The general rule can be stated as follows:

$$\text{Mod(S)} \rightarrow \text{Nom} - M - 1 \qquad \text{T15}$$

where:

$M \rightarrow$ *sambhava, āvaśyakatva, anupapatti, ...*

Further nominalizations can be constructed by combining T12–T14 with T15, combining substitution results of T15 with each other and substituting "Nom–M" for any "Nom" in T7–T11.

The transformations T1–T15 show methods by which Indian scholars have constructed an artificial language from ordinary Sanskrit. Since transforms derived from sources which are not transforms themselves can be called first order transforms, and transforms derived from n-th order transforms can be called (n+1)st order transforms, the order of transforms can be denoted by a numeral and regarded as a measure for the degree of artificiality. By adding the order numerals of the constituent transforms of a sentence, each sentence can be given an index which expresses the degree of its nominalization. In the finally resulting artificial language, implication, conditionality, concessiveness, finality, specification, conjunction, disjunction, negation and modality can be systematically expressed by means of sentences with various degrees of nominalization, resulting from various types of nominalization. Quantification, which also involves nominalization, poses a separate problem.[63]

[63] See Bocheński, *Formale Logik*, p. 513; Ingalls, *op. cit.* pp. 47–50; J. F. Staal, Means of Formalization in Indian and Western Logic, Proceedings of the XIIth International Congress of Philosophy, **10** (Firenze 1960) pp. 221–227 and BSOAS **23** (1960) 114; A. Uno, The Determination of Terms in Navya-nyāya, Journal of Indian and Buddhist Studies **7** (1958) 331–335.

Sentences marked with high nominalization indices suggest considerable reification, especially since logicians and most Hindu philosophers who use this language are realists. The issue between realists and nominalists depends on semantic interpretation. According to nominalists, the higher order transforms are identical in meaning with their sources. According to realists, they are not. In general, many transformations produce sentences that are synonymous with their sources. Exceptions are, for example, the question, imperative and negation transformations (cf. Fodor and Katz [64]). Nominalizing transformations occupy an intermediate position: semantic interpretation of nominalization involves ontological commitment.

What has been done so far corresponds mainly to a characterization of well-formed expressions of an artificial language. Transformational theory could be used for this characterization since its main purpose is to provide a description of grammaticality in natural languages. Grammaticality in natural languages corresponds to well-formedness in artificial languages. Moreover, transformational theory generates strings which could be used to exhibit the relatively fixed order of elements in nominalized expressions. However,

[64] The original statement of J. A FODOR (Projection and Paraphrase in Semantics, Analysis **21** (1960–61) 73–77) that most transformations change meaning, the most important exception being the active-passive transformation, has been exchanged, via J. J. KATZ (A reply to "Projection and Paraphrase in Semantics", Analysis **22** (1961–62) 36–41) for the formulation of FODOR and KATZ (The Structure of a Semantic Theory, Language **39** (1963) 170–210: see p. 206) adopted here. See now (added in the proof): J. J. KATZ and P. M. POSTAL, *An integrated theory of linguistic descriptions* (Cambridge Mass. 1964), esp. Ch. 4. Whether any two expressions are ever strictly *P*-related, identical in meaning or synonymous has been questioned, with special reference to terms rather than sentences, by N. GOODMAN (On Likeness of Meaning, Analysis **10** (1949–50) 1–7, On Some Differences about Meaning, Analysis **13** (1952–53) 90–96) and B. MATES (Synonymity, University of California Publications in Philosophy **25** (1950) 201–226). The discussion continues. The criticism of synonymity as well as the related attacks by Quine and White (from 1950 onward, but see *Word and object* p. 67 note 7) on the analytic/synthetic distinction were clearly foreshadowed in 1940 by Austin, who considered sentences as well as terms (see *Philosophical Papers* pp. 30–37).

the real work can only begin when the well-formed expressions are at hand. Only a logical language can provide the foundation for logic and an adequate treatment of interesting philosophic problems. But this is another topic.

It may be too early to speculate on the possible forms of transformational rules by which Western artificial languages can be derived from Western natural languages. I hope the above fragment will provide some methods which can be utilized in this type of foundational research. The result might contribute to the foundation of a science of language, which is empirical as well as creative and which includes an analysis of some of the roots from which logical and philosophic perplexities arise.

University of Amsterdam, The Netherlands

ANNEX TO CHAPTER 10

EINIGE LINGUISTISCHE BEMERKUNGEN ZUM VORSTEHENDEN THEMA

PETER HARTMANN

Nach einem freundschaftlich gewährten Einblick in sein Manuskript äusserte der Verfasser des vorstehenden Beitrags den Wunsch, dass seiner Darstellung noch einige Bemerkungen aus der Sicht eines Linguisten beigegeben würden. Dem komme ich gerne nach: Einmal sind die angeschnittenen Fragen so behandelt, dass sie für die Linguistik von erheblichem Interesse sein müssen – denn sie vereinigen semantische und formale Gesichtspunkte mit einem direkten Blick auf das für die Sprachtechnik entscheidende *Verhältnis zwischen Wort und Satz*, Name und Aussage, Teil und Ganzem. Zum anderen eröffnen sie eine neue Stellungnahme zur *Rolle der Sprache* als Symbolismus – was ebenfalls einem Linguisten nicht unwichtig bleiben kann, der bei aller systeminternen Forschung und Analyse gelegentlich wissen muss, in welcher Weise sein Objekt überhaupt realiter existent ist. Er wird eine Hilfe hierzu umso mehr begrüssen, wenn sie seitens einer Philosophie kommt, die es nicht ablehnt, ihre Behandlungsobjekte auch sachlich zu kennen.

Wenn es einem Linguisten auch niemals zustehen kann, allgemein zu sagen, was zuvor von den indischen Grammatikern zu bemerken war "the Indian grammarians, who often use more logic than professional logicians" (p. 152), so darf er darin doch vielleicht eine Nähe zu dem Gedanken sehen, der sich bei ihm als dem Analysator der natürlich-sprachlichen Formgebung gelegentlich meldet: dass die linguistisch-grammatische Analyse dort voraussetzungsfreier (formaler) ist als die log(ist)ische, wo sie noch *vor* aller Bindung durch Semantik liegt und ihr Objekt erforscht als wirklich *leere*, nämlich nur-kombinative (grammatische) *Form* – auch wenn diese "an sich" so "nie vorkommen kann". Eine entsprechend weit angelegte Linguistik könnte also zum Thema des Formalen in Ausdrücken etwas aussagen, das auch dem Logiker interessant sein dürfte, denn "logical expressions are closely related to the structure of languages in which they occur" (p. 152). Obgleich eine linguistische Lehre von den möglichen (vorhandenen) leeren Ausdrucksformen – als induktive Forschung – zunächst nur bis zu natürlichen letzten Formgebungen in realisierten Sprach- und Formulierungstypen kommen kann, würde sie doch ebenso eventuelle Abhängigkeiten (Ähnlichkeiten) zwischen natürlichen Ausgangssprachen und künstlichen Formalsprachen besser erkennen lassen, wie auch zur Frage eines nicht-mehr-natürlichen, voraussetzungsfreien und nur seinem log(ist)ischen Zweck adäquaten Symbolismus einiges beitragen können.

So zu Fragen wie:

> Ist überhaupt eine Sprache möglich, die, als reduziertes und speziell präpariertes System, *ausschliesslich* nach den Forderungen der von ihr auszusagenden Korrelate – der logischen Formen und Typen – eingerichtet ist?
>
> Ist sie überhaupt nötig oder genügt es, bei welcher halb-natürlichen oder halb-künstlichen Sprachform immer, jeweils *dazuzusagen*, wie die eine oder andere "unvollkommene" Ausdrucksformation hinsichtlich einer logischen Form zu verstehen ist?
>
> Wie sehen solche Forderungen aus, zu denen es ja z.B. gehören wird, dass es *gleichgültig* sein kann/muss, an welcher Stelle etwas wie die altbekannten Glieder Subjekt/Prädikat im Ausdruck erscheinen, auf Elemente welcher Formgebung (Wortart) der Subjekts- oder Prädikatwert fällt?
>
> Wie verhält sich andrerseits eine beliebig künstliche Sprache, wenn sie zwecks Sicherung ihres Informationswertes ihre wie immer benutzbaren Symbole (Namen, Ausdrücke) der *Funktion* nach unterscheiden muss, obgleich es für deren Bedeutungen vielleicht gleichgültig ist, wie sie überhaupt ihren Ausdruck finden?

So wenig hier zur Autonomisierung der Logik gesagt werden kann, so gewiss scheint es auch einem Linguisten, dass sie primär über eine Analyse ihrer semantischen Formen ("Sprache") gewinnbar und zu bewerkstelligen ist. Daher wäre eine allgemein und formal genug gefasste Lehre der leeren Ausdrucksformationen, eventuell auch der Werte derartiger Formen, den Interessenten beider Gebiete dienlich.

Doch wenden wir uns den im vorigen mitgeteilten Tatsachen zu. Die drei behandelten Punkte Reification aufgrund von Benennung, Quotation in Form eines Satzteils (Substantivs, Namens) und Substantivierung zum Satzteil werden für den Linguisten zweifach wichtig: 1. formal, 2. inhaltlich.

In formaler Hinsicht: Alle drei Titel fallen unter das Oberthema der Opposition zwischen Teil und Ganzem. *Reification* ist die Anerkennung eines Teilwertes (einer Bedeutung) als real-existent, "parts of sentences ... considered as referring to things" (p. 154); *Zitieren* ist das Einbeziehen von Teilen oder Ganzen in ihrer Originalform in einen "neuen" bzw. anderen Satzausdruck; *Substantivierung* ist das Verwandeln von Noch-nicht-Namen in Satzteile unter Einbau in satzgemässe Formgebung. Damit ergibt sich ein Durchblick, der Vorkommen von Textstücken vereinigt, die linguistischerseits nicht immer so gesehen und erfasst werden: nämlich Wörter als Wörter mit neuem (z.B. Zitat-)Bezug, Sätze als Satzteile und Aussagen in Geltung als zumteil mehrstufige Wörter. Das zeigt, wie hier die sogenannte Funktion, genauer der Wille zur *Elementverwendung* und *Ausdrucksbildung*, Elemente verschiedenen Umfangs vom Affix (p. 166) bis zum mehrgliedrigen Satz übergreift, und als entscheidende Struktur tritt hervor: dass man sich beim Sprechen auf etwas – eine "Tatsache dass...", "as for..." (p. 163) – beziehen will, um darüber oder dazu noch etwas (aus-)zu sagen. Und anscheinend ist diese Bezugnahme daran gebunden und davon abhängig, dass ihr Symbol nur Teil (Name, Nennung, Wort) ist im Gegensatz zu einem anderen und Anderswertigen, das Mehr-als-Teil (Aussage, Satz) ist. Die Erstellung eines Modells für die Verwandlung von und zu derartigen Satzteilen mittels CHOMSKYscher Transformationsregeln zeigt form- und inhaltsstrukturell, wie man aus *sources* oder *original strings* (p. 173) detaillierte mehrstufige Namen gewinnt: Zugleich ein Beispiel dafür, dass man sich bald sogar über logischen Voraussetzungen in der Grammatik wird Gedanken machen dürfen, vielleicht allerdings ebenso auch über grammatische Voraussetzungen für die Ausbildung von logischen Symbolismen. *Kombinierbarkeit* und *Umformbarkeit* wird man dafür als die leerste (formalste) Voraussetzung noch vor aller Zweckrichtung einer natürlichen oder künstlichen Sprache ansehen, auch wenn sie nur in *Zweckentsprechung* zu einem inhaltlichen Bedürfnis (Motivation) existent werden kann.

In inhaltlicher Hinsicht: Bei allen drei behandelten Punkten sind Weisen und Veränderungen der semantischen Bezugnahme dargestellt. Die *reification* wirkt ontisierend, das Zitat macht Sprachinhalte oder Sprachformen zum Bezugspol, die Substantivierung ist die grammatische Form einer derartigen Behandlungsstufe. Dabei verbleibt man gedanklich bewusst in der *Ebene der Symbolisierung* (Sprache), in den sprachlichen Formen und Prozeduren der Ausdrucksbildung, kann jedoch hiernach die Frage nach dem Verhältnis von *Symbolisierung und Realität* wieder neu und mit bewussterer Schärfe stellen. Denn alle mögliche oder ungerechtfertigte Verdinglichung *per signum* besagt nichts gegen reelle Onta als Bezugs- und Erfahrungskorrelate, entbindet auch nicht davon, sich um die Vorhandenseinsart der jeweils symbolisierten Sorte von Wirklichkeit gesondert und unter möglichster Absehung von Symbolisierungen zu kümmern: wird doch alles an "Welt" im Symbol immer nur "neutralisiert" getroffen, selbst wenn man sie zur Verifizierung der Symbole hinzu-fordern muss (Quine p. 11). Analog zeigt der mögliche Grad an Künstlichkeit (p. 185) die verschiedene Symbolisierung von Behandlungs- oder Abstraktionsstufen an: genau das aber wird ausschliesslich durch das Vorhandensein einer Sprache möglich, die mittels Elementkombinationen "neue" Klassifikationsmehrheiten – in Namen und Aussagen – zu bezeichnen erlaubt.

So gesehen bilden die vorgeführten Nominalisierungstypen (p. 185) einen durch seine logischen Konsequenzen aufschlussreichen Beitrag zu einer Linguistik, die sich auch als Analyse der in der Menschensprache angelegten Möglichkeiten versteht. Das unsere Sprache ausmachende Symbolisierungsverfahren erscheint als ein Verfahren bzw. als eine *Menge von geregelten Prozeduren*, worin feststehende *Einheiten* (oppositive Signale) mit festen oder variablen *Bezugswerten* (Bedeutungen) nach usus bedingten *Regeln* benutzt werden, die vom Deskribenten her hierarchisierbar sind und eine Sinn- oder Bezugsstufung ("Abstraktion") durch ein Operieren mit wechselnden Symbolteilen (Wörtern, Namen) und Symbolganzen (Sätzen, Aussagen) erlauben. Die *Grammatik* wird zur Beschreibung der Anwendung dieser je systeminternen (sprachbestimmten) symbolischen Technik; das macht die Frage nach dem einzelsprachlichen Wie (spezielle Grammatik) ebenso wichtig wie das Achten auf tatsächliche Gemeinsamkeiten (Allgemeine Grammatik) in den Sprachprozessen.

Münster, Westfalen, Germany

CHAPTER 11

ON THE LOGIC OF PREFERENCE AND CHOICE

H. S. HOUTHAKKER [1]

1. Preliminaries

Two recent monographs by Scandinavian logicians ([4] and [16]) testify to a widening interest in a field that has long been the preserve of economists, except for occasional forays by psychologists and mathematicians. We may expect much good from such a fresh approach whose objectives, mostly located in the realm of ethics, are evidently much broader than the necessarily narrow ones of economics and psychology. It would be unfortunate, however, if these new investigations were to be pursued in isolation, without drawing on the valuable insights obtained in connection with specific applications. This danger is not imaginary in view of the wide gaps that now separate the various disciplines. In the nineteenth century it was not uncommon for one man to be versatile both in philosophy and in economics; indeed there was then no clear boundary between the two domains. As both became increasingly technical, however, communication became more difficult. The philosophy of science, which might have been expected to facilitate such communication, has in fact confined itself almost entirely to physics, a discipline that for all its importance and spectacular successes is hardly representative even of the natural sciences, let alone of science in general.[2]

[1] The author gratefully acknowledges the useful comments made by Professor Kenneth J. Arrow on an earlier version of this paper. The latter is not responsible for any remaining errors.

[2] A notable exception is the valuable little book by Bocheński [2]. His inclusion of economics among the historical sciences, however, is open to question.

The present note is a modest effort to report on some contributions by economists to the logic of preference and choice that appear worthy of the attention of philosophers. In keeping with this aim it concentrates on the more formal aspects rather than on the context in which these contributions emerged, but a brief discussion of this context will be found in section 4. The concluding section 5 is devoted to some methodological points.

2. Some questions of definition

The terms "preference" and "choice" are often employed loosely (not least by economists) and it is therefore necessary to give some precision to their use in what follows. Since they are of a fairly elemental nature it is hard to give a helpful definition in even more primitive terms. Thus we are not much enlightened by the explanation that if a person prefers a to b he likes a better than b. On the other hand it is also unsatisfactory to leave these two terms completely undefined and merely to state certain rules for their manipulation. By way of informal clarification it may therefore be said that the term "preference" will refer to certain features of a person's state of mind, and the term "choice" to a person's overt act of a certain type. Furthermore it is basic to the following account that preference if related to choice as the possible to the actual. A person prefers a to b if, when confronted with a choice between a and b, he chooses a. Preference, therefore, is potential choice.

Unfortunately we cannot simply reverse this relation and assert that choice is realized preference. There is the phenomenon of random choice, which we observe, for instance, if a traveller abroad has to choose a meal from a menu of which he does not understand anything. To interpret such a choice in terms of underlying preferences would be either wrong or tautological. A minimum of knowledge has to be present, but the mere presence of some degree of uncertainty concerning the objects of choice does not make the notion of preference inapplicable.

Clearly nothing very interesting can be said about completely random choice except perhaps that it obeys the laws of probability, and we shall confine ourselves here to choice based entirely on preferences, or *preferential choice*. A more complete theory would integrate random and preferential choice, but it is still in its infancy[3]. Here we shall attempt to axiomatize the concept of preferential choice without reference to the concept of preference, and then relate these axioms of choice to the axioms of preference, which are much better known.

It may be asked why there is this apparent asymmetry between choice and preference; more specifically why we have to speak of preferential choice but not (say) of selective preference. The reason is that preference has been interpreted in terms of potential rather than actual choice. For this interpretation it does not matter whether the hypothetical choice is open to the individual concerned. Thus a person may well say that he prefers Napoleon to Hitler even though he will never have to choose between the two. Such nonoperational preferences are to some extent a counterpart to nonpreferential choices.

3. The axioms of preference and of choice

We consider a set X of distinct objects $x_1, x_2, \ldots,$ not necessarily finite or denumerable. On X we define a binary relation P, whose arguments are the elements of X. The expression "$x_1 Px_2$" means that x_1 is preferred to x_2. The relation P satisfies the following well-known axioms, common to all ordering relations:

P1. not $x_1 Px_1$ (irreflexivity)
P2. x_1Px_2 implies (not x_2Px_1) (antisymmetry)
P3. (x_1Px_2 and x_2Px_3) implies x_1Px_3 (transitivity)

As is well known either the first or the second of these axioms can be deduced from the remaining two.

Further axioms for the relation P can be stated according to the specific nature of the objects $x_1, x_2, \ldots,$ but we shall not do so in this section since it is concerned only with general characteristics of preference and choice. There is one axiom, however, which appears to be valid for all preference orderings, although it does not hold for all ordering relations. Let us write "not x_1Px_2" as "$x_1\bar{P}x_2$", where $\bar{P}$ consequently stands for "is not preferred to". It

[3] An interesting attempt was made by Davidson and Marschak [3].

follows from axiom P1 that $\bar{P}$ is reflexive, and from P2 that it is connected; P2 can therefore be replaced by

P2*. $x_1\bar{P}x_2$ or $x_2\bar{P}x_1$

even though P itself need not be connected. The additional, apparently general axiom we have in mind asserts the transitivity of $\bar{P}$:

P3*. $(x_1\bar{P}x_2$ and $x_2\bar{P}x_3)$ implies $x_1\bar{P}x_3$

It can easily be verified that P2 and P3* together imply P3, but P2 and P3 do not imply P3*. Among the ordering relations that satisfy P3* is "greater than", among those that do not is that of ancestry. In this paper we shall not use P3*, except briefly at the end of this section, because its consequences for choice have not yet been fully explored.

The axioms of choice require some preliminary discussion. Let the set X have subsets $X_1, X_2, \ldots$, which may or may not overlap. We define choice as a set function $\hat{x}(X_j)$, which allocates to each subset X_j a unique element $\hat{x}_j$, the "chosen" element. The first axiom of choice is evidently

C1. $\hat{x}_j \in X_j$ (only elements of X_j can be chosen)

It is to be noted that unlike the elements $x_1, x_2, \ldots$ the functional values $\hat{x}_1, \hat{x}_2, \ldots.$ need not be distinct: the same element may be chosen from different subsets to which it belongs.

It is more difficult to find an axiom that characterizes choice as preferential. Samuelson [9] and [10], who was the first to explore this problem (in a particular context, to be discussed in Section 4 below) considered the elements $\hat{x}_1$ and $\hat{x}_2$ chosen from two different but overlapping subsets X_1 and X_2. Suppose $\hat{x}_1$, which by C1 must belong to X_1, also belongs to X_2, but is not identical with $\hat{x}_2$. Then $\hat{x}_2$ must have been preferred to $\hat{x}_1$, for since $\hat{x}_1$ belongs to X_2 it could have been chosen instead of $\hat{x}_2$, but was not. Hence $\hat{x}_2$ is "revealed to be preferred" to $\hat{x}_1$. But if $\hat{x}_2P\hat{x}_1$, then $\hat{x}_2$ cannot belong to X_1, for if $\hat{x}_2$ had been available it would have been chosen instead of $\hat{x}_1$. This argument suggests the following axiom, known as the "weak axiom of revealed preference":

C2. not $(\hat{x}_1 \neq \hat{x}_2$ and $\hat{x}_1 \in X_2$ and $\hat{x}_2 \in X_1)$

where $\hat{x}_1$ and $\hat{x}_2$ appear symmetrically.

To see how C1 and C2 fit in with the axioms of preference we must first define choice in terms of preference:

Def. I. "$x_1 = \hat{x}(X_1)$" is equivalent to "$x_1 \in X_1$ and $x_1 P x$ for all $x \neq x_1$ such that $x \in X_1$"

Since C1 is part of this definition we need only verify that $\hat{x}(X_1)$ thus defined satisfies C2. This is already clear from the informal reasoning that justified the introduction of C2; more formally suppose that $\hat{x}_1 \in X_2$ and $\hat{x}_2 \in X_1$. Then by Def. I it must be true that either $\hat{x}_1 = \hat{x}_2$ or ($\hat{x}_1 P \hat{x}_2$ and $\hat{x}_2 P \hat{x}_1$), but the latter alternative is excluded by P2.

It will be noticed that P3 has not been used so far, which may be considered an indication that C2 does not completely characterize preferential choice. That this is indeed so can be seen by considering choice from three or more subsets. Let $\hat{x}_1 \in X_2$ as before, and also $\hat{x}_2 \in X_3$. From this we infer that $\hat{x}_2 P \hat{x}_1$ and $\hat{x}_3 P \hat{x}_2$, hence by P3 that $\hat{x}_3 P \hat{x}_1$. It is not necessarily true, however, that $\hat{x}_1 \in X_3$, so $\hat{x}_3$, unlike $\hat{x}_2$, is only "indirectly" revealed to be preferred to $\hat{x}_1$. All the same the assumption of preferential choice would be contradicted if $\hat{x}_3 \in X_1$. Thus we arrive at the revised axiom, essentially due to Von Neumann and Morgenstern [15] and also proposed by Ville [14] and Houthakker [5]:

C2*. not ($\hat{x}_1 \neq \hat{x}_2$ and $\hat{x}_2 \neq \hat{x}_3$ and .... and $\hat{x}_{n-1} \neq \hat{x}_n$ and $\hat{x}_2 \in X_1$ and $\hat{x}_3 \in X_2$ and .... and $\hat{x}_n \in X_{n-1}$ and $\hat{x}_1 \in X_n$),

of which C2 is a special case. It has already become clear that if $\hat{x}(X)$ satisfies Def. I it also satisfies C2* by virtue of P3. The axioms of preference, together with Def. I, therefore imply the axioms of choice. C2* has become known as the "strong axiom of revealed preference".

To consider the converse problem we have to define preference in terms of choice:

Def. II. "$x_1 P x_n$" is equivalent to "there exist admissible subsets $X_1, X_2, \ldots, X_{n-1}$ such that $x_1 = \hat{x}_1$ and $\hat{x}_2 \in X_1$ and $\hat{x}_3 \in X_2$ and .... and $\hat{x}_{n-1} \in X_{n-2}$ and $x_n \in X_{n-1}$."

At first sight this definition may seem unnecessarily complicated, for we have already suggested in section 2 that "$x_1 P x_n$" means that x_1 will be chosen out of the set whose only elements are x_1 and x_n. The important word in the definition, however, is "admissible". Which subsets are admissible depends on the specific subject matter to which the formal theory is applied. Thus we shall see in the next section that in the economic theory of consumer's choice the only admissible subsets are certain convex polyhedra in n-dimensional Euclidean space. Sets consisting of only two points are not convex and therefore not admissible in this theory.

There is no difficulty in showing that if P is defined by Def. II then C1 and C2* ensure the satisfaction of P1, P2 and P3. To prove P3, for instance, we note only that the two chains of admissible subsets whose existence defines respectively $x_i P x_j$ and $x_j P x_k$ link into each other with the condition $x_j \neq \hat{x}_j$, and can therefore be combined into a single chain defining $x_i P x_k$.

It will be apparent from this informal discussion that the axioms of choice and those of preference are equivalent [4]. There are, however, a number of related problems which may be briefly mentioned at this stage, in particular those concerning the concepts of indifference and utility. Indifference is a relation between objects of choice defined by

Def. III. "$x_1 I x_2$" is equivalent to "$x_1 \bar{P} x_2$ and $x_2 \bar{P} x_1$".

The properties of this relation follow from the axioms of preference. P1 and P2 ensure that the definition is not self-contradictory, while the reflexivity and symmetry of indifference are entailed by Def. III itself. The matter of transitivity is less straightforward: "$x_1 I x_2$ and $x_2 I x_3$" is equivalent to "$x_1 \bar{P} x_2$ and $x_2 \bar{P} x_1$ and $x_2 \bar{P} x_3$ and $x_3 \bar{P} x_2$", but P3 does not enable us to conclude from this that "$x_1 \bar{P} x_3$ and $x_3 \bar{P} x_1$". The latter conclusion would be warranted, however, by P3*, mentioned earlier but not used so far. If P3* is postulated, therefore, I has all the characteristics of an equivalence relation.

[4] For more details see Uzawa [12] [13] and Schoenfeld [11]. The first-mentioned of these articles is based largely on the present author's unpublished lectures given at the University of Tokyo in 1955.

A further important consequence of P3* is the following

THEOREM. *($x_1 P x_2$ and $x_2 I x_3$) implies $x_1 P x_3$.*

The proof is immediate: if the subscripts 2 and 3 are interchanged P3* can be written as

$$\text{not } (x_1 \bar{P} x_3 \text{ and } x_3 \bar{P} x_2 \text{ and } x_1 P x_2)$$

and hence as

$$(x_1 P x_2 \text{ and } x_3 \bar{P} x_2) \text{ implies } x_1 P x_3$$

from which the theorem follows upon replacement of "$x_2 I x_3$" by its definition. In the literature this theorem, and also the transitivity of indifference, have usually been introduced as separate postulates.

The difficulties with the indifference concept in the theory of choice result from the fact that while choices may "reveal" preferences, they can only reveal indifference in a very indirect way, if at all. Even in the theory of preference Def. III does not tell us whether there are any pairs of indifferent objects; thus if P is connected there will not be any. In fact, as Little [8] and others have shown, it is quite possible to dispense with the indifference concept altogether.

Much the same applies to the concept of utility, at least in its simpler (and hence more useful) interpretations. The explanation of choice in terms of utility is much older than the explanation in terms of preferences, for it was not until the advent of modern logic that relations began to be taken seriously. Even now it is sometimes argued that if x_1 is preferred to x_2, then x_1 must contain more of something (namely utility) than x_2. This argument is impeccably tautological provided nothing is assumed about utility, but as soon as one assumes utility to be (for instance) a measurable one-dimensional magnitude, any new conclusions become arbitrary and the whole argument a source of needless confusion. This does not mean that in the modern theory of choice there is no room for the utility concept, which is more amenable to mathematical analysis than the preference concept, but it does mean that assumptions about utility should not be introduced *ad hoc*, preference being primordial.

As a final comment on the axioms we shall discuss a property of preferential choice which has often been regarded as particularly indicative of rationality. This is the so-called "independence of irrelevant alternatives" first formulated by Arrow [1]. Suppose that an individual chooses x_1 if the set of alternatives open to him is X_1. Now let X_1 be reduced by omitting from it the subset $X_1{}^*$, which does not include x_1. Then the rational individual will presumably still choose x_1 out of the new set of available alternatives $(X_1 - X_1{}^*)$; his choice will be independent of the irrelevant alternatives contained in $X_1{}^*$.

An example from politics may illustrate Arrow's axiom and show at the same time that it, and indeed the whole idea of explaining choice from preference only, is not as obvious as it may seem at first sight. Consider an Englishman who favors the Liberal party. If in his constituency there is a straight choice between the Liberals and the Conservatives, or between the Liberals and Labour, he will vote Liberal. Now suppose all three parties appear on the ballot. According to the axiom he should still vote Liberal, for the new alternative was previously dispreferred and is therefore irrelevant. In reality, of course, a man's vote may depend not only on his first preference but also on his assessment of the likely outcome of the election. Thus our Englishman, arguing that the Liberals have no chance of winning anyway, may decide to vote Conservative in order to keep out Labour, whom he likes even less.[5] This is not consistent with a simple preference explanation, but it is not a specific objection to Arrow's axiom, which has been instrumental in clarifying decisions in a variety of context.

It will now be shown that Arrow's axiom, which we may put formally as

C3. If $x_1 = \hat{x}(X_1)$ and $X_1^* \subset X_1$ and $x_1 \notin X_1^*$, then $x_1 = \hat{x}(X_1 - X_1^*)$,

is equivalent to the weak axiom of revealed preference C2. First we prove that C2 implies C3. Put $X_2 = (X_1 - X_1^*)$ and note that

[5] In this example the set $(X_1 - X_1^*)$ is assumed to be available before the set X_1, rather than after, but this does not matter because Arrow's axiom, like all the axioms discussed in this paper, is of a static nature: "before" and "after" are dynamic notions which do not apply in this context.

$x_1 \in X_2$ and $\hat{x}_2 \in X_1$, since $(X_1 - X_1^*) \subset X_1$; hence it is not possible that $\hat{x}_2 \neq \hat{x}_1$. To prove the converse, we now put $X_2 = X_1 - X_1^* + X_2^*$, where X_2^* and X_1 are disjoint. If $\hat{x}_2 \in X_2^*$, then $\hat{x}_2 \notin X_1$, hence C2 is satisfied. On the other hand if $\hat{x}_2 \notin X_2^*$ then $\hat{x}_2 \in (X_1 - X_1^*)$, hence by C3 we have $\hat{x}_2 = \hat{x}(X_1 - X_1^*)$, so we have to prove that $\hat{x}_1 \notin X_2$ or $\hat{x}_1 = \hat{x}_2$. Suppose first that $\hat{x}_1 \in X_2$; then $\hat{x}_1 \notin X_1^*$, since $X_1^* \not\subset X_2$. Also $\hat{x}_2 \notin X_1^*$, hence $\hat{x}_2 = \hat{x}(X_1 - X_1^*)$, but $\hat{x}_1 = \hat{x}(X_1 - X_1^*)$, so $\hat{x}_1 = \hat{x}_2$. Finally let $\hat{x}_1 \neq \hat{x}_2$; then $\hat{x}_1 \neq \hat{x}(X_1 - X_1^*)$, hence $\hat{x}_1 \in X_1^*$, which means that $\hat{x}_1 \notin X_2$. This completes the proof of equivalence.

4. The economic context

Reversing the historical development we shall now discuss the interpretation of the axiomatics of preference and choice in the economics of consumer demand. The problem there is to find a theoretical framework for the analysis of consumers' expenditure. Such a frame work should be formulated in terms of the data to which the theory is applied. In the case of consumer demand these data are observations on the prices and quantities of various commodities, and the main purpose of the theoretical framework is to translate propositions about preferences (which in general are not directly observable) into propositions about prices and quantities. The theory refers mostly to a single individual even though the applications of ultimate interest are to the aggregate behavior of large groups of people.

For the individual consumer prices are regarded as given parameters; he is assumed to decide on the quantities he will buy on the basis of his preferences and of a budget constraint, according to which the total cost of his purchases cannot exceed a given sum of money (his income). The principal question then is how, for fixed preferences, the quantities bought will vary in response to changes in prices and income. In mathematical terms prices and quantities can be conceived as components of n-dimensional vectors in Euclidean space; thus a particular price constellation can be represented as $p_1 = \{p_1^1, p_1^2, \ldots, p_1^n\}$, where the superscripts designate particular commodities. Similarly the quantities bought when prices are p_1 can be represented by $q_1 = \{q_1^1, q_1^2, \ldots, q_1^n\}$.

Total expenditure can then be written as a vector product

$$p_1 \cdot q_1 = p_1^1 q_1^1 + p_1^2 q_1^2 + \ldots + p_1^n q_1^n.$$

Similar expressions can be formed where prices and quantities do not belong to the same situation; thus $p_1 \cdot q_2$ is the cost of the goods bought in situation 2 at the prices prevailing in situation 1.
The budget constraint becomes

$$p_i \cdot q_i \leq \mu_i$$

where μ_i is income in the i^{th} situation (i arbitrary). In practice the inequality sign can be replaced by an equality sign since savings can be included among the n commodities. Together with a restriction of the components of the vector q_i to nonnegative values the budget constraint defines a convex polyhedron in n-dimensional space whose vertices are the coordinate axes and whose top is the origin. All polyhedra of this type which correspond to positive prices and income are "admissible subsets" in the sense of Def. II in Section 3.[6]

Let us now consider the weak axiom of revealed preference C2. The "chosen element" $\hat{x}$ of the general formulation now becomes the commodity bundle q_i bought in the i-th price-income situation. The expression "$\hat{x}_2 \in X_1$" is translated as "$p_1 \cdot q_2 \leq p_1 \cdot q_1$", which means that in situation 1 the commodity bundle q_2 *could* have been bought (since it cost no more than the bundle q_1 which *was* bought), but in fact was not bought; hence q_1 is "revealed to be preferred" to q_2. From this (assuming, as in general we may, that $q_1 \neq q_2$) the axiom infers that $x_1 \notin X_2$, or in the present context,

$$p_2 \cdot q_2 < p_2 \cdot q_1.$$

The common sense of this is that, since q_1 is revealed to be preferred to q_2, q_1 will be bought instead of q_2 whenever q_1 is available; hence in the situation where q_2 was actually bought, q_1 cannot have been available, and this can only mean that its cost at prices p_2 was greater than the available income $\mu_2 = p_2 \cdot q_2$. The proposition

$$p_1 \cdot q_2 \leq p_1 \cdot q_1 \text{ implies } p_2 \cdot q_2 < p_2 \cdot q_1$$

[6] In some economic contexts other subsets may also be admissible, but the above describes the standard case of consumption theory.

is the original form of the weak axiom as proposed by Samuelson [9].

An immediate consequence of the weak axiom is a refined version of the "law of demand", which is fundamental to consumption theory and indeed to economics in general. Suppose the situations 1 and 2 are such that $p_1 \cdot q_2 = p_1 \cdot q_1$; subtracting this from its consequence $p_2 \cdot q_2 < p_2 \cdot q_1$ we obtain

$$(p_2 - p_1) \cdot q_2 < (p_2 - p_1) \cdot q_1$$

or more symmetrically

$$(p_2 - p_1) \cdot (q_2 - q_1) < 0$$

which means that in total price variations lead to quantity variations of opposite sign. In particular if only one price (say p^1) changes, then

$$(p_2^1 - p_1^1) \cdot (q_2^1 - q_1^1) < 0$$

so that the quantity bought of the commodity considered must be larger the lower its price, provided that $p_1 \cdot q_2 = p_1 \cdot q_1$. Without the latter condition, which implies that the income μ_2 is adjusted in such a way that the consumer could still buy q_1 in situation 2 if he wanted to, the law of demand need not hold.

It would lead us too far to discuss the economic interpretation of the strong axiom C2*. On the observational level it leads to certain symmetry conditions concerning the effect of a change in the price of one good on the quantity bought of another good. In principle it can also be used to reconstruct the preference ordering underlying a consistent system of price-quantity relations. If the strong axiom (and hence the weak axiom) is found to be satisfied in an empirically given set of price-quantity data, then that set can be interpreted as reflecting a single underlying preference ordering. In this way it is possible to test, for instance, whether there are differences in preferences among different countries, that is to say whether the differences in consumption patterns among countries are attributable entirely to differences in prices and income levels [7]. In this test countries are treated as individuals, which is legitimate because the axioms (as distinct from their provisional interpretation) do not say anything about the origin of the preference ordering.

5. Some methodological remarks

It seems appropriate in the present volume to terminate this essay in applied logic with some comments on methodological aspects.

First of all, the explanation of choice from preference should be viewed with the clarification of section 2 in mind. There choice was described as an overt act, preference as a state of mind. By and large, overt acts are more accessible to scientific observation than states of mind, although this certainly does not mean that states of mind are less important. Nevertheless the increased emphasis of economic theory on propositions with directly observable implications has been an essential factor in the transformation of economics that has taken place in recent decades. In the nineteenth century economics was mostly concerned with the derivation of normative conclusions from abstract assumptions, which were regarded as valid *a priori*. The problem of testing either assumptions or conclusions therefore hardly arose. It did not arise any more for the minority of economists belonging to the historical and institutional schools, which were averse to generalizations and theory and concentrated on description. Until these two extremes of rationalism and empiricism were seen to be equally untenable there was not much scope for the development of economics as a positive science.

This development had to await the elaboration of modern statistical methods, the tools *par excellence* of the inexact sciences. Without the theory of statistical inference, a product of the twentieth century, the course not only of economics, but also of psychology, biology, astronomy and many other disciplines, would have been very different. Once philosophers of science overcome their preoccupation with physics (where statistical methods happen to be less important) they may yet discover that the truly significant revolution in twentieth century science is neither the theory of relativity nor the uncertainty principle, but the systematic integration of theory and observation that mathematical statistics is making possible.

But this is a digression. What matters for our present purpose is the reorientation of theoretical work that is required by the integration just mentioned. To the contemporary researcher a theory is of ultimate interest only to the extent that it leads to testable statements about observations (and for this recognition, which now seems obvious, we should give due credit to the philosophers of science). The more directly it yields such statements, the better the theory. From this point of view the "revealed preference" approach is a great advance over earlier formulations.

More interesting, perhaps, is another feature which can be fully appreciated only in the light of the history of consumption theory,[7] namely the minimal character of the assumptions concerning preferences. In the early years of the theory (roughly the second half of the nineteenth century) the standard assumption was that the consumer attached a certain measurable utility to the commodities he might acquire, and that his actual choice was the result of making total utility as large as possible. Later it was realized that the measurability of utility was irrelevant, which led to formulations in terms of "ordinal" utility, from which the same observable conclusions could be derived. Once the necessity of assumptions began to be questioned Occam's razor came to be wielded with ever increasing vigor. Some economists even proposed to abandon all references to the consumer's state of mind and to assume nothing but the existence of price-quantity relations, but this was clearly going too far, for any assumptions about these relations would then become completely gratuitous. So the question arose what are the weakest assumptions from which could be derived conclusions such as those of section 4. The answer has already been given, namely that a consistent preference ordering has to exist. Since this assumption is both necessary and sufficient, it is indeed the weakest that can be made without sacrificing important conclusions. The whole development is a remarkable instance of the search for economy of thought emphasized around the turn of the century by Mach and Poincaré.

Now it might be argued that the pursuit of minimal assumptions is after all of merely esthetic interest, and that it involves only harmless tautological reasoning to proceed from hypotheses that are sufficient without being necessary. This argument unfortunately

[7] See [17], [18] and [6].

ignores the psychology of research. Once "superfluous entities" are introduced scholars will start discussing them, often without any consideration of their usefulness. Thus many books and articles have been written about the measurability of utility at the expense of more fruitful analyses.[8] It is true, on the other hand, that it is sometimes more important, especially in the exploratory stages of a problem, to find productive assumptions than to find minimal assumptions.

Finally it is worth noting that the reconsideration of the theory of choice described above led economists naturally to adopt symbolic logic where specifically mathematical methods, especially the calculus of maxima and minima, had ruled supreme. Although the applications of logic to economics have so far remained on an elementary level, this area should nevertheless be counted as one where contemporary logic has proved its worth.

Harvard University, Cambridge, Mass., USA

Bibliography

[1] K. J. Arrow, *Social Choice and Individual Values* (New York, Wiley 1951).

[2] J. M. Bocheński, *Zeitgenössische Denkmethoden* (Bern, Francke 1954).

[3] D. Davidson and J. Marschak, Experimental Tests of a Stochastic Decision Theory, in *Measurement: Definitions and Theories* ed. by C. W. Churchman and P. Ratoosh (New York, Wiley 1959).

[4] S. Hallden, *On the Logic of Better* (Lund, Gleerup 1957).

[5] H. S. Houthakker, Revealed Preference and the Utility Function, Economica, New Series **17** (1950).

[6] H. S. Houthakker, The Present State of Consumption Theory, Econometrica **29** (1961).

[7] H. S. Houthakker, Some Problems in the International Comparison of Consumption Patterns, in *L'évaluation et le rôle des besoins ....* ed. by R. Mossé (Paris, CNRS 1963).

[8] Actually the measurability of utility is relevant in a probabilistic context which has not been considered here.

[8] I. M. D. LITTLE, A Reformulation of the Theory of Consumer's Behaviour, Oxford Economic Papers **1** (1949).
[9] P. A. SAMUELSON, A Note on the Pure Theory of Consumer's Behavior, Economica, New Series **5** (1958) (with addendum in same vol.).
[10] P. A. SAMUELSON, *Foundations of Economic Analysis* (Harvard U.P. 1947).
[11] P. SCHOENFELD, Grundzuege der Theorie der faktischen Praeferenz, Zeitschrift für Nationalökonomie **22** (1962).
[12] H. UZAWA, Note on Preference and Axioms of Choice, Annals of the Institute of Statistical Mathematics **8** (1956).
[13] H. UZAWA, Preference and Rational Choice in the Theory of Consumption in Proceedings of a Symposium on Mathematical Methods in the Social Sciences (Stanford U.P. 1960).
[14] J. VILLE, Sur les conditions d'existence d'une ophélimité totale et d'un indice du niveau des prix, Annales de l'Université de Lyon **9** (1946).
[15] J. VON NEUMANN and O. MORGENSTERN, *Theory of Games and Economic Behavior* (Princeton U.P. 1944).
[16] G. H. VON WRIGHT, *The Logic of Preference* (Edinburgh U.P. 1963).
[17] H. O. A. WOLD, A Synthesis of Pure Demand Analysis, Skandinavisk Aktuarietidskrift **26–27** (1943–44).
[18] H. O. A. WOLD, *Demand Analysis* (New York, Wiley 1953).

CHAPTER 12

SENSE, DENOTATION, AND THE CONTEXT OF SENTENCES

FRANCESCA RIVETTI BARBÒ

0. This research falls within the field of semantics. Starting from an inquiry into some of Frege's, Church's, Russell's and Quine's theories (at Nos. 1–9; see hereafter No. 0.2[1]) its main results give an outline of a theory of designation (summarized at Nos. 10–11; see No. 0.1).

Finally some correlated topics are briefly discussed (Nos. 12–13; see No. 0.3).

0.1 The chief result is that the meaning of words which is to be sought in the context of sentences is their designation, as distinct from their sense (No. 11). Thus before a sentence is asserted, expressions have only a sense (No. 10); according to the different kinds of word even senses (No. 10.1) as well as the ways of designating (No. 10.2) are of different kinds. The things spoken about – denoted through the arguments of sentences – are (following the terminology here adopted) the designata which verify (or falsify) sentences (No. 10.3).

0.2 These theses are proved and more extensively stated during the previous inquiry (No. 1–9).

The analysis of Frege's demonstration of his distinction between sense and denotation will draw us back to one of the fundamental principles of inquiry, as stated in *Grundlagen* (No. 1). First of all

[1] In numbering the sections, decimals are used for subdivision of wider parts; e.g., No. 1 is subdivided in Nos. 1.1, 1.2, and No. 1.2 in Nos. 1.21, 1.22, etc. Moreover the passage No. 2.1 may either be a part of or a comment on the passage No. 2. (this last one being analogous to the criterion of numbering used by Wittgenstein for the propositions of his *Tractatus*). This holds good also for further subdivisions, e.g for No. 2.12 in respect of No. 2.1 etc.

the proof of the distinction is put forth (No. 2) as it stands in both of Frege's writings (No. 2.1. and 2.2.) – as well as some of his further remarks (No. 2.3.). The examination of Frege's arguments (No. 3) – that is the search for the very basis of the above proof (No. 3.1) and the analysis of other of his teachings (No. 3.2) – points out that the proof rests on the very possibility of stating the relevant sentences. This result (see No. 3.3) leads us back to the above principle (stated by Frege himself) stressing the importance of the context of sentences (No. 4). This principle and Frege's well-known distinction (though he did not realize it) illuminate each other (No. 4.1 and No. 4.4): hence the meaning which is to be sought in the context of sentences is the denotation, as distinct from the sense (No. 4.2), more exactly the denotation of its subjects (Nos. 4.3 – 4.3.1).

By this conclusion (summarized at No. 5) we are driven to take into account the theory of descriptions (No. 6), because of its stressing the importance of contexts, although not agreeing with Russell's elimination of Frege's sense (No. 6.1). In some statements of *On Denoting* (No. 6.2) the main features of the theory are to be found at their very origin (No. 6.3); whereas Quine's restatement rectifies it (No. 6.4). Thus I point out that here, too, the primacy of the sentence-context and the distinction sense – denotation should illuminate each other (No. 6.5).

My aim is thus to specify – starting from the theory of descriptions so modified – the various kinds of sense and designation (No. 7) proper to quantifiers, variables, and bound variables (pronouns) (No. 7.1) and to the other parts of sentences (No. 7.2). Also for these, i.e. for names (No. 8.1) and for (singulary or n-ary) predicates (No. 8.2), the same approach helps to determine what their sense is (at Nos. 8.11 and 8.21 respectively) and how they designate (at Nos. 8.12 and 8.22). Designation is warranted by the context of asserted sentences (No. 9), both whether they are closed quantifying variables (No. 9.1) or are replacing them with constants (No. 9.2); although before their being asserted sentences also only express sense (No. 9.3.).

0.3 After summarizing (at Nos. 10–11) the above results (see No. 0.1), I open discussion on further topics (No. 12): phrases in "oblique use" (No. 12.1), nominata of names not occurring in sentences (No. 12.2), and finally the true and false as designata (No. 12.3). At the end a short remark about "ontology", as, perhaps correlated with the above semantics (No. 13).

1. I will analyse Frege's argument by which he proves his distinction between sense and what he called *Bedeutung* – the denotatum, or nominatum, or even designatum or reference.[2] The distinction is very well known and so is the puzzle – akin to the "paradox of analysis" [3] – in order to solve which it has been introduced; but, strangely enough, the argument itself does not seem to have been analysed as soundly as its importance would deserve.[4]

Yet the examination of this proof will throw new light on the very distinction by its recalling a principle stated years before, by Frege himself.

From the very beginning of his *Grundlagen der Arithmetik* he had stated that the meaning of words is to be sought in the context of sentences. At that time he used '*bedeuten*' without any further discrimination [5] (in this sense I shall use 'meaning' and its derivatives); later on he distinguished the sense from the *Bedeutung*, obviously taking this last one in a narrower sense (for which I shall use 'denotation' [6], But at this time Frege no longer toke

[2] G. Frege, [ÜSB] Über Sinn und Bedeutung, in Zeitsch. f. Phil. u. phil. Krit. n.s. **100** (1892) 25–50. English translations: by M. Black, in The Phil. Rev. **57** (1948) 209–230; repr. in P. T. Geach and M. Black, [Tr. Frege] *Translations from the Philosophical Writings of Gottlob Frege*, (Oxford 1952), pp. 56–78; and by H. Feigl, in H. Feigl and W. Sellars, [Readings] *Readings in Philosophical Analysis*, (New York), pp. 85–115. A previous short account was published in [FB] *Function und Begriff*, (Jena 1891), p. 31; English translation by P. T. Geach, in [Tr. Frege], pp. 21–42. For terminological remarks see footnote 6.

[3] See A. Church, Review of M. G. White and M. Black, in Journ. Symb. Log. **XI** (1946) 132–133: "The paradox of analysis is a special case of Frege's puzzle, and is to be solved in the same way" (*ibid.*, p. 133).

[4] In fact the distinction is generally taken as already established; see for instance the passages cited below at note 22.

[5] Frege points it out explicitly in [ÜBG] Über Begriff und Gegenstand, in Vierteljahrs. f. Wiss. Phil. **16** (1892) 192–205: p. 198; [Tr. Frege], p. 47.

[6] In expounding Frege's thought I generally follow M. Black's and P. T. Geach's translations (op. cit. in note 2), except in a few cases. So I use *to denote* as a translation for *bedeuten* and *bezeichnen* (since they are synony-

into account the principle stated in his *Grundlagen*; though it proves to be closely related to that distinction, as will be pointed out by the analysis of its very proof.

2. The difference of sense from denotation might be accepted on account of a given theory of knowledge. However this is not Frege's starting point, provided his proof of the distinction is grounded on the examination of linguistic signs and of their meaning.

It is important to notice that Frege's demonstration is concerned (in both writings) only with names, more exactly with proper names, "naming" (as he says in *Sinn und Bedeutung*) "a definite object and not a concept or a relation".[7]

2.1 In *Function und Begriff* the distinction is mentioned in connection with the polemical remark – against psychologists – stating that numerals are to be distinguished from what they stand for. Frege points out that '2', '1 + 1', '6 : 3',[8] all stand for the same thing; if you would object that 1 + 1 is a sum, whereas 6 : 3 is a quotient, the reply would be (according to him) that "different expressions correspond to different conceptions and aspects, but nevertheless always to the same thing".[9]

2.2 In *Sinn und Bedeutung* the proof of the distinction starts from the need of explaining what equality is ("is it a relation?"), or more exactly what entities are the ones related by an equality. This would give at the same time the reason why sentences stating identity, like '$a = b$', express (as Frege says) proper knowledge.

mous) and *denotation* for *Bedeutung* (of words), thus following CHURCH's suggestions (see his Review of M. Black's translation, in Journ. Symb. Log. **13** (1948) 152–153); hereby I use *denotatum* for *Bedeutung* (the object). For *Gedanke* see note 16. For *Vorstellung* I use *representation*: see No. 2.3. An alternative terminology can be found in R. CARNAP, [MN] *Meaning and Necessity*, (Chicago 1947), and in Feigl's translation (op. cit. in footnote 2). See Carnap's terminological remarks in [MN], note 1 at p. 97 and note 21 at p. 118.

For my own contentions I will use also to *designate*, to *name*, etc., but in slightly different senses; see footnote 59 and Nos. 10–11 and 12.2.

[7] [ÜSB], p. 26; [Tr. Frege], p. 57; [Readings], p. 86.

[8] Words written between inverted commas ' ' (and only these) are in material supposition (i.e. autonymous use): therefore "'dog' has three letters", is a true sentence. Whereas quotation marks " " are used here only to point out expressions, though leaving them in formal supposition.

[9] [FB] pp. 4–5; [Tr. Frege], p. 23.

The cognitive value of $a = b$ is different – and stronger – than that of $a = a$; so it is worthwhile stating the sentences: "the point of intersection of a and b is the same point of intersection of b and c" (provided that a, b and c are the median lines of a triangle) and "the morning star is the evening star", just because by these you say something more than a mere tautology. What is said by these sentences has a proper cognitive value if and only if the difference between 'a' and 'b' and between 'point of intersection of a and b' and 'point of intersection of b and c' etc. expresses a difference of "the mode of presentation" of the thing spoken about [10]; i.e. if each name, besides its denoting the same object denoted by the other name of the same pair, expresses a sense, different from the sense of the other name; so that each sense is one of the different "modes of presentation" of the same thing.

Frege points out that meaning is given to words in quite an arbitrary way; everyone could thus take "any arbitrarily producible event or object as a sign for something".[10] So, if the difference between 'a' and 'b' would concern only these signs, then the phrase '$a = b$' would regard only the signs themselves, inasmuch as these are taken as objects; and therefore this phrase would only say that 'a' and 'b' have both been arbitrarily chosen as names for one and the same object. Whereas generally by '$a = b$' we want to express proper knowledge, and not only a linguistic convention concerning signs.

To guarantee this cognitive value of statements of identity it is necessary to distinguish the thing for which the sign stands (the one *bedeutet* or *bezeichnet* by it), i.e. its denotatum or nominatum (the *Bedeutung* of Frege), from what the same sign expresses, i.e. its sense (*Sinn*). This is the proper proof of the distinction given by Frege.

2.3 Frege's further remarks on the same topic come after this argument, and for a good reason. In fact they do not give any new proof of the distinction itself, though better specifying what sense and denotation are, and thus throwing new light on their difference.

Frege goes on setting forth the difference between representation ("idea" in Geach's translation, "image in the psychological sense"

[10] [ÜSB], pp. 26–27; [Tr. Frege], pp. 56–57.

in Feigl's) and sense: the one is subjective, the other, on the contrary, cannot be "a part or a mode of the individual mind", because it may be "the common property of many".[11]

So the sense is to be distinguished (at least in direct speech) from the object spoken about; as this object is the denotatum (*Bedeutung*) of the name, sense is conceived by Frege as something which "lies in between" the thing – the object itself – and the fully subjective representation.[11]

Furthermore sense does "illuminate only a single aspect of the denotatum", and we will never get at a "comprehensive knowledge" of the named thing.[12] In a given language, or at least in a given context, each name should have a definite sense.[13] This does not involve however that a denotatum always corresponds to the sense, as there are names having sense but destitute of denotation;[13] however this is an anomaly of natural languages to be avoided in scientifical ones.[14]

These seem to be Frege's most important remarks concerning the "ordinary use" of words; the only one I want to take into account here. I will give just a hint to their "oblique use" later on (in this case, according to Frege, the denotatum is the very sense of the words).

3. Now I will analyse Frege's above mentioned proof of the distinction, in order to illuminate its theoretical foundation.

In both Frege's works concerning it – *Function und Begriff* and *Sinn and Bedeutung* – this distinction is proved by pointing out that as many different senses as there are names, are corresponding to only one denotatum.

In fact, if several non-synonymous names stand for the same object, it is plain that within the (generic) meaning of each name we must distinguish the sense expressed by the name from the denotatum for which it stands: i.e. the object it denotes through its sense.[15]

[11] [ÜSB], pp. 29–30; [Tr. Frege], pp. 59–60; [Readings], pp. 87–88.

[12] [ÜSB], p. 27; [Tr. Frege], p. 58.

[13] [ÜSB], pp. 27–28; [Tr. Frege], p. 58.

[14] [ÜSB], pp. 40–41; [Tr. Frege], pp. 69–70.

[15] Frege's example about the moon, its image in the object glass (the sense) and in the eye (the subjective idea) points out that the sense is the very means by which we get at the object [ÜSB], p. 30; [Tr. Frege], p. 60.

Not only the distinction, but its very necessity is so proved.

3.1 How may, however, the identity of the denotatum – concurrent to the plurality of different senses – be verified and explicity stated? It is obvious that, if this identity could not be verified, there would be no necessity of admitting the difference between denotation and sense, which has been hereby demonstrated. The reply will be given by a closer examination of Frege's above-mentioned reasoning.

We have seen that in *Sinn und Bedeutung* the identity of the denotatum is stated in true sentences, like "$a = b$", "the point of intersection between a and b is the same as the point of intersection between b and c" etc.: Hence the very fact of stating these true sentences assures that one and the same denotatum, which is therefore to be distinguished from the many different senses, is really given.

In *Function und Begriff* it might appear as if the stating of a true sentence could be avoided in order to make sure that '$1 + 1$', '$6 : 3$', '2' etc. denote the same denotatum, though expressing different senses. However the dispensability of stating the relevant sentences is due only to the fact that the equalities: "$1 + 1 = 6 : 3 = 2$" are so well known that it is useless to state them explicity; notwithstanding this, the names '$1 + 1$', '$6 : 3$' and '2' having the same denotation cannot be separated from the truth of the sentences "$1 + 1 = 2$" and "$6 : 3 = 2$". Whereas if the relevant sentences were either false or not well formed, or even not known as being true, the identity of the denotatum spoken about would not hold, or (in the last case) would not be acknowledged; so, as the denotata of '$6 : 3$' and of '$1 + 1 + 1$' are not the same, the sentence "$6 : 3 = 1 + 1 + 1$" is false; and I do not know whether '21,175' and '725×43' have the same denotatum, until I have verified the sentence "$725 \times 43 = 21,175$".

I do not want to question whether, in stating equalities, it is only after acknowledging the sameness of the denotatum that we can rightly affirm the relevant sentence as a true one; in fact this seems to me the normal process of knowledge, as it falls within the field of pragmatics (in Morris's sense) of language. Notwithstanding this, it is evident that stating that there is only one denotatum is explicit only when I affirm it (by saying for instance "$6 : 3 = 1 + 1$"), thereby acknowledging the truth of the relevant sentence.

So I would conclude – relying on the result of the foregoing analysis of Frege's argument, as well as on theoretical reasons – what follows: it is the possibility of formulating true sentences (stating equalities) which assures that the identity of each denotatum is concurrent to the diversity of senses of the names of a given set (like '$1 + 1$', '2', '$6 : 3$'). This is the fundamental basis of the proof.

3.2 We are driven in the same direction even by Frege's remarks on the connection between denotation and the truth-value of sentences; just the ones from which he starts establishing his well-known thesis on the denotation of sentences (I shall have a hint at it later on).

Here I recall only his stating that if one should be satisfied with the bare sense of the statement – i.e. with the not affirmed proposition (the *Gedanke* [16]) devoid of truth-value – he would be taking no account of denotation of the names there occurring. "Thus" as Frege concludes "it is the striving for truth which urges us to penetrate beyond the sense to the denotatum"; moreover "it is the denotatum of the name to which the predicate is ascribed or denied".[17]) Later on I shall point out that the denotatum here really relevant is just the one of names occurring as subjects, and this will lead us to important conclusions. Here I simply want to stress the fact that the advancement from the bare sense of sentences to their truth (or falsity) is conditioned by the acknowledgement of the presence of the denotata of the names occurring in them.

3.3 On this basis we can say that if 1., it is in the search of

[16] M. Black's translation is *thought*; H. FEIGL translates with *proposition*, following (see footnote in [Readings], p. 85) R. CARNAP, [MN] (cit. in footnote 6); A. CHURCH, Review of M. Black (cit. at footnote 6), prefers *proposition*. B. RUSSELL, [Principles] *The Principles of Mathematics*, (Cambridge 1903), App. A, translates with "propositional concept". It might seem strange that Frege uses 'Gedanke', while in German you can also use 'das Gedachte', thus mentioning the thought in objective sense.

[17] [ÜSB], pp. 32–33; [Tr. Frege], p. 63; for the translation see also [Readings], pp. 90–91.

truth that we want to take hold of the denotata, and if 2., it is of the denotata of the subjects that predicates are affirmed or denied, and besides, if 3., the applying or with-holding of predicates occurs only within sentences which can have a truth-value – their names having a denotation – and, 4., truth or falsity are to be found only in such statements; then it is clear that just in the context of sentences stated as true we have the assurance, because of names occurring in them, to have not only a sense but a denotation too.

This substantiates my above thesis as to the basis of the proof of the distinction sense-denotation – i.e. that it depends on the possibility of stating the relevant true sentences. Moreover those teachings will help me in going farther with the theory of denoting here advocated; firstly they will help in stressing the importance of the context of sentences in this connection (see Nos. 4, 4.1, 4.2); secondly they will give the guiding thread to specify how subjects denote within such contexts (see No. 4.3).

4. The said conclusion leads back to the principle stated by Frege himself in his *Grundlagen der Arithmetik* (1884), which Is hall call the principle of the primacy of the context: "never ask for the meaning of words in isolation, but in the context of a proposition".[18] The same statement we find later, strengthened perhaps, in Wittgenstein's *Tractatus*,[19] and recalled even by Carnap.[20]

4.1 When Frege established that principle he had not yet distinguished the denotation from the sense; [5] later on, when he stated this difference, he did not recall that principle.[21] Far from this, he simply extended his new distinction to the other parts of speech; thus he opened a new way, followed, later on, by Carnap.[22]

[18] G. FREGE, [GLA] *Die Grundlagen der Arithmetik* (Breslau 1884); English translation by A. AUSTIN (Oxford 1952) p. X. See also *ibid.*, § 62, p. 73.

[19] L. WITTGENSTEIN, *Tractatus Logico-Philosophicus* (London 1922), 2nd ed. 1933, Prop. 3.3.

[20] R. CARNAP, [MN] (cit. in footnote 2), p. 7; the text is quoted at footnote 64.

[21] M. DUMMETT, (being acquainted also with Frege's unpublished papers) notices that this statement "occurs in Frege's *Grundlagen* [. . .] and in no other of his writings" (Nominalism, in Phil. Rev. **65** (1956) 491–505).

[22] Carnap remarks that Frege's pair of concepts "coincides, in ordinary

(So Wittgenstein follows the first of Frege's suggestions; while Carnap follows his later teaching, though giving acknowledgement also of the importance of the context. But neither of them established – as far as I know – a thorough comparison with the other view).

Frege, immediately after demonstrating, by the said argument, that *names* have a denotation different from their sense, applied this distinction both to *sentences* (their sense is the non-asserted proposition, their reference the True or the False) and to *predicates*, taken as linguistic expressions, and relation-signs.[23] So all ex-

(extensional) contexts" with his own pair of "extension and intension" ([MN], p. 124); and this distinction applies, as is wellknown, to all the expressions he calls designators, i.e. to (declarative) sentences, individual expressions and predicators.

I would remark that Carnap does not point out that Frege, first of all, proves the distinction for proper names; in fact he says that "After having explained the distinction in a general way, Frege proceeds to apply it to sentences". Thus Frege's pair of concepts turns out as being construed in order to fit to names, predicates, and sentences (for the consequences of this view see below, footnote 24).

Carnap's exposition of Frege's "preliminary" explanation of the distinction consists in illustrating it by (Frege's) examples – without any further analysis of the proof. But notice that going this way Carnap is driven to remark that "For the reader [. . .] it is not so clear as for Frege himself what is to be understood by his two terms"; although, as Carnap acknowledges, "Frege assumes that he knows quite clearly what he means by 'sense' and 'nominatum'" ([MN], p. 120). Consequently Carnap concludes that "Frege's preliminary explanations of his terms are not sufficient as a basis for his results. In order to understand the specific sense in which Frege means his terms, we have to look [. . .] at the reasoning by which he reaches his results" ([MN], p. 120–121); here "Frege makes use of certain assumptions", which "can be formulated as principles of interchangeability" (*ibid.*).

It is plain that Frege's reasoning is considered by Carnap at a different point – a later one – from the one here analysed; and accordingly the results are different.

[23] For sentences see [ÜSB], pp. 32–36; [Tr. Frege], pp. 62–66. For predicates (taken in the linguistic sense) see [ÜBG], p. 198; [Tr. Frege], pp. 47–48. On this last point see the discussion between W. Marshall and M. Dummett in Phil. Rev. **62** (1953), **64** (1955), **65** (1956). See especially the argument, based on a manuscript of 1906, in M. Dummett, Note: Frege on Functions, *ibid.* **65** (1956) 229–230, and the passages quoted by E. D. Klemke, Professor Bergmann and Frege's "Hidden Nonimalism", *ibid.* **68** (1959) 507–

pressions, both single words and sentences, have a denotation as well as a sense; and the context of sentences would have no influence on the denotation of words.

On the contrary I would emphasize how both theses – the principle of the primacy of the context in the search for meaning, and the splitting of meaning in sense and denotation – do illuminate each other. Their connection will provide the very foundation of the whole semantics of language I wish to advocate.

4.2 To restate the principle of the primacy of the sentence-context, in order to make it conform to the distinction denotation-sense, it will be useful to recall what has been clarified so far about the denotation of expressions. As we saw sentences can be true (or false) if and only if the names there occurring do have a denotation; and conversely, we seek the denotation in order to affirm the truth of sentences; moreover the explicit assurance of the presence of only one denotatum for several names having different senses lies in the sentence (if a true one) stating their identity. Therefore we must conclude that, though words have a definite sense from the moment they are assumed as significant signs in a language, their denotation (i.e. the coming into act of the ability of names to denote the objects spoken about) is warranted just by the context of true sentences.

Hence the kind of meaning the determination of which is to be sought, according to the above principle, in the context of sentences is the denotation of words, not their sense. It follows that words do denote only as long as they are inserted in sentences.[24]

514: p. 510. For 'predicate' and 'relation-sign' as taken in the linguistic sense, see below, footnote 48.

[24] The view here advocated, that designation depends on the context of sentences, while words have a sense even in isolation, is a theoretical one (see below Nos. 10–11). (Moreover it concerns natural, and not formalised, languages: see below footnote 60).

Obviously the two questions – the theoretical and the historical, concerning Frege's interpretation – are independent; but I would suggest that the above analysis of Frege's argument leads to this theoretically important result (of No. 4.2).

Accordingly the distinction sense - denotation turns out to be akin to the distinction between *significatum* (i.e. Frege's "sense") and *suppositio*.

For the theory of *suppositiones* see J. M. Bocheński, *Formale Logik* (Freiburg-Muenchen 1957) pp. 186–199; E. A. Moody, *Truth and Consequence* *in Mediaeval Logic* (Amsterdam 1953) pp. 18–19; PH. BOEHNER, *Medieval Logic* (Manchester and Chicago 1952) pp. 27–51. In fact words have a *suppositio* (i.e. do designate) only when inserted in propositions (sentences) while they have a *significatum* (i.e. they express a sense) even in isolation. See W. OCKHAM, *Summa logicae* ed. PH. BOEHNER (St. Bonaventure, N.Y. 1957), Ia Pars, c. 63um. According to earlier authors in a proposition only the subject has its *suppositio*, while predicates (and adjectives) *copulant*; see W. O. SHYRESWOOD, in G. PRANTL, *Geschichte der Logik im Abendlande*, vol. III (Leipzig 1867), p. 17, note 60. For the difference between earlier and later scholastics see JOHANNES A S.TO THOMA, *Ars Logica*, ed. by B. REISER (Torino 1948), Ia pars, Lib. II, cap. X, p. 30. For the two different senses of *suppositio*, the stricter and broader ones (this last including also *appellatio*), see also W. BURLEIGH, *De puritate artis logicae*, cit. by PH. BOEHNER, *op. cit.*, p. 45. It seems to me that the very extension of *suppositio* to *all* categorematic terms occurring in sentences – proper to later scholastics – led them to turn the traditional doctrine of *suppositio* in the "complicated technical apparatus", mentioned by P. T. GEACH, The doctrine of distribution (in Mind **LXV** (1956) 67–74: pp. 71–73. (Where Geach shows how this doctrine of *suppositiones* led to the later doctrine of distribution; *ibid.* pp. 73–74).

In approaching Frege's Bedeutung to the medieval *suppositio*, I do obviously not follow the current views. (Even W. and M. KNEALE, *the Development of Logic* (Oxford 1962), though admitting that Mill's denotation and connotation cover at least some kind, respectively, of *suppositio* and *significatio* (*ibid.*, p. 373), do not mention these in connection with Frege's distinction; but they differentiate Frege's pair also from Mill's', (*ibid.*, p. 496). In fact only the critical analysis of Frege's proof of the distinction led me to assimilate sense and denotation to *significatum* and *suppositio*; this is clear on the basis of the arguments of Nos. 3.1, 3.2, 3.3.

Whoever does not rely on the proof (like Carnap, see note 22), but on the illustration of the distinction, and on its application to all categorematic terms, will obviously be led to assimilate it to other distinctions of traditional logic. On one hand, with A. CHURCH (*Review of Carnap's Introduction to Semantics*, in Phil. Rev. **72** (1943) pp. 298–304; see p. 301), to approach it to the one of Port-Royal logic, between comprehension and extension, and to Mill's connotation and denotation; on the other hand, with Carnap, to the pair nominatum-sense (see [MN], pp. 126–127). Notice that Church accurately speaks of their being only "similar distinctions" without any closer historical or theoretical correlation, while Carnap seeks for which traditional pair of concepts – the *explicanda* – Frege introduces his new ones, as *explicata*; and it seems to me that Carnap is right in rejecting, as *explicanda*, both pairs of Port-Royal and Mill's logic, while accepting as such *explicanda* the pair nominatum – sense of everyday language.

As the extension of the distinction sense – denotation to *all* categorematic words assimilates it obviously to the distinctions of Port-Royal and of Mill's

4.3 Since the sentence is (as stated in Frege's explanations) an organic complex,[25] it will be useful to specify the way this happens; that is how the designations of the following expressions, subjects, predicates and relation-signs, and sentences taken as a whole, are connected one to the other.

Though here I depart from Frege's theories (as I admit, for each of these expressions, a different way of designating), the leading thread for this research can be found – especially for names occurring as subjects – in his own teaching. The theory of reference I would propose here will be than completed by resorting – especially as far as predicates and relation-signs are concerned – to suggestions taken from the other semantical analysis of language: the one hinging on the theory of descriptions, inaugurated by Russell.

4.3.1 In *Sinn und Bedeutung* (the work in which the distinction is proved *ex professo*) the possibility for sentences to be true or false depends on there being the denotata of the names occurring as subjects. Though Frege speaks elsewhere of the denotation of predicates and relations signs[23] – and admits a denotation for any name, even if being part of a predicate[26] – he does not mention at all these other denotata in connection with the truth-value of

logic – provided that these apply to all categorematic terms (except to Mill's "not connotative" proper names; if you are willing to grant that there are such names) – I would notice a certain ambivalence in Frege's pair of concepts, and especially in his *denotation*; in fact Frege's denotation seems to be akin – to begin with, i.e. according to the proof of the distinction – to the medieval *suppositio*, while at the end it can be assimilated to the later extension or to Mill's denotation.

For the correlation between the extension of terms and their designation see my essay *Il "senso e significato" di Frege* in *Studi di Filosofia e di storia della Filosofia in onore di Francesco Olgiati* (Milano 1962) pp. 420–483; pp. 40–42.

[25] See B. RUSSELL's comment on Frege's logical theories, in Appendix A of [Principles], (cit. in note 16): §§ 482 ff; see also *ibid.*, § 136. See also L. WITTGENSTEIN, *Tractatus*, prop. 3.141.

[26] See [ÜBG], pp. 194–195; [Tr. Frege], p. 44, where Frege acknowledges that a name like 'Venus', having a denotation (that given planet) can "form part of a predicate".

sentences: thus the denotations of other words there occurring seem to have no importance in this respect.

Therefore the presence of the truth-value (considered by Frege, from now on, as the denotation of the sentence) seems to be conditioned by the presence of the denotata of the arguments only. Frege himself does not point this fact out explicitly, but this could be inferred: 1) from Frege's stating that the predicate is affirmed or denied of the denotatum of the name: so that whoever does not admit its presence can neither apply nor withold the predicate; 2) by the examples given by him.[27]

Hence it is the experience of our speaking to lead to these conclusions: 1) the presence of the denotata of the subjects of a sentence is the condition of the very possibility to get its truth or falsity, starting from its mere sense – the non affirmed proposition –; and conversely 2) the assertion of a sentence as true involves the explicit acknowledgement of the presence of the denotata of its subjects.

These are the objects spoken about in sentences: i.e. the things (denoted by the subjects) to which the sense of the predicate (or relation-sign) is applied or witheld; the things that I would call (leaving Frege) the designata of the sentence itself.

4.4 Thus the thesis of the primacy of the sentence-context, as fas as the determination of the meaning of words is concerned, points out exactly how the splitting of their meaning into sense and

[27] [ÜSB], pp. 32–33; [Tr. Frege], p. 62. I quote the whole passage: "Hat vielleicht ein Satz als Ganzes nur einen Sinn, aber keine Bedeutung? Man wird jedenfalls erwarten können, dass solche Sätze vorkommen [...] Und Sätze, welche Eigennamen ohne Bedeutung enthalten, werden von der Art sein. Der Satz "Odysseus wurde tief schlafend in Ithaka ans Land gesetzt" hat offenbar einen Sinn. Da es aber zweifelhaft ist, ob der darin vorkommende Name "Odysseus" eine Bedeutung habe, so ist es damit auch zweifelhaft, ob der ganze Satz eine habe. Aber sicher ist doch, dass jemand, der im Ernste den Satz für wahr oder für falsch hält, auch dem Namen "Odysseus" eine Bedeutung zuerkennt, nicht nur einen Sinn; denn der Bedeutung dieses [p. 33] Names wird ja das Prädicat zu- oder abgesprochen". It is easily seen that, though there are here three names occurring in the sentence, only one is relevant to our question: the subject 'Odysseus'. Both the other names 'Ithaca' and 'Land' have their denotation, but this is unimportant in respect of the designation (in Frege's sense) of the sentence: its truth-value.

denotation is to be taken; moreover this distinction shows the way in which the primacy of the context is to be understood.

5. As a conclusion I should say that each expression has its own sense; but only in the context of sentences, expressions (the categorematic ones) explicitly designate the things of which we are speaking; this substantiates the thesis of the primacy of sentence-context in the determination of the meaning of words.

This result depends on the reason why Frege distinguishes the denotation from the sense: as we have seen the basis of the proof lies in the fact that several senses correspond to only one denotatum; and this is assured, as pointed out, by the possibility of stating a corresponding true sentence. This is why I conclude that the virtual ability of each categorematic term to lead, through its sense, to something, becomes an actual "standing for" (in the case of the subject) or "applying to" (for the predicate or relation-sign) the thing spoken about, only in the context of sentences.[24]

It is easily seen that the conclusion, unlike the starting point, is not the same as Frege's: at least in the way of conceiving the designation of the other parts of sentences, proper names excluded, and of sentences themselves.

On the other hand the reasoning itself leads back – as previously seen – to Frege's principle of the primacy of sentence-context in the determination of meaning.

6. The conclusion thus achieved is strangely quite akin to the core of the other semantic analysis, "apparently", as Church maintains, "the only tenable alternative to Frege's account":[28] i.e. Russell's theory of descriptions, especially as improved by Quine.

I do not want to expound it, nor elaborate its analysis, but it will be useful to recall some of Russell's and Quine's theses, which I shall use to complete and clarify the theory of designation here proposed.

6.1 To avoid misunderstandings I anticipate the main results of this approach, in stating that: 1) the point of agreement (between my own achievement and the said theory) lies in the importance

[28] A. CHURCH, Review of Carnap (cit. in note 24), p. 302.

given to the context (which however must be put in the right light, also in regard to the theory of descriptions): this is expected to be one of the fundamentals of the account of semantics I wish to advocate. 2) The chief divergency between Russell's theory of meaning on one hand, and Frege's and the one here supported on the other, is the elimination of the sense of expressions as distinct from their denotation. This seems to be untenable; in fact we shall see that – as Church pointed out – this elimination would lead to the disappearance of the meaning of descriptions themselves, if taken in isolation.

Quine's contribution will correct this defect, but – in my opinion – not completely: in fact, though accepting a meaning distinct from reference, he identifies it with the signs themselves, i.e. with the class of synonymous expressions.

Another preliminary remark will condition what follows: references to expressions of formal languages are expected to clear up the way of meaning of the corresponding expressions of natural languages: the relation which can be set up in between serves in fact to throw light upon them.

6.2 To begin I shall recall the initial statements of Russell's first account of his new theory of descriptions,[29] where its main features can be found at their origin.

Russell starts, in *On Denoting*,[29] stating that he takes "the notion of the variable as fundamental"; where the 'x' occurring in '$C(x)$' "is essentially and wholly undetermined", and '$C(x)$' is an open sentence (according to Russell: '$C(x)$' means a propositional function). Then Russell considers '$C(x)$ is always true' – by means of which '$C(x)$ is sometimes true' is definable – and so he defines 'everything' 'nothing' and 'something'. These "are not assumed to have any meaning in isolation, but a meaning is assigned to *every* proposition in which they occur".[30]

Since the bondage imposed upon variables by quantifiers can be

[29] B. RUSSELL, [OD] On Denoting, in Mind n.s. **14** (1905) 479–493; reprinted in B. RUSSELL, *Logic and Knowledge* essays 1901–1950, ed. by R. C. MARSH (London, pp. 44–56). I will quote from this reprint.

[30] [OD], p. 42. Though departing from Russell's use in *On Denoting*, I use even here quotation marks following the convention stated in footnote 8.

held as the link unifying the sentence – whose arguments are thus bound variables – the very fact of taking variables as fundamental shows the importance given to the context in this theory. And this importance is still stressed by Russell's denying the meaningfulness of isolated words.

So far the "lack of significance in isolation" can appear (as we shall see better, later on) rather plausible. But this contention is immediately applied also to the other parts of denoting phrases, and even to denoting phrases taken as a whole, and out of sentence-context. Holding this course, Russell declares here (I mention it only as an example) that 'a man' when occurring in a denoting phrase, is "by itself wholly destitute of meaning".[31]

6.3 These quotations are enough to point out the two main interfering aspects of Russell's theory of descriptions: 1. the dependance of meaning (of denoting phrases) from the sentence-context; 2. the denial of any kind of meaning different from denotation. The first point might seem analogous to Frege's stating the primacy of sentence-context in the search for meaning; but for the cases in which the context is to be taken into account, Russell stresses its importance, by stating that *all* meaning of denoting phrases is given by their context: and this is closely correlated to the second thesis, quite different from Frege's, provided that it

[31] [OD], p. 43. As is well known, Russell started, in [Principles], Appendix A, by a criticism of Frege's view: "It seems to me that only such proper names as are derived from concepts by means of *the* can be said to have meaning, and that such words as *John* merely indicate without meaning. If one allows, as I do, that concepts can be objects and have proper names, it seems fairly evident that their proper names, as a rule, will indicate them without having any distinct meaning; but the opposite view, [...] does not appear to be logically impossible", (*ibid.*, § 476). Notwithstanding this, in [OD] (note at p. 42), Russell points out that "the theory there advocated" in [Principles] "is nearly the same as Frege's, and is quite different from the theory to be advocated in what follows". In fact in [OD] *any* meaning different from denotation is swept away.

This contention of Russell has been frequently and thoroughly criticised. See for instance P. F. Strawson, On referring (in Mind **59** (1950) 320–344; repr. in A. Flew, *Essays in Conceptual Analysis* (London 1956) pp. 21–52), urging that Russell confuses meaning with referring (and with mentioning). The same confusion is pointed out by Quine; see texts cit. in note 34.

involves the rejection of the sense as distinct from denotation. It is just on account of this second thesis that Russell is forced to state that words – not being proper names having a denotation - do not have any meaning at all if taken out of the context.

6.4 On the contrary I would argue that the contentions, reported at No. 6.2., as to the words having a meaning or lacking it, hold not for a monolithic meaning but for their denotation as distinct from their sense. This step forward has been taken, at least partially, by Quine.[32]

It is well-known, that he states that what is warranted by the bondage imposed by quantifiers on variables – i.e., I would say, by the context – is just the reference of words.[33] On the other hand, he admits that singular terms have a meaning even if deprived of designation.[34] And the importance of this should not be minimized, even though I would not share his thesis identifying the sense with the set of synonymous expressions.[35] Finally Quine's extension of the paraphrase of names in descriptions to all names, the singular ones included, enlarges the importance of the context to the same extent: [36] that is, at least potentially, to all discourse.

[32] W. V. O. QUINE, [DE] Designation and Existence, in Journ. of Phil. **36** (1939) 701–709, reprinted in [Readings] pp. 44–51; [NEN] Notes Existence and Necessity, *ibid.* **40** (1943) 113–127, reprinted in L. LINSKI, *Semantics and the philosophy of language* (Illinois 1952) pp. 77–91; [FLPV] *From a Logical Point of View* (Cambridge Mass. 1953); [WO] *Word and Object* (Cambridge Mass. 1960).

[33] [FLPV], pp. 6–8, 12–13, 85–86, 103; [DE], pp. 707–708; [NEN], p. 118; [WO], Ch. IV–V. See also W. V. O. QUINE, [ML] *Mathematical Logic*, revised edition (Cambridge Mass. 1951), pp. 149–150; *Methods of Logic* (London 1952) p. 224.

[34] [DE], pp. 702–703; [FLPV], p. 9; [NEN], pp. 119–120.

[35] The main difference between Quine's "meaning" and Frege's "sense" was rightly pointed out by A. CHURCH (Review of Quine, [NEN], in Journ. Symb. Log. **8** (1943) 45–47): Quine "suggests, e.g., that the meaning of an expression might be identified with the class of all the expressions synonymous with it; while Frege's sense is an abstract object not having a syntactical make-up" (*ibid.*, p. 47).

[36] [DE], pp. 706–707 [FLPV], pp. 7–8; [WO], pp. 185–186. Quine points out (in [FLPV], p. 39) the importance of the "reorientation in semantics [. . .] whereby the primary vehicle of meaning came to be seen no longer in

6.5 If we take into account the way just suggested (i.e.; so substitute, in 6.2, the denotation, as distinct from sense, for the unique "meaning") the importance of the above remark (see No. 4.4.) comes out – even for the theory of descriptions, as well as for Frege's theories: – that the lack of any distinction of the denotation from the sense prevents us from rightly stating how the primacy of the context is to be understood;[37] as well as, on the contrary, that distinction and this principle do illuminate each other.

7. Therefore we shall be able to find, in the theory of descriptions thus modified, the starting point to restate the theory of sense and designation, as I would advocate it.

Suggestions drawn from the above theory will in fact help to determine how the words of different kinds – quantifiers, variables, and bound variables (No. 7.1), categorematic words (No. 7.2), names and predicates separately taken (Nos. 8.1–8.3) – do designate or do concur in the designation of the context; thereby I shall also specify the various kinds of sense, proper to each of them.

7.1 As for quantifiers their sense consists - besides the indication of a given quantity – in their ability to warrant the designation of the context. This denoting comes into act upon prefixing them to the due open sentence (matrix), providing the sentence-context thus obtained with bound variables – i.e. (still following Quine) with pronouns[38] –.

Regarding the variables – which are bindable by them – we have seen that, according to Russell, they are wholly undetermined. This is in essential agreement with Frege's teaching that they do point out a gap; i.e. they assure that "unsaturedness" thanks to which predicates or relation-signs (of which they are arguments)

the term but in the statement. This reorientation, explicit in Frege [GLA], § 60 underlies Russell's concept of incomplete symbols defined in use".

[37] The objection is even stronger, provided that in this case not only it becomes impossible to give the right statement of that principle, but the meaning of the context itself vanishes, as will be proved below at Nos. 7.2 and 8.2.1; see also footnote 42.

[38] W. V. O. Quine, [ML] p. 68–70. The importance of pronouns is stressed by Quine in [WO], p. 186: "That variables alone remain as singular terms may be seen as testifying to the primacy of the pronoun".

are in need of supplementation and are so able to unify sentences.[39]

It is plain that the aspects of things denoted by means of pronouns – i.e. by bound variables – are however not described by them, but expressed only by the categorematic words of the phrase;[40] and those aspects are the sense that such words express – as we shall see (at Nos. 7.2 and 8.2) –. Whereas the sense expressed by bound variables (pronouns) is not a property or a relation of things spoken about, but consists only in their ability to point to the things so designated, from the very moment they have been inserted in the due sentence-context.[40]

It follows that variables, quantifiers and pronouns cannot by themselves designate anything – if taken in isolation from the context –; in fact though vaguely pointing to something, they cannot by themselves give us the means to know determinately *which* is the designated thing. Therefore the above described, and only that, is their sense. (So we understand that Russell's refusing meaning to isolated words (other than proper names) is less objectionable in regard to expressions of such kinds: exactly the ones with which he begins stating this peculiar contention).

7.2 If, in a given sentence, bound variables (i.e. pronouns) are the only words on which designation relies, it is obvious that the other words there occurring cannot denote, when isolated from them; (thus the extension – with Russell – of lack of meaning to the other parts, the categorematic ones, of denoting phrases would be acceptable only if it would be a question of lack of designation); but these words go on – also in isolation – expressing the sense not expressed by the bound variables; this sense consists (as we have just seen) of at least one aspect of the things they come to designate when in the due context.

To prove it, it is enough to point out that, if these words, taken

[39] See [ÜBG], p. 197, quoted below, at footnote 54; see also G. FREGE, [GGA] *Grundgesetze der Arithmetik*, vol. 2 (Jena 1893, 1903); vol. I, pp. 5–6.

[40] This holds for all "variable-binding operators" including the prefixes for descriptions and class abstractions; but I will not dwell upon these here. The reason is that their application does not produce sentences but singular terms, and – from my point of view – generally there is no guarantee, for singular terms taken in isolation, that they effectively have a denotation; (but see below, No. 12.2).

alone, would not even maintain their sense, they could not concur then to give any kind of meaning to any context in which they should be inserted; this is in opposition to what Russell believes. In fact after stating that 'a man', when occurring in a denoting phrase, is quite destitute of meaning, Russell goes on saying it "gives a meaning to every proposition in whose verbal expression 'a man' occurs";[41] but it is impossible that something provides another thing a meaning which it, itself, does not have; not even if that thing comes to form a part of this one: so if a rose have no scent, it would not concur to the scent of the bunch! Thus, in the case we should go on denying (with Russell) the possibility of a sense distinct from the meaning of names he identifies with their denotation, then (as Church already pointed out) [42] also the very meaning of descriptions would vanish.

This conclusion is inescapable at least for the case of indispensability of the description (in conformity with Russell); in fact, should the description serve (as in this case) to translate words which are only "seeming names", that is pseudo-names destitute of denotation, and therefore of any kind of meaning, then – as a good translation would preserve synonymy – the description too should be quite meaningless. But in this way all these expressions – seeming names and descriptions, synonymous to them – could no longer belong to a meaningful language; and I cannot see which could be

41 [OD], p. 43.

42 A. Church, Review of Carnap (cit. at note 24), pp. 302–303: "If, however, we do reject the notion of sense [...] then Russell's well-known conclusion seems inescapable that descriptions [...] may not be admitted except as introduced by contextual definition, i.e. that a sentence ostensibly containing a description must be construed as a mere abbreviation for a sentence of another form in which neither the description nor any name synonymous to it appears. [...] As a result, the notion of designation largely disappears along with that of sense – except perhaps in the case of sentences. [...] Predicates must be construed as having no meaning in isolation [...] See also A. Church, *A Formulation of the Logic of Sense and Denotation* (in *Structure, Method and Meaning*, Essays in honour of H. M. Sheffer, ed. by P. Henle, H. M. Kallen, S. K. Langer (New York 1951) pp. 3–24: p. 15 note 17); and Review of W.V.O. Quine, Whitehead and the rise of modern logic, in Journ. Symb. Log. **7** (1942) 100.

the use of translations or linguistic analyses turning words into meaningless sense-data.

8. Having thus established that also such categorematic words have a sense by themselves, Quine's formulation of the theory of descriptions will give us the clue to further specify what sense this is, and how we can get to the designation through it; this must be established for names (No. 8.2), for predicates and relation-signs (No. 8.3), and finally also for sentences (No. 9).

8.1 Quine enlarges the theory of descriptions in pointing out that *every* name (not only "seeming names") can be expanded in a description.[43] So every name has a description synonymous to it (remember Quine's "Pegasus" i.e. "the pegasizing thing") and "a singular term need not name to be significant".[44]

8.11 In fact when names are paraphrased in descriptions the burden of denotation is taken up by bound variables; whereas, independently from these, the other words of the description maintain a meaning of their own. As the denoting – i.e. the pointing to the object spoken about –, has already been eliminated from this meaning, the meaning itself can obviously be nothing but their sense: the very sense of the corresponding name, that is one or more aspects of the thing which could be the denotatum of the description.

As for names, it follows that if (for any reason whatever) a name should lack the denotatum which could be pointed to – in the corresponding description – by the bound variable, the meaning

[43] Quine's contention was already anticipated by Russell. (So that Carnap makes no difference between them; see [MN], p. 74). See B. RUSSELL, *The Problems of Philosophy* (London – New York 1912) p. 84, where Russell had already pointed out that "common words, even proper names, are usually really descriptions". Even an anticipation of the famous "pegasizing" is to be found in RUSSELL, *An Outline of Philosophy* (London 1927) p. 267: "Of these [proper names] there are no examples in actual language [. . .] thus assertions about "Peter" are really about everything that is "Peterish". To get a true proper name, we should have to get to a single particular or a set of particulars defined by enumeration, not by a common quality". (This obviously depends on Russell's distinction between knowledge by acquaintance and knowledge by description). The above contention was already stated, about "Socrates" in P.M., 2nd ed., vol. 1, p. 50; therefore "Truly elementary judgements are not very easily found" (*ibid.* p. 45).

[44] [FLPV], p. 9; see also texts cited in note 34.

the name has to maintain will be the sense expressed by the other words of that description.

Thereby we go back to Frege's thesis stating that all names have a sense besides their denotation: I should rather say besides the possibility of denoting something.

8.111 This approach of Quine's thesis to Frege's is possible only as far as we leave out of consideration two of their theories: on one side Quine's identification of what he calls the meaning of words with the set of synonymous words, so eliminating all abstract entities. On the other side, we do not take into account that Frege considers only singular names as names; "common names" would not be, according to him – anything but an "unfortunate expression" creating confusion.[45] (I think the distinction between singular and common names is to be kept, and would be very interesting to analyse, but I am not going to dwell upon this now).[46]

8.112 If (as already proved) there are no names destitute of sense, there can be instead occurrences of names for which there is no sufficient reason to admit they actually have a denotation too.

In fact, how do I know that 'a horse' and 'a winged' and 'a winged horse' do denote something? 'A winged horse' does not denote anything, at least at the present state of our zoological experiences.[47]

As for descriptive phrases taken in isolation and destitute of

[45] G. FREGE, Review of E. Husserl, in Zeits. f. Phil. u. philos. Kritik **103** (1894) 313–332: p. 236; [Tr. Frege], pp. 82–83. And G. FREGE, [KBS] (cit. at note 14), p. 454; [Tr. Frege], p. 105.

[46] I examinated this point in my essay *Il "senso e significato" di Frege* (cit. in note 24, last paragraph) pp. 32–40, Nos. 28–36.

Moreover, as names can be paraphrased in descriptions, the comparison between singular and common names would correspond to that between definite and indefinite descriptions. Thus it would be useful to examine Russell's passage from definite descriptions alone, (as in [OD] and in PM, vol. 1, 2nd ed., p. 30 and p. 66) to both definite and indefinite descriptions(as in his *Introduction to Mathematical Philosophy* (London 1919) p. 167) See G. E. MOORE, *Russell's Theory of Descriptions* in *The Philosophy of Bertrand Russell*, ed. by P. A. SCHILPP (1944) pp. 175–225.

[47] See, e.g., the "indesignative occurrences" of names, by Quine, [NEN] p. 116. In [DE], p. 703, Quine notices that "Pegasus" is not a *name* in the semantic sense".

quantifiers, once we have excluded their supposed insignificance and their pointing to a denotatum as well, the only way out left is the one already suggested: i.e. to identify their meaning with their sense. This is also coming out from the above examples (think of 'winged horse') and will be better focussed in the discussion (see below) on the sense of predicates and relation-signs.

8.12 The assurance of denoting is usually given, even for names, by some kind of context. Firstly the context of sentences; when I say that "there is at least one horse" I state that at least in one case, 'a horse' stands for a denotatum, besides expressing a sense. And secondly – in case – a context of circumstances showing a name is used to call, or to point to something (see later on No. 12.2). (Notice that the above case of sentences seems to be the only one relevant to science, since sentences are the only expressions which can be true or at least confirmable).

8.2 The thesis that names can be paraphrased in descriptions throws new light even upon the way of meaning of predicates or relation-signs.[48] On this subject it is enough to remember that to enlarge a name in a description in which a predicate or relation-sign occurs involves that the noetic or sensible content, i.e. the aspect of the thing spoken about, which was up to now the sense of the name, will be expressed, from now onward, by the predicate or relation-sign occurring in the corresponding description.[49] This

[48] I take 'predicate', in the linguistic sense (see Frege, [ÜBG] p. 198, [Tr. Frege] p. 147). Frege used also 'Begriffswort' and 'Begriffsausdruck'; in analogy with this I will use 'relation-sign' for words indicating relations.

[49] That this is the sense of the name corresponding to such a description is implicit in the remark by A. CHURCH, Review of Carnap (cit. in note 24) p. 301: "The designatum of 'the author of Waverley' is the man Sir Walter Scott; the sense is the description of a man by his being the author of Waverley (not, of course, a property in this case, but another sort of an abstract object, which let us call a description). The designatum of 'Sir Walter Scott' is again the man Sir Walter Scott; the sense is again a description but of a different sort." See also the passage (by Church) quoted in footnote 61.

As to the aspect of the object which is the sense expressed by one of its names, I agree that it can be of any sort. It may also be quite individual, and therefore not a property or relation that could be shared by many; though only in this last case the corresponding description is compound with a proper predicate or relation-sign.

is implicit (for one of the possible cases) in Frege's view of the "concept-names" (*Begriffsnamen*), the ones obtained by prefixing the definite article to predicates expressing a concept under which only one individual falls;[50] with the one difference, that Frege considers the concept as the denotation of the predicate,[23] and not, as I will do here, as its sense.

According to Frege this sense is in fact one of the aspects, i.e. one of the modes of presentation of the object; and I should add that in many cases (though not in all) this aspect can be taken in abstraction from the thing it determines, thus becoming the very sense of a predicate (or a relation-sign). This predicate will then be able to be affirmed (within a sentence) of an argument, either bound variable or name, denoting the thing itself; the thing to which the (before abstracted) sense will be so applied again. When one or many aspects are shared by several objects, we have of course a common name and its corresponding predicate, whose sense is applicable to all of them.

This remark is good not only for singulary predicates (expressing attributes) but also for *n*-ary ones (expressing relations). Let us take the relation "...is the author of...", if the words 'author of Waverley' express (as just proved) an aspect of the object "W. Scott" of which we can speak in a sentence – e.g. "W. Scott is the author of Waverley" –, then 'author of' can obviously express an aspect of each of the two objects "W. Scott" and "Waverley", – at least when in the right sentence (as above) –, this aspect being just the relationship of each of them with the other.

8.21 The foregoing remarks give us the ground for an important conclusion: if the burden of designation weighs on quantified variables, obviously predicates and relations signs, when considered independently from the bondage of their variables (i.e. if taken in their

It is plain that this question is the same as the one concerning the difference between singular and common names (see above footnote 46); but I would suggest to abandon both Frege's theories about *Begriffsnamen* (see hereafter, and note 50) and common names (see No. 8.111. and note 46.).

50 According to Frege, *Begriffsnamen* can be derived only from such names. See [ÜSB], pp. 41–42; [Tr. Frege], pp. 70–71; [ÜBG], p. 197; [Tr. Frege], pp. 46–47; [GGA] (cit. in note 39), vol. I, pp. 18–20.

intension), are relieved of this duty; that is they cannot designate when out of the context where their variables are bound by quantifiers, but shall have necessarily a sense (as already proved). And we just saw that this is the very sense of the name, which can be paraphrased in the description where the corresponding predicate or relation-sign occurs.

8.211 Accepting on the one hand Frege's thesis according to which: 1a) the sense of names is an aspect – or mode of presentation – of the things so denoted, 1b) from given predicates we can obtain "concept-names",[50] whose sense is the aspect itself, indicated also by the respective predicate; and on the other hand Quine's stating that 2a) every name can be paraphrased in a description, and 2b) descriptions designate through bound variables – so that predicates there occurring do not designate when in isolation – thus we come to a theory of the meaning of predicates and relation-signs equally distant from the two extreme points of Frege's and Russell's doctrines respectively.

According to Russell, descriptive phrases, and even predicates there occurring, have no meaning if out of the context; according to Frege, predicates and relation-signs, besides having a sense,[51] designate respectively concepts and relations.[52] From the previous considerations we are instead driven to consider just these concepts and relations as the *sense* – and not as the denotatum – of the respective expressions. So the presence of the so-called (by Frege) concept-names, and on the other hand the paraphrasability (asserted by Russell and Quine) of names into descriptions, point out that the content of concepts can be the sense expressed by both predicates and corresponding names (i.e. a given aspect of the object named by these last ones).

[51] This is Dummett's opinion: "It is difficult to give exact references to justify my statement: it was seldom to Frege's purpose to discuss the sense of functional expressions [...] But a careful reading of *Über Sinn und Bedeutung* will make it plain that Frege had no idea of ascribing *Sinn* only to denoting expressions and sentences, as will [GGA], vol. I, pars 2, 29". (M. DUMMETT, Frege on Functions, a Reply, in Phil Rev. (1955) 96–107: note 5 at p. 98). For further references see above footnote 23.

[52] [ÜBG], p. 198; [Tr. Frege], p. 47–48.

8.22 This thesis, according to which concepts and relations are the sense of the respective expressions, is obviously connected with the one saying that "to be is to be a value of a variable".[53] It follows that there is evidently no place for denotations of predicates and relation-signs other than the ones pointed to by their arguments. Moreover the fact they are followed by free variables shows that – by their very nature – such signs are applicable to something else (in fact Frege's "unsaturedness" is nothing but this).[54]

Also these expressions are expected to reach the fullness of their meaning through operations on these variables; which consists, essentially, in introducing the designation of the objects; so that the sense of predicates or relation-signs will be applied to (or witheld from) them, within the sentences thus obtained.

9. In the context of asserted sentences this application or withholding is done when the things spoken about are actually pointed to, either by the bondage of the variables, or by the substitution of constants for them; these are the two operations by which we advance from sentence-forms to sentences, true or false.

So far I have followed the modern view ascribing on one hand the existential import to the argument(s), on the other the cohesion of the context of sentences (with Frege) to their predicates or relation-signs, or (otherwise) to their bound variables. By this I do not deny the possibility of an alternative analysis assigning both to the copula, as in Aristotelian logic;[55] but this view will not be taken into account here.

Following the other one I would so return, also on the basis of my analysis of the theory of descriptions, to the above thesis regarding sentences; the one I have reached (in No. 4.2) through the examination of some of Frege's remarks (though departing from his own teaching).

[53] [DE], p. 708.

[54] [ÜGB], p. 197,foot note; [Tr. Frege], p. 47: "What I call here the predicative nature of the concept is just a special case of the need of supplementation, the "unsaturedness" ...". Frege was aware of the metaphorical character of "unsaturedness": see [ÜBG], p. 205; [Tr. Frege], p. 55.

[55] The fact that the existential import is assigned only to the copula comes out also from the treatment of contradictories in ARISTOTLE, *Categories*, 13b 26–32.

Accordingly it is only in the context of sentences that the existential import of quantified variables or of names (being their arguments) is testified, by stating the sentence as having a truth-value; and reciprocally the acknowledgement of the presence of the due designata is indispensable to its truth.[56] These designata are just what is spoken about within the sentence.

9.1 Concerning the closure of sentences through the bondage of variables I would add the following remarks. The fact that each variable (bound by one and the same quantifier) can have many occurrences in a statement, is due to the need of applying all the various senses of predicates or relation-signs, to which it serves as argument, to the object (or objects) ambiguously designated through this variable; thus the reciprocal connections (between those properties and relations) are assured, they too, by these many occurrences of one and the same variable (or variables).

Thus the bondage imposed by quantifiers also concurs in ensuring the unity of the sentence-context, by the very fact that they do designate – thanks to their existential import – the given denotata.

9.2 As for statements obtained by substituting constants for variables, I shall recall some of Frege's (already mentioned) thesis: 1) the name, subject of a true sentence, must have its designation, and 2) in such true sentences the predicate is assigned to the denotatum of the subject, whereas 3) the only junction of predicate and subject is not enough to pass from the sense to the denotation, and from the proposition (sense of the sentence) to its truth-value; hence, even in sentences formed by means of substitution, it is the denoting of names (and not only their sense) to give unity to the statement. And inversely, the very fact of stating a sentence as a true one, involves the acknowledgement of the presence of the denotata of its names.

9.3 This does not prevent us from considering the sense expressed by sentences in leaving out of consideration both their truth-value and designation.

Before a sentence is asserted as true what the complex of its

[56] See W. V. O. Quine, [FLPV] p. 103 "an entity is assumed by a theory if and only if it must be counted among the values of the variables in order that the statements affirmed in the theory be true."

words expresses is a junction of the senses of the categorematic terms there occurring; which can rightly be considered, with Frege, as the sense of the sentence,[57] i.e. as the thought (objectively taken[16]), it expresses.

We have a proper (i.e. true or false) sentence, when the phrase is not only used to express a sense (the content of the "thought" or proposition) but is also explicitly stated. Hereby we implicitly affirm its truth too,[58] and explicitly acknowledge the existential import of its arguments.

10. Synthesizing the results achieved – through the critical analysis of Frege's proof of his distinction sense-denotation, as well as of some typical aspects of Russell's theory of descriptions, especially as developed by Quine – I would propose the following sketch of an analysis of meaning.

10.1 Words have – since they are introduced in a language – some meaning; but this does not involve our acknowledgement that things really are so and so, as signified by not affirmed expressions. So words have first of all a sense, even if isolated from the context of sentences, and this sense is an abstract meaning.

Notice that this kind of meaning – the sense – is to be found within the context of a language; whereas the other kind of meaning – the denotation – is chiefly warranted by the context of sentences.

10.1 As for categorematic terms, their sense is – as Frege stated for names – at least one of the aspects of the objects spoken about.

57 [ÜSB], p. 35; [Readings], p. 91: "By joining subject and predicate we always arrive only to a proposition; in this way we never move from a sense to a nominatum or from a proposition to its truth-value". M. Black, [Tr. Frege], p. 64, has "combining" instead of "joining".
For one of the good reasons why sense has to be distinguished, also for sentences, from the full meaning they have when asserted, see G. Frege, Die Verneinung (in Beitr. zur Phil. des Deutsch. Ideal. **1** (1918–1919) 143–157; [Tr. Frege], pp. 117–135), pp. 145–144; [Tr. Frege], pp. 118–119.

58 As to the truth as being implicit in every (asserted) proposition see Ch. S. Peirce, *Collected Papers*, 4.282. A connection can be found between this thesis and Carnap's assertion that the sentence being true is nothing but the sentence itself; (see citation in footnote 70).

10.1 As far as free variables are concerned, they just indicate – as Frege said – that peculiar "unsaturedness" of predicates and relation-signs which shows their predicability of subjects.

10.13 The sense of quantifiers is – besides the quantity ("all" or "some") which they indicate – the existential import, by means of which they warrant the designation within the sentence they close.

10.2 By affirming sentences, we pass from sense to designation.[59] In fact in such statements – affirmed sentences – we implicitly admit their truth; and this involves – first of all – our acknowledgement that *there is* what we are speaking about; the thing spoken of is obviously either the denotatum of the subject (or subjects) of the sentence, or the designatum of its bound variables.[60]

10.21 So the names, being arguments of a proper sentence, denote the things spoken about, through their sense: hence these are the denotata for which they stand in the statement, producing them through at least one of their aspects.

10.22 In sentences the predicate or relation-sign is affirmed or denied of its argument (or arguments); thus the sense of the (singulary or n-ary) predicate is applied to, or witheld from, the *denotatum* of each of the names there occurring as subjects, and

[59] For this sketch of a theory of reference, I use one term, i.e. 'designation' and its derivatives, for any kind of indicating things spoken about, through the sense of any expression; thus "to designate" is taken in an undiscriminated sense. While I use other terms for its specifications; thus I take 'denote' for the only subjects; 'refer to' for predicates; 'to be verified by...' for sentences; while I would use another verb, e.g. 'to name' (see below No. 12.2) for the designation of names taken out of sentence-contexts. If I may venture such suggestions for a language foreign to me!

[60] As we shall see (at No. 10.22) the denotatum of the subject is also the object to which the predicate is referred within the sentence. This I would contend for natural languages; but obviously this does not prevent us from constructing languages (mainly formalized languages) on conventions leading to a different semantical structure (such as if we assign to each term both a sense and a denotation of its own, independently from the sentence-context). A. Church, A Formulation of the Logic of sense and denotation, Abstract in Journ. Symb. Log. **11** (1946) 31, incorporates the distinction in a logistic system with names of denotata and names of the senses of these names. But these do not seem to be semantical structures of natural languages, if designation is taken in the above explained sense; (for the correlation of this designation with the extension of terms see my essay, cit. in note 24).

this through the senses of these names; so each of these denotata, already determined by the sense expressed by its name,[61] is once more determined, in affirmative sentences, by the application to it, whereas in negative sentences by the witholding from it, of the sense of the predicate.

Thereby the predicate or relation-sign, which already expresses a sense, becomes a designation too, inasmuch at it is referred to the thing(s) themselves which are spoken about in the sentence;[60] the ones determined by the application (or witholding) of their sense. So these things become the designata (or references) of the predicate or relation-sign too.[62]

10.221 Let us illustrate this through some of Frege's examples: when we say "the leaf is green", we assert as true that the denotatum itself, which is viewed through the complex sense expressed by the word 'leaf', is *also* determined by the sense of "being green"[63] expressed by the predicate.

When we say "the morning star is the evening star", we assert that the sense, being the relation "is identical to", determines, through the senses of the two names 'morning star' and 'evening star', the denotatum of these two names as being one and the same.

For the case of sentences obtained by binding the variables, it is obvious that the aspects of the designata are all expressed by predicates and relation-signs, whereas the quantifiers serve only to point to things, so far quite indeterminate.

10.3 The things spoken about – either denoted by names or pointed to by bound variables – verify or falsify the whole sentence.

[61] A. CHURCH, *Introduction to Mathematical Logic*, vol. 1 (Princeton 1956) pp. 6–7: "Of the sense we say that it determines the denotation, or *is a concept* of the denotation. Concepts we think of as non-linguistic in character [...] every concept of a thing is a sense of some name of it in some (conceivable) language". And Church points out (*ibid.*, footnote 17) that "this use of *concept* is a departure from Frege's terminology".

[62] This view of the designation and sense of predicates and relation-signs is obviously quite different from Frege's.

[63] Notice that, up to Frege, in German, the copula "ist" of "dies Blatt ist grün" can be eliminated by saying "dies Blatt grünt"; see [ÜBG], p. 194; [Tr. Frege], pp. 43–44.

In fact the proposition (in Frege's sense) i.e. the sense expressed by a sentence, is asserted as true when we (at least) believe the sense expressed by the (singulary or n-ary) predicate does actually determine, affirmatively or negatively, that of which we are speaking.

I should call the denotatum of the name, which is determined by the sense of the predicate, and verifies the sentence, the *designatum*[59] – distinct from the sense – of each of these expressions: i.e. of the sentence and of its categorematic terms, (the most designative expressions according to Carnap).[64] So the *designata* are *what* we speak about, through the senses of expressions.

11. It has become clear that the meaning of expressions which – in Frege's first view (1884) – is to be sought in the *context* of sentences, and not in single expressions taken in isolation, is their *designation*.

Once the designata have been not merely grasped but also actually recognized as such, i.e. when the fact that they are (and are so and so) [65] is explicitly stated in sentences, only then have we got at the fullness of *meaning* of linguistic expressions,[64] that is at their full cognitive value.[65]

11.1 This conclusion eliminates also the main difficulty we find in admitting that only sentences have a complete meaning. In fact it has already been objected that,[66] if the meaning should depend on the context of sentences, it would not be possible to

[64] See also R. Carnap, [MN] p. 7: "Only (declarative) sentences have a (designative) meaning in the strictest sense, a meaning of the highest degree of independence. All other expressions derive what meaning they have from the way in which they contribute to the meaning of the sentences in which they occur."

[65] We come to this by stating a sentence as true. See A. Tarski, Der Wahrheitsbegriff in den formalisierten Sprachen, in Studia Philosophica, **1** (1935) 393–405.

[66] M. Dummett, Nominalism, in Phil. Rev. **65** (1956) 491–505: "Any attempt to express clearly [. . .] even the idea that the meaning of sentences is primary, that of words derivative, ends in implicitly denying the obvious fact – which is of the essence of language – that we can understand new sentences which we have never heard before".

understand new ones never heard before; yet we express and communicate our knowledge by formulating *new* sentences.

The difficulty vanishes if we state that what the context provides is the designation of words, and not their sense. The latter is to be found in words even if isolated from sentences, since the context relevant to the sense of words is the one of languages, not of sentences.

12. This paper concerns only the designation within sentences in ordinary use (direct speech); therefore I shall just give a hint to three more points: i.e. the phrases in "oblique use", the denotation of proper names taken in isolation (out of the context of statements), and the truth-values taken as the denotations of sentences.

12.1 As to the phrases "in oblique use" – i.e. subordinate clauses, as reported speeches and similar ones [67] – the very fact that they are used in this special way depends on the context in which they are. So – any way you want to deal with them more specifically – the theory of designation here advocated seems to be the more appropriate to explain the designation of these phrases too, as it makes designation depend on the context of speech.

12.2 As to proper names, it is plain that they can be used to designate something even out of the context of sentences: as if, for example, a noun is used to call somebody. In this case I should like to single out, by a different term, this specific kind of the (generic) designating: perhaps using for this 'to name'.

I would remark (besides the fact that usually this is done in extrascientific discourse) that designation depends, here too, on the context; it is not, of course, the context of a sentence (here

[67] [ÜSB], pp. 36–49; [Tr. Frege], pp. 65–77. For this kind of speech Frege remarks that "It is hard to exhaust all the possibilities given by a language" ([USB], p. 49; [Tr. Frege], p. 77). It is plain that this variety is possible as long as the subordination of clauses depends on the context of speech.

[68] This kind of context has been emphasized by P. F. Strawson, *On Referring* (cit. in note 31). This contention obviously follows Strawson's general view that "...referring is not something an expression does; it is something that somebody can use an expression to do" (*ibid.*, p. 29); but I think that also independently from this view the above contention can be maintained.

inexistent!) but the one provided by the circumstances which assure we are pointing to the thing named[68] (as when a hunter is calling his dog "Dick!").

12.3 As to Frege's well known thesis concerning the denotation of sentences (the true or the false) and their sense (the propositions) I would add what follows.

As for the sense of sentences, it is given by the junction of the senses of the words there occurring. This seems to be what Frege meant by *Gedanke*: i.e. the non-asserted proposition expressed by the sentence.

As for the designata of sentences, the ones here described are, on the contrary, quite different from Frege's – i.e. the true and the false –, as they are the things themselves spoken about; [69] that is the things denoted by the arguments of the sentence.

It is obvious that in most cases we do not speak of the true and/or the false, but of something else (so that Carnap could say that the truth of a sentence is nothing but the sentence itself).[70] Thus the true and the false are not – generally – such designata of sentences.

12.31 – This does not prevent us from taking the true and the false as what we are speaking about in other speeches (especially in the field of logic): e.g. the so-called propositional variables can be taken as variables of truth-values, within a context where the true and the false are just the things spoken about.[71]

13. To conclude I would add that the thesis I have supported so far is not bound, as regards designata, to a given ontology;[72] i.e. it does not give us means to state – univocally perhaps – which

[69] Frege himself notices that "the designation of the truth-values as objects may appear to be an arbitrary fancy or perhaps a mere play upon words, . . ." [ÜSB], .p. 34; [Tr. Frege], pp. 63–64. In a footnote of the later paper *Die Verneinung*, (cit. in note 57), p. 151; [Tr. Frege], p. 126, Frege seems to give a slightly different account of truth.

[70] R. CARNAP, *Introduction to Semantics* (Cambridge Mass. 1942) pp. xii + 263: p. 26.

[71] For this point see Nos. 55–57, pp. 58–61, of my essay *Il "senso e significato" di Frege* (cit. in note 24).

[72] For references and discussions of various views on this point see L. APOSTEL, Logic and Ontology, in Logique et Analyse **3** (1960) 202–225.

kind of being is that of things denoted by names or bound variables.[73]

In fact designata are these things of which we speak, and thus they are the very objects of our knowledge; therefore it must be up to the various sciences – the philosophical ones included – to state *what* these are. Whereas a theory of language (or of knowledge) can obviously deal with things only in so far linguistic signs (or knowledge) are related to them.

As for the sense of expressions it was proved that it has to be distinguished from the signs, as well as from their designata; hereby the very fact they are abstract entities[74] – i.e. neither linguistic signs nor the very things generally spoken about[75] – is also proved. And this result depends on research falling within the field of the theories of language and of knowledge.

Concerning designata I would only add that, considering how various the objects of sciences are – e.g. animals, numbers, values of different kinds, etc. – it comes out that there are as many different kinds of being as there are various objects of knowledge spoken about. This is, the only "ontology" immediately suggested by that very knowledge of extralinguistic things which necessarily precedes any semantical investigation.

Università Cattolica del Sacro Cuore, Milan, Italy

[73] This contention is supported also by A. MENNE, *Zur logischen Analyse der Existenz* (1958) pp. 97–106. Even QUINE ends his famous *On what there is* with a similar remark (though relying on a quite different point of view).

[74] I agree with Church in maintaining that abstract entities are not eliminable. For a description of such abstract entities see – as to Frege's proposition, i.e. the thought expressed by a sentence – G. FREGE, Der Gedanke, in Beitr. z. Phil. d. Deutsch. Ideal. **1** (1918–1919); English translation by A. M. and M. QUINTON, in Mind **65** (1956) 289–311; especially pp. 292, 302, 308–311.

[75] Of course we can also speak of senses and thus they become the designata of such expressions, as when we say: "sense is an abstract entity". In this sentence the sense of the name 'sense' is identical to its denotatum.

This is a feature interesting to analyse, but which is not examined here.

CHAPTER 13

LEIBNIZ'S LAW IN BELIEF CONTEXTS

RODERICK M. CHISHOLM

1. The author of *Marmion* is identical with the author of *Waverly*; it is true that George believes the author of *Marmion* to be Scotch; and it is false that George believes the author of *Waverly* to be Scotch. These facts, according to many philosophers, present us with two undesirable extremes. We might reject Leibniz's law, according to which if a certain thing A (say, the author of *Marmion*) is identical with a certain thing B (say, the author of *Waverly*), then whatever is true of A is also true of B. Or, we might say that, if George believes the author of *Marmion* to be Scotch, the fact that he has the belief is *not* a fact about the author of *Marmion*; and that, if he does not believe the author of *Waverly* to be Scotch, the fact that he does not have the belief is not a fact about the author of *Waverly*. In the one case, we would be committed to saying that, although the author of *Waverly* is identical with the author of *Marmion*, the author of *Waverly* may yet have certain properties which the author of *Marmion* does *not* have. And in the other case we would seem to be saying that George's beliefs do not pertain to the author of *Waverly* (i.e., the author of *Marmion*). It is clear, I think, that these *are* undesirable extremes. I wish to suggest, in the spirit of Russell, that both of these extremes may be avoided.

2. By "Leibniz's law," we mean, not the "*salve veritate*" principle,[1] but the more fundamental

[1] "Two terms are the same (*eadem*) if one can be substituted for the other without altering the truth of any statement (*salve veritate*)."

(1) For every x and every y, if x is identical with y then whatever is true of x is true of y.

Our problem arises if we consider this law along with three other statements which also seem to be true. These comprise: a statement of identity

(2) The author of *Waverly* is identical with the author of *Marmion*;

a belief-statement

(3) Although it is true that George believes that the author of *Marmion* is Scotch, it is false that George believes that the author of *Waverly* is Scotch;

and a general statement about the relevance of belief to reality

(4) For every x, if anyone believes that x has a certain property F, then his believing that x is F is something that is true of x; and if he does not believe that x is F, then his not believing that x is F, is also something that is true of x.

The four statements together seem to commit us to a contradictory conclusion:

(5) There exists an x such that George believes that x is Scotch, and such that it is false that George believes that x is Scotch.

To solve the problem, we must show either that one of the first four statements is false, or that there is no justification for believing that the premises commit us to the conclusion.

I shall propose a solution for this particular example. If the solution is adequate, then, I suggest, it may be generalized to take care of other cases where Leibniz's law, in application to psychological contexts, seems to lead to similar difficulties.[2]

3. Some have attempted to solve the problem by denying Leibniz's law and submitting to a kind of relativism. Thus it has been said: "What is true of a thing under one description may not be true of it under another description; hence what is true of a

[2] But for modal contexts ("it is necessary that...," "it is possible that ...,"), where an analogous problem arises, the solution may need to take a different form.

thing under the description 'author of *Marmion*' may be false of it under the description 'author of *Waverly*'."

Others have attempted to solve the problem by denying premise (4) – "If something x is believed by a man to have a certain property F, then his believing that x is F is something which is true of x" – and thus submitting to a kind of subjectivism. For to deny (4) is to say, in effect, that our beliefs have nothing to do with the objects they purport to be about, and this is to revive the doctrine that "the mind cannot get outside the circle of its own ideas."

We will find the solution, I think, if we look more carefully at premise (3) and then consider the relation between the premises and the conclusion. Although, as I have suggested, the problem is not primarily a problem about the nature of language, we can solve it only by making use of the insights of those philosophers and logicians who have emphasized its linguistic aspects.

4. Let us make four preliminary observations. These concern (a) the logic of belief-statements, (b) Russell's theory of descriptions, (c) what Quine has said, in the spirit of Russell, about the adjectival nature of singular terms, and (d) the disjunctive or indeterminate character of certain statements about belief.

(a) The general point about the logic of belief-statements concerns the result of using the apparatus of quantification-theory in the formulation of such statements. We may think of "George believes that" as being a type of modal operator and distinguish the case in which it modalizes an existential quantification *in sensu composito*, e.g., "George believes that there exists an x such that x is honest," from that in which it modalizes such a quantification *in sensu diviso*, e.g., "There exists an x such that George believes that x is honest." In the first case, the modal operator immediately precedes the existential quantifier and thus modalizes the entire quantified statement; in the second case, the existential quantifier immediately precedes the modal operator and thus quantifies into a modal context. Let us note that the first of the two statements of our example does not imply the second: George may have a general faith in human nature and thus believe that there is an honest man without being able to specify any particular person as a person whom he believes to be honest. Similarly, although there is no one whom I believe to be President Johnson's successor, I believe that there is someone who is President Johnson's successor.[3]

We may affirm this general principle: no belief-statement *in sensu composito* ("George believes that there exists an x such that ...") implies any belief-statement *in sensu diviso* ("There exists an x such that George believes that...").[4]

Part of our problem, then, is to answer the question: Shall we paraphrase "George believes that the author of *Waverly* is Scotch" *in sensu composito* or *in sensu diviso?*

(b) Let us remind ourselves, following Russell, that many statements of the form, "The thing which is F is G," where "F" and "G" occupy the position of adjectives, may be paraphrased into statements of the form: "There is just one x such that x is F, and x is G." (For simplicity, we may use the expression "just one thing x" instead of writing out Russell's paraphrase in full.) And "The thing which is A is identical with the thing is B" may be paraphrased as "There is just one thing x such that x is A, and just one thing y such that y is B, and x is identical with y."

(c) Following Quine, let us assume that any noun or substantival expression (e.g., "dog") which is used to denote an object may readily be transformed into an adjective (e.g., "canine") which is true of that object; if the noun is a proper name (e.g., "Socrates"), it may be turned into an adjective (e.g., "socratic") which is true exclusively of that object. In the latter case, the proper name (e.g., "Socrates") may be replaced by a descriptive phrase of the form, "The x such that x is F," where "F" is the adjective (e.g., "socratic") that replaces the proper name.[5]

[3] Compare W. V. Quine, Quantifiers and Propositional Attitudes, Journal of Philosophy **53** (1956); Jaakko Hintikka, *Knowledge and Belief* (Ithaca 1962), pp. 148–150.

[4] One of the problems of epistemology could be said to be that of formulating general principles enabling us to pass from psychological statements *in sensu composito* to psychological statements *in sensu diviso*. For, according to some conceptions at least, the only statements which are directly evident are first person psychological statements *in sensu composito* ("I believe that ...," "I seem to remember that...," "It appears to me that...," *etc.*).

[5] "So there is no longer any obstacle to treating all singular terms as

(d) Now we need only note the disjunctive or indeterminate character of the type of belief-statement with which we are concerned.

The statement "George believes that the author of *Waverly* is Scotch" is indeterminate in that it presents us with two different possibilities, realization of either one being sufficient to make the statement true. We might say, in somewhat intuitive terms, that the sentence allows, first, for the possibility that the descriptive phrase ("the author of *Waverly*") is being used to describe the *object* of the belief (the actual thing which the belief is about) and, secondly, for the possibility that the descriptive phrase is being used to express the *content* of the belief (what it is that is believed about the thing).[6] Or, somewhat more precisely, "George believes that the author of *Waverly* is Scotch" would be true under either of these conditions: (i) the author of *Waverly* is such that George believes him to be Scotch; and (ii) George believes that just one thing wrote *Waverly* and that that thing is Scotch.

Now we have an answer to our question "Should we paraphrase 'George believes that the author of *Waverly* is Scotch' *in sensu diviso* or *in sensu composito?*" The answer is that we should paraphrase it as a disjunction of two statements, one *in sensu diviso* and the other *in sensu composito*.

The context in which such a belief statement is made may well rule out one or the other of the two possibilities which the statement presents. But it is essential to note that the statement itself presents us with two possibilities. One of the mistakes which give rise to our present problem is that of supposing that only one of the

descriptions. Given any singular term of ordinary language, moreover, say 'Socrates' or 'Cerberus' or 'the author of *Waverly*', the proper choice of 'F' for translation of the term into 'the x such that x is F' need in practice never detain us. If a pat translation such as 'the x such that x wrote *Waverly*' lies ready to hand, very well; if not, we need not hesitate to admit a version of the type of 'the x such that x is – Socrates' or 'the x such that x is – Cerberus'..." W. V. QUINE, *Methods of Logic* (New York 1956), pp. 218–219; compare: *Mathematical Logic* (New York 1940), pp. 149–152.

[6] This distinction doubtless brings to mind Frege's distinction between the *Bedeutung* and the *Sinn* of a term. But we need not here commit ourselves to Frege's theory of reference.

two possibilities – the interpretation *in sensu composito* – would render the statement about George and the author of *Waverly* true.

Consider the true statement, "Columbus believed that the land we now call 'Cuba' is in the Indian Ocean." The statement would be false if "the land we now call 'Cuba'" could be taken only as expressing the content of the belief. But "the land we now call 'Cuba'" can also be taken to describe the actual object which the belief is about, and because of this fact the statement is true. In other words, the statement is true in virtue of the fact that the land we now call "Cuba" is such that Columbus believed it to be in the Indian Ocean. (Compare this statement, taken from a biography: "Some of his comrades at school thought that the future Pope would never hold ecclesiastical office."[7])

5. Making use of the points just made, let us now labor the obvious and note how certain belief-statements may be paraphrased.

We may use lower-case letters to indicate positions in which nouns occur, and upper-case letters to indicate positions in which adjectives occur. The adjective which is derived, according to Quine's procedure, from the noun indicated by a lower-case letter may be indicated by the upper-case version of that same letter. Thus if "*a*" is replaced by "Socrates," then "A" should be replaced by "socratic."

The two alternatives which are presented to us by "S believes that *a* is F" may now be spelled out as: "Either (i) there is just one thing *x* such that *x* is A, and S believes that *x* is F; or (ii) S believes

[7] Russell writes in: On Denoting: "... when we say, 'George IV wished to know whether Scott was the author of *Waverly*', we normally mean 'George IV wished to know whether one and only one man wrote *Waverly* and Scott was that man'; but we *may* also mean: 'One and only one man wrote *Waverly* and George IV wished to know whether Scott was that man." *Logic and Knowledge* (London 1956), p. 52. Russell said that such sentences in ordinary language are *ambiguous*, sometimes referring to the one possibility and sometimes to the other (compare: *Principia Mathematica*, Vol. I, p. 186); I am suggesting, however, that they are, not ambiguous, but *indeterminate*, telling us that one or the other of the two possibilities obtains.

that there is just one thing x such that x is A, and also that x is F."

The contradictory of "S believes that a is F" – i.e., "It is false that S believes that a is F" (as distinguished from "S believes that it is false that a is F") – is a denial of each of the alternatives just considered. That is to say, "It is false that S believes that a is F" may be paraphrased as: (i) It is false that there is just one thing x such that x is A, and S believes that x is F; and (ii) it is false that: S believes that there is just one thing x such that x is A, and that x is F."

The method of paraphrasing other belief statements may now be obvious. But perhaps we should note that such a statement as "S believes that the thing which is A is identical with the thing which is B," which contains *two* descriptive phrases, presents us with *three* alternatives: viz., "Either (i) S believes that there is just one thing x such that x is A, and just one thing y such that y is B, and that x is identical with y; or (ii) there is just one thing x such that x is A, and S believes that there is just one thing y such that y is B, and that x is identical with y; or (iii) there is just one thing x such that x is A, and just one thing y such that y is B, and S believes that x is identical with y." And so on, for more complex belief statements.

6. We may now turn back to premise (3) of our puzzle

> Although it is true that George believes that the author of *Marmion* is Scotch, it is false that George believes that the author of *Waverly* is Scotch

and paraphrase it in the manner described (but retaining, for the sake of simplicity, the substantival expressions "*Waverly*" and "*Marmion*"):

(a) It is false that: there exists just one thing x such that x wrote *Waverly*, and George believes that x is Scotch.

(b) It is false that: George believes that there exists just one thing x such that x wrote *Waverly* and that x is Scotch. Either: (c) there exists just one thing x such that x wrote *Marmion*, and George believes that x is Scotch; or (d) George believes that there exists just one thing x such that x wrote *Marmion* and that x is Scotch.

This expanded version of premise (3) tells us, then, that (a) and (b) are true and that either (c) or (d) is true.

To solve our problem, we need only consider the result of assuming (c) to be false and the result of assuming (c) to be true.

If (c) is false, then we will have two statements – (a) and (b) – telling us what George does *not* believe (but not telling us what he disbelieves). We will have just one statement – (d) – telling us what George *does* believe. And this one statement which tells us what George does believe is a belief-statement *in sensu composito* ("George believes that there exists an x such that ..."). But the conclusion of our puzzle is a belief-statement *in sensu diviso* ("There exists an x such that George believes that..."). Hence if (c) is false, we have no reason for supposing that the premises of our puzzle imply the conclusion.

And what if (c) is true? Our second premise ("The author of *Waverly* is identical with the author of *Marmion*"), taken in conjunction with (c), implies that (a) is false. But our third premise implies that (a) is true. Hence if (c) is true, the third premise is false and therefore, once again, we escape the conclusion.[8]

More generally: if we know of the A that it is identical with the B and of George only that he believes that the A is C, we may not infer that George believes that the B is C (much less that the B is such that George believes it to be C); but if we know of the A, not only that it is identical with the B, but also that it is such that George believes it to be C, then we may infer, not only that George believes that the B is C, but also that the B is such that George believes it to be C.

Brown University, Providence, R.I., USA

[8] I am indebted to Herbert Heidelberger for correcting an error in an earlier version of this paper.

CHAPTER 14

CONJECTURAL INFERENCE AND PHENOMENOLOGICAL ANALYSIS

ANNA-TERESA TYMIENIECKA

The statement may be ventured that phenomenological analysis combines the two methodological procedures which Leibniz had distinguished as "analysis" and "synthesis". The analytic and synthetic aspect of the structural approach elaborates a body of knowledge, yet by itself it would amount to no more than an elucidation of what has always been there to be seen, to a tautology. Analysis and synthesis cannot contribute to authentic progress in knowledge other than by serving as a springboard to further inquiry.

What could constitute such a further step in research for the phenomenological method which in its program had to reject empirical reduction, deductive reasoning, and idle speculation?

In the development of post-Husserlian phenomenology either the postulates of the methodological clarification and rigor have been abandoned, as in the metaphysical doctrines which originated in Husserl's thought and in the philosophy of the *Lebenswelt*, or, as in phenomenological ontology and phenomenologically inspired science, no progress has been made beyond purely structural descriptions.

Are we bound to remain at the purely descriptive level, abandoning any hope of real progress in knowledge? Or should we hastily despair for clarity and precision of foundations and abandon ourselves to vague speculations?

In the present essay, on the contrary, we will attempt and legitimate a program of enquiry, as well as its objective and method, which would take into account the results of structural analysis and tend to go further; relying upon the separate individual structures it seeks the outline of the whole, of the *universal order* and of the *architectonic project.* It is no more a new structure we look for, but the set of *conditions* for all structures given. Taking as a starting point the structural data of the given, this program goes on to ask its *reasons*.

From ontology we move to cosmology. In the first part of our essay we will stress the basic arguments of our inquiry and outline our conception of the *universal order and architectonic project* (*plan*) *of the universe* as the objective for a research comprising the structural level and projecting it into a *teleological* one, cosmological, coupled with the notion of the *conjectural inference* as the appropriate method towards its discovery.

In the second part, the conception of conjectural inference will be determined in more detail and be further specified when we discuss certain similar preoccupations that we see in the thought of Leibniz.

I

1. The anticipatory evidence and the choices of the method

In general, it can be asserted that we focus our attention on foundations and method only when the edifice of research is outlined and when we have already come to grips with the problems and difficulties of its realisation. First, because our attention in every undertaking is essentially oriented towards the aim to be achieved; the search for the ways or means to attain it comes as a secondary preoccupation. The nature of the aim proposed entails the indication of appropriate means to attain it. The question of the method comes to our attention only when the process of attainment is unduly hindered, or when we wish to establish the validity of results obtained.

The preoccupation with the method is secondary, furthermore, because problems of foundations, being thus subordinated to their aim, cannot be authoritatively scrutinised other than by being checked by the results, that is by confronting the results obtained with the objective aimed at. The *anticipatory evidence* of such results as possible and desirable is then the first condition of every type of research or pursuit.[1] The question of foundations and method arises when the need is felt to justify the results obtained by the nature and validity of that anticipatory evidence. Consequently, no discussion of the method can be plausibly pursued without a grasp and clarification of its corresponding anticipatory evidence of the results.

The phenomenological method in all its forms consists essentially of a combination of the analytic and synthetic procedures, in the sense in which Leibniz uses these terms. Its analytic aspect consists, firstly, in distinguishing and ascertaining a level of analytic visibility at which the given subject matter is considered; secondly, in dissociating its various structural elements, differentiating them from each other. This differentiation leads to determine their specific nature and their relations to each other in their rôle of constructing jointly the given object. Thus the basis for a selection of those elements which play within the whole a specific constructive rôle is prepared, and to operate this selection no longer belongs to analysis proper; it is obviously a synthetic operation. Such a selection necessitates, first, a hierarchical arrangement of these elements according to the importance in their constructive function to be established; second, this selection culminates in the grasp of a structural pattern towards which all the elements coincide. This culminating phase of the "eidetic insight" or in Husserl's words "the viewing of the essence" (*Wesenschau*) is synthetic par excellence.

In the clarification of the phenomenological method its objective foundation consists in the assumption of ideal structures inherent in the subject matter. This objective requisite foretraces the aim towards which the method is oriented; the analytico-synthetic operations in this form are coupled with a foresight of the possible structures which they are geared to reveal. The validity of these procedures is itself justified by the fact that they are capable of

[1] We are pleased to have found a confirmation of this view in a discussion of scientific methods made by a scientist himself. See H. S. HOUTHAKKER, The Present State of Consumption Theory, Econometrica **29** (1961) 705.

fulfilling such expectations. Together with its anticipatory evidence indicating a universe of possible structures as its aim, the phenomenological analytico-synthetic procedure outlines the program of phenomenology as initially seen by Husserl. The method plays in this program a secondary rôle implied by the anticipatory evidence of possible results.

This program, fulfilled to a considerable degree in various concrete phenomenological studies, in phenomenological ontology, aesthetics etc, as well as in several realms of science which it has inspired, offers the universe of knowledge in the form of a set of basic structures corresponding to all types of beings in general and a number of structural fragments showing the great diversity of their concretisations in a more specific form.[2]

2. The outline of the program of conjectural inquiry

However, at the analytico-synthetic level, we are left with a series of separate and apparently isolated structural fragments, forms of beings, processes, relations seemingly disconnected from the soil in which they take root and from their framework. Although the particular sciences which have adopted phenomenological foundations, such as some branches of sociology, psychology, economics, history etc.[3] have solved some of their particular problems by means of strictly determined structural devices alone, nevertheless, only a limited range of questions have been asked and solved: questions concerning the *given* and the mode of its *givenness*. The structural approach to the given can do nothing more than bring out the various mechanisms of its functioning as given. Piaget's psychology of intelligence has revealed the pattern according to which the functioning of human responses at all levels is organised, a pattern which has solved the puzzle of what intelligence is. Georges Gurvitsch shows the structures of the social life,

[2] For the phenomenological investigation of basic ontological structures see the present writer's Eidos, Idea and Participation, *Kant Studien*, 1958. For the results of the phenomenologically inspired sciences see by the same author: *Phenomenology and Science in Contemporary European Thought* (Noonday 1962).

[3] *Ibid.*

banishing thereby the phantom of the absolute primacy of "society" over the individual; Hedwig Conrad Martius distinguishes the pattern of a self-governing, the self-sufficient functioning of development, solving embryonically the problem of the self-determination of the growing individual without falling into the excess of the "emboîtement" theory. Phenomenological ontology, by clearly distinguishing and elaborating the structures of five general types of beings (thereby solving many of the puzzles about the problems of matter and form, substance and accident etc.) purports the same view.

And yet, although all the distinguishable structures left at the analytic level appear disconnected and groundless, isolated without *explicitly* showing a structural continuity with the others, all of them indicate an *implicit* interdependence. To give a simple example, the structure of the organism indicates dependence upon the environment necessary to the fulfillment of its functions as a mechanism. To grow, to persist in existence, certain climatic conditions, mineral composition of the surroundings, food etc. are indispensable. The structure of intelligence entails as its necessary counterpart certain selections of physical and mental conditions in the environment in response to which the "intelligent" being develops reactions, taking then on the organizational pattern of "intelligence".

An inquiry of the marginal indications entailed by the structural elements themselves pushed far enough would show that the structure may well be the nucleus of the individual beings, yet it is not self-enclosed or sufficient to fulfill the very functions which it itself prescribes for the individual.

It states certain patterns of givenness, of the given, but it fails to explain why they are such and not otherwise. Although it even hints at the co-existence of other beings, it does not explain why it indicates such possible co-existants and not others.

The individual structures do not appear as self-explanatory; the *reasons* for their existence, for the particular selection of their implicitly indicated co-existence, the relations they have, are not revealed. These questions and others concerning their *reasons* transcend the structural level; they coincide to a particular configuration: by the indications of the structural fragmentation we are made aware of the outline, *the universal project of the totality*. First, it crystallises into the *anticipatory evidence of the universal order*; second, by the *conjectural inference*, we are led to the *architectonic project of the universe.*

3. The conjecture of the universal order

All the structures which we encounter in the analytic inquiry show basically the formal features of the five basic structural patterns which correspond to the five fundamental types of beings: individual being (real or ideal), intentional being, idea, process and pure quality. An existential analysis would show that the real individual being is existentially the strongest, that is that all other types of beings whether autonomous like ideas, or heteronomous like the intentional objects are in their actual existence (ideal, intentional etc.) dependent existentially upon the existence of the individual being.[4] The individual being, specifically the real individual, appears as the center towards which the final significance of biological, social, artistic, economic, mathematic, etc. structures converge. All these and all possibles structures express the profound nature, the behavior, the array of connections etc. of the natural being, man, animal, plant etc. Consequently, these structures find their final commitments in the nature and place which the real individual occupies in relation to the rest of the universe. In the position of the real individual they find in turn their final explanation.

As a matter of fact, only the structural nucleus of the real individual is available for direct inspection in structural analysis. Yet, since in our inquiries we start always out from the primitive experience of the real individual as it exists in the world, we avoid subrepticiously separating its ideal nature from this context; on the contrary, its essential nucleus points out its ramifications in the world-context. It seems to point at the world, in the first place, by requiring or *postulating* a *universal order*. This postulation at the

[4] cf. by the present writer: *Essence et Existence*, ed. Aubier (Paris 1957) p. 126–144.

same time indicated this order as a *necessary* field in which the ideal nucleus extends. But a specific type of "extension" is understood, namely, the individual requires postulating the universal order as the field containing its ultimate aim and sufficient reason for its existence. That means that the inquiry as outlined by the set of postulates demanded by the real individual, and which, inspired by Leibniz, we call "conjectural inference", is no more a direct structural inspection, but a *finalistic* (teleological) investigation.

Like the empirical induction it extends our cognition beyond strictly inspectable data, but it does not share the lack of certainty of the latter. Taking root in eidetic structures of beings, we *conjecture* following the requisites needed by these structures to satisfy the conditions for their existence.

The *conjectural* procedure as we devise it, emerges as the continuation of the structural analysis once this analysis merges the ideal nucleus of structure with its situation and its ramifications in the context of the world-total from which the nucleus is originally abstracted. Its two essential merits are that, like the empirical induction, it extends our cognition beyond the strictly inspectable data, but it does not share the uncertainty of the latter. Indeed, taking root in the eidetic structures of beings, the conjecture moves along the requisites postulated by these structures, first in relation to the world-total, and then beyond it in relation to the constitutive design which they indicate.

As a matter of fact, in consideration of the individual structure, the idea of order cannot be avoided. Interconnections among individual beings are indicated by their structures. These interconnections, of an extremely specialized and infinitely varied nature, extend in all directions and yield an infinitely complex scheme of existing things and beings. The individual being is by its nature a bead in this chain; it is intrinsically interconnected with other beings within a net of the most specialized interactions and therein forms a specific context. As we have already pointed out, a certain physical dimension of this context is the closed system of nature, closed in its purposively oriented and causally connected system of operating matters. The world-order cannot be identified with the system of nature, but stands out from it as its concrete pattern indicating its actual forms, their relations and the mechanism of their operations. Neither can the world-order be conceived as an all-embracing whole with particular things and beings as its parts – that is, what we call the word-context – since it means merely the presence of the totality of beings.

But what is striking in the primitive as well as scientific view of this totality is that it is not a chaos, a static state, or a whimsical play of forces. On the contrary, the world-context appears as an articulated sequence of incessantly progressing developments. All types of beings, the system of their interrelations and, finally, this ordering progress are united in this sequence.

Of course, it can be argued, adopting a position analogical to modern physics, that the basic corpuscles of matter coalesce in their constructive activity merely by chance, thereby dispensing with order. And yet, the ideal of chance itself presupposes a norm, that is, an order, if not in things, at least conceived by and operating in man as a standard. Even if we consider the world, not in terms of intrinsic relations, but as modern statistics does, in terms of occurring facts and their frequency, the statistical laws in which these arbitrary facts are expressed obviously appeal to a superior order.

Furthermore, at the purely physical level, the coalescence of the corpuscles may appear entirely arbitrary and unaccountable, yet this arbitrariness does not exclude the necessity of there being an order, system, or code relating these primitive motions to the laws constitutive of the phenomenal level. Without it the emergence of nature would be entirely irrational and unaccountable for.

Although by the world-order we mean the system of organization of forms and operations at all physical levels, the microscopic level as well as that of distinctive beings, that is, the phenomenal level, the world-order does not necessarily contain the answer to all types of relations, operations and their results. In the first place, the world-order is a system ordering the chain of causal relations pervading the world context, and consisting of a system transforming physical matters. As such an underlying system of the workings of nature, the world-order is the object of scientific inquiry.

However, while the chain of causality is limited to the transformation of matters towards the production of singular beings, the world-order is concerned with the succession of forms, of types, which are then singularized in physical nature. As such a formal sequence, the world-order reveals an ontic nature, a distinctive form, not of being, but of an *expanding sequence* rooted in the physical causal chain as its formal organization.

Indeed, paleontological investigations have shown that there is an undeniable line of successive change of forms of species and kinds and that there is a constant progress in this line, that the world-process progresses through structurally higher, more complex, species. There are also reasons to believe, according to astrophysical research, that the inorganic forms of the universe are also engaged in a perpetually progressing activity, continually elaborating their complexity and differentiation of forms.[5]

There are, however, three reasons why the world-order cannot offer us a satisfactory explanation of the problems of the individual structure at this level.

First, if the individual being points towards the causal chain as its source, its ultimate explanation could not be found there; we could never grasp the infinite array of concrete relations involving the complete world-context as it extends in every direction and establishes its causal constitution. Science engaged in such a pursuit has set itself an endless task.

Second, as was shown in Hume's analysis of causality, there is a specific *discreteness* in the nature of causal relations, consisting of a particular lack of continuity between the causal agent and the caused effect. From the point of view of the infinite interrelations of the individual being, there is such a complex concourse of circumstances, preparing the indispensable conditions for every causal relation between A and in B with respect to X, that there is not, and could not be, evidence or proof concerning the causal agent. This correlation of agent and effect is a mere assumption based on a series of correlating observations. Therefore, to say that it is a cause of B tells us these processes and beings are correlated as *they are*, but does not tell us why they should be so correlated. We can find out by

[5] E. SCHRÖDINGER, *Expanding Universes* (Cambridge University Press 1957).

empirical observation that fish can live in water because of their respiratory organs correspond to this particular kind of element. But this statement merely correlates two empirical states of affairs. Causal "reason" gives a merely proximate reason.

Limiting ourselves to the causal level of the world-order, we would never be able to find what is the basis of this correlation; what is the basis of the worldly meachanism that has devised its workings; what is the underlying scheme of things that allows for causal relations; why it is that certain kinds of beings need precisely such conditions in order to subsist; for what universal function have these specific structures been invented, etc.

Our investigation of the world-order has shown that in its concrete nature we cannot find either the explanation for the selection of particular structures, its rules or aim, that is the sufficient reason of their existence. Therefore, the concrete world-order cannot be the sufficient reason of the individual being or any other structure. On the contrary, the world-order appears as existentially derivative and in need of explanation by factors exterior to it.

4. Conjectural requirement of the architectonic project

The idea of the universal world-order has emerged as the first anticipatory evidence of a new program of research which, embracing both the actual and possible totality of structural analysis seeks its own universal framework and conditions.

However, the nature of the concrete world-order does not satisfy the postulational requirements of separate structures; it does not explain why it is that such a type of structure came into existence, or at least into evidence, and not another one; it does not show the principles of co-existence, the rules of coordination and the final principles and aims of selection.

Yet, with the anticipatory evidence of the world-order, we have accomplished a considerable progress in our conjectural line of procedure. It allows us a second conjectural step: we can infer from it a further postulational requirement, that is, the *architectonic project* of the universe.

First, the world-order, which in its concrete matter-of-factness is incapable of accounting for its own mechanism and the rules according to which it is established, indicates *the necessity of a superior, universal order* of planning according to which it is construed; it appears as possessing a mechanism which can operate, but the joints of its operation escape it. This order is indicated as *not merely a factual mapping* of a certain state of things, but as an *architectonic plan considering universal possibilities of beings and the principles of their selection.* We find at this point that through the seemingly negative (if interpreted directly) results of our analysis of the world-order, we reach a conjectural level at which both the concrete world-order and its intermediary, the individual structures, are brought together in order to discover the common clues for their puzzling existence.

This architectonic plan does not mean merely a superior structural pattern of order. Since the world-order postulates the rules and laws according to which the structural plan could have been devised, the architectonic plan cannot be limited to the ideal consideration of the real world-total, but must include considerations of all the elements involved in the possible existence of the world-order and the real being. Consequently, it embraces the rules and laws of the universal total of beings.

In our discussion we have attempted to show how the structural level of inquiry offers new, original evidence for further research. Recapitulating: the anticipatory evidence-proposes as the objective of research, (1) the *universal order*, and (2), the *architectonic project* of the universe; it entails simultaneously the indication of an appropriate way to proceed towards their discovery: *conjectural inference.* By conjectural inference is meant the grasp of the *final reasons* or necessary conditions of the postulates indicated by individual structures. These postulates or requirements are not *given*, but have to be *inferred* as the counterpart of the individual structures demanding them. However, in this case, the inferential mode of reasoning takes as its starting point, not empirical observation in its contingent nature, inferring equally uncertain data, but indications contained within lasting structures which entailed them. It cannot be less "certain" than those structures postulated by their very nature as conditions of their givenness or existence. We witness here an essential correspondence between the respective methods and the anticipated results: the analytico-synthetic method has as its correlate the structural objective, the conjectural method corresponds to the *architectonic project* which cannot be seen, since it is not "given", but has to be reconstructed on the basis of the requirements of structures. Furthermore, there is also a strict correlation to be seen of the two methodological procedures. Each of them operates on a different level, the one, the structural level, the other, that of relations, principles and rules of operations. Yet structural analysis implies conjectural inquiry as its necessary complement and final explanation and conjectural inference is rooted in the results of structural analysis. The analysis operates "horizontally", so to speak, at the level of the givenness, of its structural mechanism, and conjectural inference operates "perpendicularly", bringing the different structural levels together under common principles.

Together, through their procedures and their objectives, they seem to constitute two methodological steps corresponding to a basic *constitutive scheme* of the universe.

Thus in the assumption of the *universal order* and of the *architectonic project* of the universe as the anticipatory evidence and in the outline of *conjectural inference* as the way to its disclosure we have a new program of cosmological inquiry in which the results of structural analysis are comprised and evaluated and which leads us to the philosophical explanation of sufficient reason and final aim of the structural universe. Consequently, this inquiry is no longer structural, but *teleological*.

Let us now see the prototype of our conjectural reasoning in Leibniz' thinking as we interpret it. Our discussion of Leibniz' methodological procedures will provide further specification to our conception of conjectural inference.

II

CONJECTURAL THINKING IN THE PHILOSOPHY OF LEIBNIZ *

1. The reconciliation of methods unsatisfactory to explain Leibniz' way of philosophising

In his various scientific and intellectual pursuits Leibniz had specifically applied different methods appropriate to the given field of inquiry, empirical induction in geology, deduction in mathematics etc. yet, in a letter which was communicated by Rémond to Leibniz, Abbé Conti contrasts the methods of Leibniz and Newton. Newton is praised for merely observing the phenomena and deriving their explanatory laws from observation only, without "worrying about their causes".[6] "What seems beautiful in the system of M. Newton, is that he neither assumes nor recognises in things anything beyond what he sees; then through wisdom he draws conclusions that would not have occurred to any one else ... he never makes a decision about principles." [7] This strictly observational method of Newton, in which explanation must consist of laws equally observable, is called "experimental philosophy" in the *Commercium Epistolicum*.[8] To this "experimental method" Conti opposes Leibniz' way of inquiry which he sees as a search for the causes and principles of phenomena beyond any observation, and which he terms "conjectural philosophy." [9]

This observation of Conti notes with striking simplicity the essential features of Leibniz' scholarly pursuits. Conti's description, stating a universal attitude in Leibniz' methodological approaches, shows most pointedly his way of procedure in the quest of ultimate principles. It is in metaphysical concern that all his particular inquiries culminate.

* Part two has appeared simultaneously in my book *Leibniz' Cosmological Synthesis*, Royal van Gorcum, Assen.

6 *Extraits des Lettres de Conti concernant Newton, communiqués par Rémond à Leibniz*, June 30, 1715, ed. Robinet (P.U.F. 1957) p. 18.

7 *Ibid.* p. 19.

8 Commercium Epistolicum Acts of the Royal Society, July 12, 1715, ed. Robinet (P.U.F. 1957) p. 19.

9 *Ibid.* p. 20.

Leibniz' theory of the method of universal analysis and synthesis conceived at a level scarcely less universal than Conti's description is often mistaken for his method in metaphysics. The essential features of that method, like the analysis of the elements of the given field into simplest components, and the use of hypothesis towards the construction of a new definition is, as Leibniz professes, essential to every approach or method he uses. In its universal conception it is the method of the universal science.[10] Yet the method of universal analysis and synthesis would apply directly only to the basic analysis of concepts, if such were possible without considering a specific field of knowledge, but cannot apply directly to any specific subject matter.[11] As it stands, it could not be the method by which Leibniz' metaphysical principles are derived. Leibniz conceives of metaphysics as a specific science, even if highest in the constitutive hierarchy. It is for him a particular section of the universal science, dealing with its own subject matter and its own specific problems.

The description Leibniz gives of what he calls the method of analysis and synthesis obviously plays an important rôle in his thinking, but in itself it falls short of accounting for the way in which his far-reaching conclusions concerning the ultimate reason of things were obtained.

We will attempt to point out the striking features of Leibniz' procedure complementing the analytical and synthetical aspect by going beyond it in a brief refutation of views which attempt to identify Leibniz' procedures in his metaphysical thinking with the specific scientific methods he uses in particular fields of inquiry.

First of all, according to its central place among other sciences, and as it deals with ultimate principles of reality, Leibniz' metaphysics, as he himself often stated, draws its inspiration not only from mathematics and logic, but to a large extent from his physical, dynamical and biological observations and convictions. Experimentation and observation (microscopic observation of external objects and direct observation of psychological phenomena) play an important rôle in the origin of Leibniz' metaphysics. Consequent-

[10] See the present author's, Leibniz' Metaphysics and his Theory of the Universal Science. International Philosophical Quarterly, September 1963.
[11] *Ibid.*

ly, on the one hand, we cannot accept Couturat's reduction of the sources of metaphysics to logic, or that by Caspari to the empirico-inductive method, each to the exclusion of the other. In the very text used by Couturat to prove his thesis it is patent that Leibniz does not proceed through a deductive chain of reasoning like Spinoza, who first poses the principles and then analytically deduces further statements from them, because the content of the further statements in this Leibnizian text contains elements which are totally absent in the previous ones, and adds to them new original material. To relate them to the preceding ones, not a deduction, but a sort of inference is indispensible.

Caspari, on the other hand, does not accept the supposition that Leibniz' principles could have been derived deductively from some *a priori* principles. He considers that Leibniz obtained new ideas from natural phenomena by the purely empirical-inductive method of the natural sciences, with special reference to the microscopic discoveries of his time.[12]

If there is the question of inference in Leibniz' procedures, it means a specific type of inference. We will get the first indication of its nature if we distinguish two ways in which, in general, the empirical-inductive method may be conceived and applied. The first would rely directly upon empirical data and incorporate them into a body of thought. The second would consider empirical data simply as a source of "inspiration", a springboard helping to form conceptions of an entirely different nature now no longer empirical, but merely analogical. There are, as a matter of fact, different types of connections to be expected according to whether they are established between empirical data by way of induction or between non-empirical data (merely analogical to empirical ones) conveyed by a specific kind of inference. The ideas Leibniz conceives by "inspiration" from what is empirical are of an essential nature, and constitute the level of structures; the inference through which they are attained is grounded upon the scheme of these structures themselves. Empirical induction is related to this inference as the empirical data are to the elements of substance, Leibniz' basic metaphysical

12 Otto Caspari, *Leibniz's Philosophie, beleuchtet vom Gesichtspunkt der phys. Grundbegriffe von Kraft und Stoff* (Leipzig 1870).

concept. The elements of the notion of the individual substance corresponding strictly to facts are relevant to phenomena as we distinguish them by observation; nevertheless, they are of a heterogeneous nature and so are their interconnections.

Thus we have arrived at the first intimation of a way both of dissecting and organising what is given and also of augmenting our knowledge about it beyond analysis. This, in a way that neither means an undue extension of what we know to wards what we do not byhypotheses burdened with the uncertainty of empirical procedures, nor a ratiocination which assumes that all knowables must obey the same laws. It remains to specify it and to examine what foothold in reality it may have rather than being an idle speculation.

But it may be alleged that since it is not possible to dissociate the two methods (the empirical-inductive and deductive) in Leibniz' thought, therefore, a simple synthesis of the two should be accepted. Such an opinion has its reasons: Leibniz clearly oscillates between a priori speculation and the method of inquiry of natural science. Auerbach insists upon the intrinsic connection among the various disciplines like theology, mathematics, physics and philosophy, and Merz upon Leibniz' effort to combine mechanical and teleological convictions, inductive and deductive procedures, observation and theory, efficient and final causality.[13] Neither of them provides a homogeneous basis for such a unification. The reconciliation of the different aspects of Leibniz' thought does mean to them a reconciliation of methods, but these methods are directly taken in their "brute" form to be that of the speculative a priori and that of empirical observation, respectively. The same holds for Wundt's interpretation of Leibniz along a naturalistic-psychological line.[14]

That such a reconciliation of methods is insufficient to account for Leibniz' metaphysical findings can be seen from the very limited rôle he attributes to empirical explanation alone; such an explanation itself (i.e. the empirical world and its nature as he con-

[13] SIGMUND AUERBACK, *Zur Entwicklungsgeschichte der Leibnizischen Monadenlehre* (Dessau 1884), J. TH. MERZ, *Leibniz* (Heidelberg 1886).

[14] WILHELM WUNDT, *Leibniz zu seinem 200-jaehrigen Todestag* (Leipzig 1917).

ceives of it) stays precisely in need of a further explanation, of higher reasons that it contains within itself.

There is an undeniable primitive conviction in Leibniz' reflections which he traces back to his early youth. This conviction gave rise to his basically empirical attitude. The world as we know it directly from observation, and prior to introducing any explanatory clues appears as a series of aggregates. We might scrutinize the world endlessly and not find in it anything but aggregates infinitely decomposable into smaller and smaller parts. However, we cannot deny the world, or these series, some sort of coherence. Even our own experience of the world exhibits an order among the phenomena which permits the continuity of our world-orientation. We do not start anew at every instant our basic orientation in the world; we can rely upon our previous observations to the point of predicting new, similar occurrences for the future. In order to explain, then, how things and beings accessible to our senses as mere aggregates can and do arrive at a coherence, an interconnectedness, it is necessary to go beyond the purely observational stage.

Once we admit that things and beings, as we know them by empirical observation, are mere aggregates, traditional philosophy offers us the following alternatives to account for their order: we can have recourse either to the mathematical point, which explains extension, or to the atoms of Epicurus and Cordemoy; we can deny the reality of bodies altogether; finally, we can admit the notion of substance.[15] The reasons why Leibniz combats the Cartesian solution, admitting of the absolute reality of extension, are only too well known.[16] He rejects as well the phenomenalism of Berkeley which discounts the reality of the world as devoid of an absolute foundation and which places it on the same footing as illusion or dream.

Leibniz confesses to having believed in his youth in atoms. His main reason for rejecting them was their uniformity. As a uniform plurality they could not account for the variety of phenomena; as the basic buildingstones of the phenomenal world, they could not explain the unity of its varied structures; nor could they

[15] Correspondance with Arnauld, April 30, 1687, Montgomery.

[16] *Ibid.* July 14, 1686, p. 136.

function as a ground for infinitely graduated continuity of phenomena.

The plurality of infinitely varied serial phenomena demands a principle of underlying unity. Their order and interconnectedness can be revealed and grasped only with reference to a unifying factor from without.

Such a unifying factor cannot obviously be the result of any particular scientific method; if it has to unify the mathematical and the empirical, transcending both, it could not have been obtained by either in particular or by their simple cooperation. To arrive at the higher reasons for both, a specific transformation of the particular scientific methods would be necessary.

How shall we proceed to find it?

In his search for an order in the succession of phenomena, Leibniz is guided first, by a quest for its unifying principle as derived from the succession itself. The law of succession, its empirical order established by observation, does not yield an explanation. Through observation, we could go infinitely further and further, discovering series after series and its empirical order. Leibniz says, "A continuum is not only divisible to infinity, but every particle of matter is actually divided into parts as different among themselves.... And since this could always be continued, we should never reach anything of which we could say, 'Here is really a being,' unless there were found animated machines whose soul or substantial form constituted the substantial unity independently of the external union of contact." [17] The quest by empirical observation for such a "substantial unity" of the phenomena of the world, bodies, things, matter and events, led Leibniz to revive the Scholastic idea of *substantial forms*.

As everywhere else, he proceeds from a recognition of the observably given. To satisfy the demands postulated by the observable, he attempts to infer principles that could satisfy them. We can see here the prototype of a procedure which we have called "conjectural inference". Indeed, the term "conjecture" seems particularly descriptive of Leibniz' quest. Undoubtedly it is a rational procedure, but its rationality is flexible and mediates between the speculative

[17] Correspondance with Arnauld, Dec. 8, 1686, Montgomery, p. 162.

rationalism of the mathematically inspired Spinoza and the empirical induction of Hobbes.[18] It is also more flexible than the conciliatory rationalism of Descartes, through which he attempts to bring together different principles accepted on their own evidence: a priori principles like, for instance, those of the existence of God and the existence of the world.

In opposition to speculative rationalism in general, which proceeds by drawing consequences from principles established on their own evidence. Leibniz does not establish principles first.[19] Leibniz searches for the type of explanation which the empirical given of the world postulates; he scrutinizes the "requirements" (*requisita*) which it implies and the direction which it indicates. He checks prospective explanatory elements against the criteria which the empirical givenness itself imposes. Thus, principles of "reasons" are neither accepted a priori nor are they deduced or inferred empirically, but "conjectured." Were the "conjectured" identical with the "empirically inferred" we would remain at the natural level of laws observable empirically – the level at which Newton's inquiry remains – and we should never reach the level

[18] In his essay: On the Elements of Natural Science (1682–1684) Leibniz introduces the notion of the "conjectural" method as follows: "Just as there is a twofold way of reasoning from experiments, one leading to the application, the other to the cause, so there is also a twofold way of discovering causes, the one a priori, the other a posteriori, and each of these may be either certain or conjectural. The a priori method is certain if we can demonstrate from the known nature of God that structure of the world which is in agreement with the divine reasons and, from this structure, can finally arrive at the principles of sensible things." Leibniz considers this method as "certainly difficult and that not everyone should undertake it." He recommends "the conjectural method a priori", which "proceeds by hypotheses, assuming certain causes, perhaps without proof, and showing that the things which now happen would follow from these assumptions. A hypothesis of this kind is like the key to a cryptograph, and the simpler it is, and the greater the number of events that can be explained by it, the more probable it is." in LEROY LOEMKER, *Philosophical Papers and Letters*, a selection transl. and ed. with an introduction (Chicago University Press 1956) p. 436–437.

[19] It is a great mistake to admit, like Rintelen, the notion of the monad as such a first principle from which the entire Leibnizean doctrine can be deduced.

of transcendent "reasons." This difference appears clearly in the example given in the above-mentioned "Commercium Epistolicum": "...Newton makes no decision as to the cause of gravity – Leibniz affirms that it is mechanical ... The one says that the first corpuscles of matter are solid because of the power and the will of the Creator; the other that they are solid because of conspiring motions".[20]

In point of fact, going further than Newton in his explanation in some cases, while sticking to "facts" more than Newton does in others, Leibniz follows a strictly determined line established step by step and inherent in the entire system of "conjectural motivation." In order to explain the nature of the corpuscles of matter one should not jump to the ultimate point of explanation, switching at the same time from the physical to the theological system of "reasons." Within the system of the physical, we need only go one step further in order to conjecture that their individual hardness is connected with the system of motion in which they all take part. On the other hand, to infer the law of gravitation from the behavior of phenomena as an observable fact, does not suffice as a "reason" for the phenomena. If the reason for gravitation has to be given, the next step is the conjecture about the nature of its mechanism.

In general, Leibniz proceeds in the following way: the empirical observation and the scrutiny of problems which arise from it point out principles capable of supplying an explanation of the given situation. These principles are pointed out or postulated, as necessary "requisites" for that explanation.[21] This "inference" (postulation) is indispensable for attaining the structural level. Only when the level of *intelligible structures* is reached, through conjecture of postulated principles, can the *analytic* method be applied at all. As a matter of fact, the analytic method *consists* in the recognition of essential structures. After the elements of

[20] Commercium Epistolicum, previously quoted, July 12, 1915, Robinet, p. 20.

[21] Our insight into this matters is largely a result of Professor F. S. C. Northrop's seminar in Philosophy of Science, Yale University, 1957–1958, in which we first became acquainted with his distinction between "concepts by intuition" and "concepts by postulation."

structures are dissociated in the course of analysis, these elements can then be differently combined by *synthesis*. So understood, the analytic method is practiced most ingeniously by Leibniz. The exploration of essential structures offers ground for further inferential progress. For instance, the recognition of the nature of the individual offers the starting point toward the notion of the universal pattern of the world – more generally expressed by the term "pre-established harmony."

In the light of our discussion, the "conjectural inference" which we have previously attempted to establish as a "hypothetical" but non-empirical way of procedure finds its prototype in Leibniz' philosophical thinking. Devised by Leibniz for the sake of taking into account all realms of scientific inquiry, despite the differences in their specific methods, while gathering materials for a philosophical reconstruction of the universe, this way of procedure in an inquiry which possesses more elaborate and differentiated phenomenological tools than the tools possessed by Leibniz, seems to promise a real progress in knowledge coupled with its clarified foundation and freed from the uncertainty of particular scientific methods.

The Radcliffe Institute for Independent Studies,
Cambridge, Mass., USA

CHAPTER 15

ON ONTOLOGY AND THE PROVINCE OF LOGIC: SOME CRITICAL REMARKS

R. M. MARTIN

In his valuable contribution to the 1960 International Congress for Logic, Methodology, and Philosophy of Science, Alonzo Church was concerned primarily with the province of logic and how it is to be distinguished from that of mathematics.[1] Roughly, logic may be said to consist, he says, "in a theory of deductive reasoning, plus whatever is required in object language or metalanguage for the adequacy, generality, and simplicity of the theory." This statement is intended by the author as a "descriptive account rather than a definition, and assuming the notion of deductive reasoning as already known, from experience with particular instances of it." The crucial word 'valid' is left out in this description, and we may assume, may we not, that Church intends 'deductive reasoning' here only in the sense of 'valid deductive reasoning'? Even so, the clause beginning with 'plus whatever' is not clear. The two paragraphs which follow are presumably intended to make amends. Because we wish to comment upon these at some length, let us quote them in full.

"That logic does not .... consist," he says, "merely in a metatheory of some object language arises in the following way. It is found that ordinary theories, and perhaps any satisfactory theory, of deductive reasoning in the form of a metatheory will lead to analytic sentences in the object language .... These analytic sentences lead in turn to certain generalizations; e.g., the infinitely many analytic sentences A $\vee$ $\sim$A, where A ranges over all sentences

[1] ALONZO CHURCH, Mathematics and Logic, in, *Logic, Methodology and Philosophy of Science*, ed. by Ernest Nagel, Patrick Suppes, and Alfred Tarski (Stanford University Press, Stanford 1962) pp. 181–186.

of the object language, lead to the generalization $p \vee \sim p$, or more explicitly $(p)(p \vee \sim p)$; and in similar fashion $(F)(y).(x)F(x) \supset F(y)$ may arise by generalization from infinitely many analytic sentences of the appropriate form. These generalizations are common to many object languages on the basis of what is seen to be in some sense the same theory of deductive reasoning for the different languages. Hence they are considered to belong to logic, as not only is natural but has long been the standard terminology.

"Against the suggestion," Church continues, "which is sometimes made from a nominalistic motivation, to avoid or omit these generalizations, it must be said that to have, e.g., all of the special cases $A \vee \sim A$ and yet not allow the general law $(p)(p \vee \sim p)$ seems to be contrary to the spirit of generality in mathematics, which I would extend to logic as the most fundamental branch of mathematics. Indeed such a situation would be much as if one had in arithmetic $2 + 3 = 3 + 2$, $4 + 5 = 5 + 4$, and all other particular cases of the commutative law of addition, yet refused to accept or formulate a general law, $(x)(y) \cdot x + y = y + x$."

Almost surreptitiously here, as an *arrière pensée*, as it were, Church has been guided by a conception of mathematical generality as well as by a conception of logic as "the most fundamental branch of mathematics." But such conceptions are the very points at issue and we may well ask whether this conception of mathematical generality *should* guide us here, and whether (and if so, why and in what sense) logic *should* be regarded as the most fundamental branch of mathematics. Why, in fact, should logic be regarded as a branch of mathematics at all? It was not so regarded for many centuries, and surely it does not go without discussion that modern developments have proved this beyond cavil. Indeed that it should *not* be so regarded will be urged below at some length.

Further, the philosopher sensitive to matters of existence, i.e., to matters of ontology and ontic commitment,[2] will observe an *enormous* difference between admitting infinitely many analytic

[2] Please bear in mind here Quine's criterion of ontic commitment. See his *Word and object* (The Technology Press of the Massachusetts Institute of Technology and John Wiley and Sons, New York and London 1960) p. 242.

sentences of the form '(A v ~A)', where 'A' ranges over the sentences of the object language, and a generalization of the form '$(p)(p \vee \sim p)$', where 'p' ranges over *propositions*. The one formulation carries with it no ontic commitment other than that carried by A; the other carries ontic commitment to an altogether new kind of entity, abstract propositions. It is clear that these latter are never needed for the purpose of formulating a theory of valid deduction for any given system, or indeed for all first-order systems given simultaneously. Church himself comments later that "the abstract general laws of logic may satisfactorily be formulated as extensional . . . ." and hence presumably without the use of propositional variables, for propositions and intensionality go hand in hand. The formulation of these laws in terms of propositions is in fact more complicated than that without them, in the obvious sense of requiring a new style of variables together with the entities over which they range. Thus at best the introduction of the form '$(p)(p \vee \sim p)$' serves neither the "adequacy", "generality", nor "simplicity" of the theory.

Similar remarks apply to the form '$(F)(y)$ . . . .', although much depends here upon what 'F' is taken to range over. If 'F' is taken to range over properties-in-extension or classes, the ontic commitment of the form '(F) . . . .' involves no intensionality whatsoever. The ontic commitment of '$(p)(p \vee \sim p)$', however, presumably involves intensionality no matter what the variables of the object language at hand range over. Also the form '$(F)(y)$ . . . .' is a form within a specific object-language and is thus linguistically relative. The form '$(p)(p \vee \sim p)$', on the other hand, is intended in some sense to hold for all languages. In allowing quantifiers over all "propositions" we are no longer speaking of the sentences or statements of just one language, but presumably of all propositions expressed in *any language whatsoever*. There is thus an implicit generalization here to all languages or language-systems. Such implicit generalization is not involved in the form '$(F)(y)$ . . . .' on the usual interpretation of the quantifiers. There are thus very essential differences between these two forms which Church fails to note.

Surely also there is an enormous difference between admitting in arithmetic all laws of the form '$2 + 3 = 3 + 2$', '$4 + 5 = 5 + 4$', and so on, and yet refusing to admit the general law '$(x)(y)\cdot x + y = y + x$'. In fact, just this difference has been at the root of some important mathematical work.[3]

Even if we grant that this difference is somewhat special, another point should be observed. We note that the variable 'x' here is presumed to range, say, over the natural numbers or positive integers. Hence the quantifier '(x)' may be read 'for all positive integers x' or 'for all natural numbers x'. The "spirit of generality in mathematics" here carries us from an infinity of statements containing proper names '2', '3', '4', ... of specific integers or natural numbers to a statement containing universal quantifiers over integers or numbers in general. Our criterion of ontic commitment may or may not be formulated so that the sentence '$2 + 3 = 3 + 2$' carries ontic commitment to the integers 2 and 3. If it is, and it is not unreasonable to allow this, the ontic commitment of '$2 + 3 = 3 + 2$' consists of a subclass of the ontic commitment of the sentence '$(x)(y)\cdot x + y = y + x$'. Thus here the spirit of mathematical generality does not lead to a change, but merely to an *increase*, of ontic commitment. This indeed seems as it should be.

For a sentence '$(A \vee \sim A)$', we have the same ontic commitment as for A, some objects in a domain D, namely, over which the variables of A (if any) range or which are denoted or designated by the proper names occurring in A. The spirit of generality then carries us to a general formula in which we allow the quantifier over the objects of D, but not to a general formula such as '$(p)(p \vee \sim p)$' where the ontic commitment is not preserved. In short, the spirit of generality in mathematics should be such as to *preserve* ontic commitment to the same kind of object, not to change it. If the ontic commitment is changed, it is no longer the spirit of

[3] See, e.g., D. HILBERT and P. BERNAYS, *Grundlagen der Mathematik*, Vols. I and II (Springer, Berlin 1934 and 1939); R. GOODSTEIN, *Recursive Number Theory*, in the series Studies in Logic and the Foundations of Mathematics (North-Holland Publ. Co., Amsterdam 1956); and the several important early papers by Th. Skolem mentioned in the bibliography to Goodstein's book, p. 188.

generality which is guiding us, but the desire or need (if such it be) of introducing some altogether new kind of object.

The points we are raising may seem minute, but out of such minutiae important truths emerge which are essential for the proper understanding of logic, ontology, syntax, semantics, theory of intension, and the like.

The small error in mathematics is of little account. It is easy to detect and readily corrected, and the result remains. But in philosophy, and especially in these delicate logical matters, small error piles upon small error to issue in a major mistake. Here the small errors are much more difficult to detect, and we must be on our guard vigilantly in trying to to stave them off. They continually press against us on all sides, and no sooner have we gotten rid of one than another forces itself upon us unawares.

II

Valid deductive reasoning does not take place in a vacuum; it is always reasoning as applied to a particular subject-matter. There is no specifically logical subject-matter, it seems, for us to reason about, validly or otherwise. There is no eternal notion *or* which we introduce the wedge sign 'v' to designate. There are of course *signs* or *expressions*, in particular the signs of logic, but there are no specifically logical individuals or things. There are no specifically logical classes or relations; there are merely specific classes and relations germane to a given subject-matter. The theory of valid deductive reasoning cannot, it would seem, be a theory of a given subject-matter. What is it, then, precisely? Church says that it does not consist "merely in a metatheory of some object language." Surely not of any *one* object-language. This would be much too narrow a characterization. But if we consider all object-languages formulated as first-order systems, say, first-order logic might well be regarded as merely their syntax and semantics.[4]

If there is no specifically logical subject-matter, we speak instead

[4] Cf. the author's *Truth and Denotation* (University of Chicago Press, Chicago and Routledge & Kegan Paul, London 1958), *passim*.

of the signs of a language and of how these signs are connected with each other and with the objects with which the language deals. We consider certain purely syntactical relations between certain formulae of the language, on the one hand, and how the formulae are related to the semantical truth-concept for that language, on the other. Roughly, in some such way, we characterize valid deductive inference for the given language or languages. And most important here, as Church in effect emphasizes, is the notion of analytic or logical truth. Given this latter for a language, the set of valid deductive inferences of the language is uniquely determined. First-order logic may equally well then be regarded as the theory of analytic truths for first-order languages.[5]

Church would no doubt regard the foregoing remarks as arising from a "nominalistic motivation." Actually, however, this view of logic is strictly *neutral* with regard to nominalism. Nominalistic, realistic, and conceptualistic systems may alike be formulated within the kind of logic discussed. This latter contains no hidden features which require defense. Church's kind of formulation, on the other hand, is out-and-out realist, as well as intensionalist. The burden of proof is therefore upon him to provide a suitable philosophical justification for these views. Clearly, the neutral formulation enjoys an important advantage in this respect.

Every language-system, of the kinds usually considered, contains a logic as a part, whether the specific subject-matter be from physics, chemistry, biology, musicology, psychology, or whatever. Logic might therefore be regarded as the fundamental branch of all of these sciences or areas of inquiry. There is nothing especially privileged about mathematical subject-matter, and hence no reason why logic should be regarded as the fundamental branch of mathematics any more than of any other science. Of course, mathematical subject-matter is especially "clear" and it is especially easy to get at the exact character of mathematical inferences. But this says merely that mathematics affords an especially convenient area to which logic may be applied. Here too then a strict neutrality

[5] Cf. the author's *The Notion of Analytic Truth* (University of Pennsylvania Press, Philadelphia and Oxford University Press, London 1959).

should, it would seem, be maintained. The study of valid deductive inference seems quite neutral as regards specific subject-matter. This neutrality permeates the history of logic, from Aristotle and Chrysippus through Leibniz to Bolzano, Boole, Frege and Peirce, and modern developments do not indicate that it should be abandoned or that it is in any way in error.

III

In *The Development of Logic*,[6] William Kneale has urged a use of 'logic' in accord with which one would not correctly speak of the "logic" of the terms for this or that special subject-matter. If this latter were admitted "arithmetic would undoubtedly be the logic of numerals, just as geometry would be the logic of shape words, mechanics the logic of 'force', analytical economics the logic of 'price', philosophical theology (if such a science exists) the logic of 'God', and so forth." It is clear that this use of 'logic' is roughly the same as what more accurately and customarily is called 'applied logic'. The use of 'applied logic' does not seem to involve "an enormous extension of a technical term" and it is not clear why, "if we think that the logic of tradition has been concerned primarily with principles of inference valid for all possible subject-matters, we must reject as unprofitable such phrases as 'the logic of 'God'." In discourse about 'God' we find in fact an extremely interesting and historically important area for the use of logic which it might indeed be very profitable to explore on a more modern footing, as Father Bocheński has recently suggested.[7] Logic may be presumed to include the syntax and semantics of such discourse quite as much as of any other.

If to the purely logical axioms and rules of a language-system we add non-logical axioms akin to Carnap's *meaning postulates*, we need not therewith alter the notion of analytic truth for that

[6] William Kneale and Martha Kneale, *The Development of Logic* (Clarendon Press, Oxford 1962) p. 740ff.

[7] In his Charles F. Deems Lectures, The Logic of Religion, given at New York University in March, 1963.

language.[8] Carnap regards meaning postulates as in some sense *primitively* L-true or analytic. But this is an extension of the word 'analytic' as used here and by Church. Thus meaning postulates concerning 'God' need in no way become logical or analytic truths. Kneale's insistence therefore that we cannot properly speak of a logic of 'God' seems gratuitous, if meaning postulates germane to a given subject-matter are not regarded as analytic truths.

Of course the general characterization of logic as the study of valid deductive inferences is, as Kneale points out, somewhat vague. Precisely what are valid deductive inferences? Well, each of the vast multiplicity of well-formulated logics attempts to answer this question, each in its own way. One cannot single out one of these logics and say by *fiat* that the inferences it condones are just the valid ones. Nonetheless these various logics are not all on a par and good arguments can be adduced that the classical, two-valued logic of truth-functions, quantifiers, and perhaps identity (the so-called first-order logic mentioned above), is the fundamental or basic one.

IV

It seems to be a rather wide-spread opinion among philosophers that, in view of the vast multiplicity available, any one logic is as good as any other. Just take one's pick and, if none of them quite suits, make up a new one. But this rather cavalier attitude is surely not defensible. Why not reject physics also, on the grounds that there is not complete unanimity as to all details of wave theory, of field theory, or of the quantum theory, and to how they are interrelated? Or reject arithmetic on similar grounds?

Very roughly, some of the arguments that first-order logic is more fundamental than the alternatives (such as higher-order logics, modal logics, intensional logics, many-valued logics, etc.) are as follows.

[8] R. CARNAP, Meaning Postulates, in *Meaning and Necessity*, 2nd ed. (University of Chicago Press, Chicago 1956) pp. 222–229.

In the first place it is a direct and natural outgrowth of the logic of tradition, stemming from Aristotle and the Stoics and Scholastics. As Kneale himself notes, "... it is desirable that any new rules we adopt deliberately for old words should depart little, if at all, from previously established customs..." What is added should, in Hobbes' phrase, contain "no offensive novelty." This typically British attitude no doubt needs defense in general, but not, it would seem, in the present context.

First-order logic may be regarded as containing the conceptual core of other logics. Most of them may be viewed as arising from first-order logic by a modification of this or that, or by an extension or deletion here or there. Other logics are then viewed and characterized with respect to their similarities to and differences from first-order logic regarded as a kind of paradigm.

No doubt one can argue also for the simplicity and naturalness of first-order logic as over and against the alternatives. Also first-order logics may be formulated wholly extensionally, as we have already noted above, as contrasted with some of the alternatives, and hence enjoy the advantages of extensionality.

Another very important argument is the following. Every logic has a syntax and a semantics – in fact, as we have observed, *is* a syntax and a semantics in the sense that it is formulated within a syntactical or semantical meta-language. These meta-languages in turn may be formulated as formal systems and hence contain a logic as a part. This logic in turn is the classical logic, irrespective of whether that of the object-language is or is not. For example, syntax may be regarded as the theory of concatenation or logical spelling, even for intuitionistic or many-valued systems. That first-order logic always seems presupposed in the syntactical meta-language of any logic whatsoever provides a most telling argument. (However, not too much seems known about the exact logical character of the *semantical* meta-languages for systems other than the standard ones, so that this argument might need modification in the light of subsequent research, or rephrasing for this or that special system.)

Another interesting point is that, as a matter of historical fact, the standard first-order logic has been used more successfully in applications than any other kind of logic. Most of the interesting applications in the sciences are of this kind, as well as those in the methodology of the sciences. Modal logics, many-valued logics, intuitionistic logics, and the like, seem not to have been used very much or very fruitfully in such applications. And surely when philosophers worry about whether such and such an inference is valid, it is always, is it not, to first-order logic (whether they consciously realize it or not) that they turn for adjudication? Logic in the sense of the "logical geography of concepts," to use Ryle's apt phrase, seems to involve only the notions of first-order logic.

Each of these arguments should be developed much more fully and no one of them is by itself conclusive. They sum, however, to lending good support to the thesis of the fundamentality of first-order logic. If so, philosophers may well look to this kind of logic as providing the most suitable tools for their work.

V

We come now to the question as to whether the notion of identity belongs properly to the province of logic. Kneale says not, but his argument seems rather arbitrary. He says that "it seems most natural to present quantification theory in rules of inference like those of Gentzen" and "this technique cannot be extended to the theory of identity." Hence the theory of identity, he thinks, is not so intimately related to entailment or valid inference as conjunction, negation, etc., are. But of course we are not compelled to formulate quantification theory in terms of "natural" deduction any more than we are compelled to formulate arithmetic in some one given fashion. (And anyhow critics of natural deduction do not find it very "natural", especially when it comes to the so-called Rule of Existential Instantiation and its ilk).

A stronger argument against regarding identity as a notion of logic, not mentioned by Kneale, would be, I think, the following. Let 'a' and 'b' be primitive individual constants of some first-order L. Let '$\sim$' express negation. The sentence "$\sim a = b$" expresses then that a and b are distinct individuals. That this is the case would not seem to be a matter of logic but of the particular subject-matter dealt with. To say that the individual Socrates is distinct from the individual Plato is to utter a matter of historical fact, not an analytic truth of some theory of valid inference.

On the other hand, if '$x = y$' and '- -x- -' are schemata for theorems of L, then '- -y- -' is also, '- -y- -' differing from '- -x- -' appropriately. Suitable rules for identity may be added to the valid rules of quantification theory of L without losing completeness, as Kneale points out. If we leave out such rules, we are in effect omitting a rather important item in the study of valid inference. Should not the "spirit of generality in mathematics" induce us to include them here?

We must recall also that the notion of identity may be introduced into some L's by definition. Thus suppose there are only two primitive predicates of L, 'P' and 'Q' of one and two places respectively. Suppose further that not both of P and Q are universal or null or that one is universal and the other null. ('Universal' and 'null' here are of course ambiguous, but what is intended is clear enough.) We may then define

$$`x = y\text{' as }`(\mathrm{P}x \equiv \mathrm{P}y) . (z)(\mathrm{Q}xz \equiv \mathrm{Q}yz) . (z)(\mathrm{Q}zx \equiv \mathrm{Q}zy)\text{'}$$

without further ado, achieving herewith the full effect of identity within this L. (The '$\equiv$' and '.' and quantifiers are taken in the usual sense.) And of course this definition generalizes to L's with any finite number of primitive predicates, under suitable assumptions concerning their nullity and universality. Because as a matter of fact the primitive predicates of most L's satisfy the requirement concerning nullity and universality – and if they do not can be suitably transformed into such as do – we see that identity is already contained in quantification theory as developed for these L's. It seems doubtful, therefore, that Kneale is correct in excluding identity from the province of logic. The fact is that for the L's considered we have identity present whether we wish it or no.

We may conclude that not all logics are of equal value for the philosopher. Not all of them are equally sound, nor are all formulations equally acceptable. Mathematicians may continue to explore special features of this or that kind of system, but their results are likely to be of little philosophical interest. The standard, classical, first-order logic of truth-functions, quantifiers, and identity provides the most suitable philosophical tools, it would seem, and is presupposed in one fashion or another even in the discussion of the alternatives.

VI

It has been suggested above that syntax and semantics may themselves be viewed as applied first-order systems. That this is the case is a most important situation for philosophy and logical analysis, with significant bearings on ontology.

Logical analysis is preeminently concerned with the syntax, semantics, and pragmatics of language. Only rarely, however, are these three compartments of semiotic clearly distinguished from one another. Some philosophers even make a virtue of blurring them together indiscriminately, especially when natural language is concerned. Well, blurring has its merits and is often desirable, e.g., in playing Debussy, where subtle pedal effects are needed. But philosophy and logical analysis, like the great fugues of Bach, live and breathe with sharp distinctions, precise lines, and clear groupings. Hence it seems eminently desirable here to distinguish carefully among logical, factual, syntactical, semantical, and pragmatical rules or assumptions.

Logic in a wider sense is often taken to include syntax, semantics, and pragmatics. The question arises now as to how ontology is related to these. What is ontology? It is a quite separate subject? A further subdivision of logic? Or is it in some sense reducible to these others?

We have been following Quine in essentials in using 'ontology of *L*' and 'ontic commitment of *L*' to refer to the domain of objects taken as values for variables in *L*. This usage seems justified and is a mere sharpening of more or less traditional notions. Ontology as the science of being *qua* being is in its modern setting essentially the study of the objects "about" which our language or languages speak. The use of 'ontology' in this modern sense does no violence to traditional meanings. Rather it is a natural outgrowth of those meanings and of the concerns by which they are prompted.

Ontology enters into logic via semantics, which is the study of the relation between signs and objects, between words and things. To study such relations we must then, occasionally at least, make reference to objects as the relata of those relations. In semantics we are not interested in objects in any deeper sense, however, so that ontology there plays only a rather subsidiary role. It has been urged that the ontology of a semantical meta-language should involve no objects other than those of the object-language under discussion. (Cf. *Truth and Denotation*.) This is clearly essential for economy of ontic commitment. But it is also essential if the semantician is not to usurp the tasks of the ontologist. In marked contrast are the formulations of semantics in which all manner of new entities are admitted other than the values for variables of the object-language (or translation of such). The kind of formulation suggested above, presupposing only a first-order logic, involves no such objects and hence is strictly neutral with regard to further ontic commitment. Such neutrality should be part and parcel of the conception of semantics, just as of the conception of logic.

It is not the logician's task to settle problems of ontology, nor is it his place either to populate or de-populate the actual universe. He merely accepts what is given him and returns it in an ordered form. This is all – but this is enough. His tools should, then, enable him to do this in the simplest and easiest way. First-order logic and the syntax and semantics built upon it, are explicitly designed with this in view.

VII

Father Bocheński's interesting discussion, "The Problem of Universals," at Notre Dame in 1956, raises several issues, which it will be well to examine from the point of view put forward above.[9] Bocheński is concerned with the question "Are there universals?"

[9] University of Notre Dame Press, 1956.

primarily with regard to three "realms or levels of reality" – that of linguistic symbols, that of "objective meanings," and that of real entities.

By linguistic symbols he means "heaps of dry ink on paper, of chalk on blackboard, etc." It is quite clear thus that, for the purposes of his discussion, Bocheński requires an *inscriptional* syntax, and if a semantics is to be employed it will have to be an inscriptional semantics. The "real entities" are "what is given, either to sensual, or (through it) to direct intellectual apprehension." If we are to distinguish carefully object- from meta-language throughout, which we must surely do, the "real entities" will no doubt be the objects (values for variables) of the object-language and the linguistic symbols will be the values of the expressional variables of the meta-language. But where do the "objective meanings" fit in? This we must ponder.

Bocheński considers first the problem of universals for linguistic symbols. What is a *universal* symbol? Roughly, Bocheński says,

> For all x and L, x is a universal symbol of L if and only if there is at least one y, z, t, and u such that y and z are symbols of L, x, y, and z have the same shape, y is predicated of t and z of u, and t is distinct from u.

Bocheński is explicitly clear here that 'is a symbol of', 'has the same shape as' and 'is predicated of' must have meaning in the underlying syntax and semantics. The 'is predicated of' presumably represents the relation of (multiple) denotation. To say that y is predicated of t is to say, is it not, that y denotes t? (But one may query the bringing in of languages or language-systems here as values for variable and hence the need for the variable 'L').

It is interesting to compare this definition with the notion of being a *common* predicate constant, as given in *Truth and Denotation*[10]. An expression a is said to be common provided it is a predicate constant and denotes at least two distinct entities. A common expression is explicitly required there to be a predicate constant. Bocheński does not require a universal symbol to be a

[10] P. 105.

predicate constant, although it seems likely that in fact it would be. If x is predicated of y, then is not x a predicate constant? And if x is a non-null predicate constant, then there is a y of which it is predicated, is there not? Bocheński tells us nothing concerning the underlying syntactical and semantical axioms required for his discussion, but clearly there are universal or common terms or predicate constants, according to him and according to the theory of multiple denolation.

Are there objective meanings? And if so, what are they? And in particular, are there *universal* meanings? Bocheński distinguishes *syntactic* and *semantic* meaning as follows. "The meaning of a term may consist entirely in the set of syntactical rules concerning that term; this is the case in a formalistic language without interpretation." Semantic meaning is, in a similar way, determined by the syntactic and semantical rules together. Let us consider an example. What is the "syntactic meaning" of the term 'man'? First, note, it is a *set*, and, moreover, a set of *rules*, more specifically of syntactical rules. A syntactical rule is, presumably, a *statement* within the syntactical meta-language. A set of such statements is then a set, an expression for which could be found only in a meta-language of the syntactical meta-language (presumably a *semantical* meta-language of the syntactical meta-language, the latter being assumed interpreted.) The "syntactic meaning" of 'man' can be given then not in a meta-language but only in a meta-meta-language. And similarly for "semantic meaning." This is surely awkward and probably not intended. Also there is difficulty concerning 'concerning'. When does a rule "concern" a term and when not? Mere occurrence is not sufficient, for the term may occur vacuously.

As an incidental comment Bocheński states that "it seems that ... [higher level] functors [predicates or names of functions] and quantifiers occur *only* in formal sciences, and are not needed in [the] empirical sciences." Of course, Bocheński means to speak here only of formalized versions, of "rational reconstructions," of the language of the sciences. Taken literally this statement is false, for as a matter of fact most formalized or semi-formalized versions of the language of science do employ predicates and quantifiers of higher level. But let us insert 'need' between 'quantifiers' and 'occur' in Bocheński's statement. Let us call this result *Bocheński's thesis:* functors and quantifiers of higher level need occur only in the formal sciences, and are not needed in the empirical sciences. This is a most interesting thesis and may well be true.

Bocheński goes on to discuss objective meanings in connection with the Goodman-Quine nominalistic syntax.[11] He seems to say that Goodman and Quine "consider a given language as a purely formalistic system without attaching any interpretation, i.e., any semantic meaning, to its symbols. This has been proposed for logic and mathematics by Goodman and Quine, who suggest that we may consider them as a pure abacus." But that any language-system may be viewed purely syntactically was known long before Goodman and Quine. Further, their syntax is applicable to all systems whatsoever, although they consider in detail only a particular system for logic and mathematics. Also when we say that a system is "given without interpretation," we may wish to say merely that the system is "given" without semantical rules in the sense of Rules of Denotation or Rules of Truth. Or we may wish to say that the system is "given" without Rules of Meaning (in some Fregean sense). It seems that these two senses of "interpretation" are not always clearly distinguished.

Bocheński notes that "if we try to dispense with monadic name-determining functors [predicates], we fail and we *must* fail." As an example, he cites the predicate 'has the same shape as' which is needed in inscriptional syntax. He seems to suggest that Quine and Goodman do not recognize this circumstance and support "the opposite theory." But neither Quine nor Goodman, nor indeed anyone, it would seem, has seriously suggested that all primitive *predicates* may be dispensed with either in an object- or meta-language. In the language for inscriptional syntax, certain primitive predicates applicable to inscriptions are admitted, so-called *shape* predicates, enabling us to state that a given inscription has a given shape. But this is surely not to support the "opposite theory" that all "name-determining functors" may be dispensed with.

[11] See their Steps Toward a Constructive Nominalism, The Journal of Symbolic Logic **12** (1947) 105–122. Cf. also *Truth and Denotation*, pp. 227–245.

Bocheński states that "such words like 'the shape of tee' must be semantically meaningful, in order that we might recognize *which* heap of dry ink is a tee. And that means on our assumption that there are semantic meanings corresponding to the functors in the meta-language." But clearly we can and do often say that a given predicate is significant in a given language without assuming that there is a "semantic meaning" corresponding to it. A one-place predicate need not be regarded as *designating* a meaning but only as *denoting* the objects to which it applies. The Rules of Denotation supply all that is needed to give significance (an interpretation) to the primitive predicates.[12]

To show that meta-linguistic rules must be formulated with the help of "name-determining functors" Bocheński states the rule of *Modus ponendo ponens*. But the rule is formulated in terms of *assertion*. *Modus ponens* is not, ordinarily at least, taken to be a pragmatical rule concerned with assertion, but only a semantical or syntactical rule concerned with truth, logical truth, or theoremhood: If A and '$(A \supset B)$' are truths (logical truths, theorems), so is B a truth (logical truth, theorem). Stated in terms of assertion, the *Modus ponens* becomes an empirical statement concerning actual behavior, and may or not be true.

Contrary to the view suggested, however, Bocheński thinks that "when we state truly e.g., 'φ a' [where for the moment we may think of 'φ' as a one-place predicate constant and 'a' as an individual constant], there is something in the phenomenal world which corresponds not only to 'a', but also to 'φ'." Hence 'φ' must, according to him, be regarded as designating. Suppose there is no correlate to 'φ' in the phenomenal world. Then, Bocheński argues, "we must say that we do not say anything about that world. Because whatever we say [assuming for the moment that all individual constants are defined by means of Russellian descriptions], we do so with functors [predicates]. But we most certainly *do* say something about the phenomenal world. Consequently the ontology ... [which requires no such correlate] must be rejected.

[12] Cf. the author's On Denotation and Ontic Commitment, Philosophical Studies **13** (1962) 35–39.

The only alternative seems to be an extreme skepticism." But quite clearly if we regard 'φ' as *denoting* rather than as designating, we can have an interpreted linguistic framework in which we can talk about the world significantly and without skepticism.[13] Bocheński's argument, it would appear, fails to do justice to the semantics based on denotation, which – or some theory like it – seems essential, incidentelly, for Leśniewski's ontology as well as for that of Quine and Goodman.

To summarize. In this paper we have (1) put forth a few critical remarks concerning Church's intensional view of ontology and logic, (2) urged that the latter may be viewed as the syntax and semantics of scientific language, (3) put forth a few critical comments concerning Kneale's conception of the province of logic, (4) suggested several arguments as to why the classical, first-order logic should be regarded as the fundamental one, (5) reflected upon whether or not identity should properly be included in the list of logical notions, (6) recalled that syntax and semantics themselves may be formulated as first-order systems, (7) commented briefly on the role of ontology in semantics, and (8) reflected critically upon Bocheński's discussion of the problem of universals in its relation to certain formulations of semantics.

New York University, N.Y., USA

[13] If, in addition to Rules of Denotation, Rules of Meaning are thought needed, these may be given in terms of the notion of analytic truth for the language at hand. See the author's *Intension and Decision, A Philosophical Study* (Prentice-Hall, New York 1963).

CHAPTER 16

REFLEXIONS SUR LA METHODE DE TEILHARD DE CHARDIN[1]

NORB. M. LUYTEN O.P.

Peut-on parler de méthode chez Teilhard de Chardin, auteur déroutant s'il en fût, ce que prouvent les réactions extrêmes qu'il suscite, allant de l'enthousiasme délirant au refus le plus catégorique? [2] Ces attitudes contradictoires du public en face de son oeuvre, ne montrent-elles pas précisément un certain désarroi devant une pensée qui, tout en s'annonçant comme scientifique, sort des cadres de ce qu'on entend communément par science?

[1] Il peut sembler paradoxal, pour ne pas dire incongru, de consacrer une étude à Teilhard de Chardin dans un recueil dont la plupart des contributions risquent bien de se mouvoir sur le terrain de la logique la plus rigoureuse. Quelle figure va faire Teilhard, que l'on classe volontiers parmi les poètes ou les prophètes, dans cette ambiance toute imprégnée de l'austérité méthodique et de la rigueur scientifique, propres à la logique?

N'ayant aucune compétence dans le domaine logique, j'ai pourtant tenu à m'associer à l'hommage rendu à un collègue auquel me lient, en dehors de l'appartenance à une même faculté, des liens d'estime et d'amitié. L'éditeur de ce volume a été assez aimable d'estimer qu'une étude sur la méthode de Teilhard pourrait trouver sa place dans cette Festschrift. Encouragé par cet avis favorable, je me suis enhardi à offrir ces modestes pages en hommage à mon cher collègue jubilaire.

[2] Dans la littérature abondante sur Teilhard, plusieurs études ont été consacrées à sa méthode. Notons plus spécialement: C. d'ARMAGNAC, Philosophie de la nature et méthode chez le P. Teilhard de Chardin, in Archives de Philosophie **XX** (nouv. série) (1957) pp. 3–41; L. MALEVEZ, La Méthode du P. Teilhard de Chardin et la Phénoménologie, in NRT, t. 79, pp. 574-599; F. RUSSO, La Méthode du P. Teilhard de Chardin, in Recherches et Débats, cah. 40 (1962), pp. 13–23. Signalons encore le chapitre XV, Un renversement de méthode dans l'ouvrage de H. DE LUBAC, *La pensée religieuse du P. Teilhard de Chardin*. Il ne m'a pas semblé que ces differéntes études ren-

Il y a d'ailleurs d'autres indices qui semblent aller dans le même sens. Alors que le souci de la méthode a amené les différentes sciences à se distinguer nettement les unes des autres, l'œuvre de Teilhard relève de disciplines aussi diverses que la biologie et la morale, la physique et la théologie, la paléontologie et la sociologie. N'est-ce pas le signe évident que ce n'est pas une œuvre scientifique, mais une espèce de vision d'ensemble, dans laquelle tout est mêlé, au détriment évidemment de la rigueur méthodique.

Et, puisque nous parlon de vision, Teilhard est manifestement un intuitif, épris d'absolu. Il exprime l'intention de son œuvre dans la formule bien significative: "Voir et faire voir" (Le Phénomène humain, Prologue p. 25). Comme tout intuitif, il se sent porté vers les vues d'ensemble, les grandes synthèses, plus que vers les recherches méthodiques de détail. Ne signale-t-il pas lui-même comment, par tempérament déjà, il est avide d'absolu dès sa plus tendre enfance: "Aussi loin que je remonte dans mes souvenirs (dès avant l'âge de dix ans), je remarque en moi l'existence d'une passion nettement dominante: la passion de l'Absolu ... Le besoin de posséder en tout 'quelque absolu' était dès mon enfance l'axe de ma vie intérieure ... L'histoire de ma vie intérieure est celle de cette recherche, portant sur des réalités de plus en plus universelles et parfaites..." (*Mon Univers*, 1918, cf. fac-similé dans CUENOT, *Pierre Teilhard de Chardin*, entre les pp. 74 et 75). Quelqu'un qui avoue ainsi avoir été guidé dans ses recherches par cette " passion de l'absolu", ne risque-t-il pas d'être bien plus un passionné qu'un scientifique?

Ce qui rend cette question encore plus pressante, c'est que toute la pensée de Teilhard s'insère dans une perspective religieuse. Par toute sa personnalité, Teilhard est un homme profondément religieux.[3] Il ne laisse aucun doute sur l'inspiration religieuse, et

daient superflues une réflexion sur la méthode de Teilhard, telle que je la présente ici. Je m'en tiens aux textes mêmes de Teilhard, en tâchant d'en dégager les aspects méthodologiques principaux. Une discussion plus ou moins polémique des vues avancées dans les articles cités aurait, à mon avis, nui à une présentation claire et critique. Le lecteur jugera de la légitimité de ce point de vue.

[3] Voir surtout les textes d'une belle élévation religieuse, comme par exemple *Le Milieu divin*, et *La Messe sur le Monde*, dans Hymne de l'Univers.

même l'intention apostolique de sa recherche scientifique. Invité à exposer en quelques pages l'essentiel de sa pensée, il présente sa " Physique" (nous reviendrons sur ce terme), comme débouchant sur une apologétique et une mystique. (cf. Texte publié dans P. GRENET, *Teilhard de Chardin, un évolutionniste chrétien*, p. 191–3). Et dans une lettre à son Supérieur Général, il souligne avec insistance le ressort religieux de toute sa recherche: " Depuis mon enfance, ma vie spirituelle n'a pas cessé d'être complètement dominée par une sorte de "sentiment" profond de la réalité organique du Monde; sentiment originairement assez vague dans mon esprit et dans mon cœur ... mais sentiment graduellement devenu avec les années, sens précis et envahissant d'une convergence générale sur soi de l'Univers; cette convergence coïncidant et culminant, à son sommet, avec Celui in quo omnia constant." (P. Grenet, o.c. p. 213). Cette interpénétration voulue de recherche scientique et de convictions religieuses, n'enlève-t-elle pas d'emblée à la première toute garantie d'objectivité? Un esprit guidé par un tel parti-pris, peut-il être considéré comme un véritable homme de science?

On voit, les raisons ne manquent pas pour opposer d'entrée en matière une fin de non-recevoir à Teilhard, scientifique.

Et pourtant. Sommes-nous sûrs de pouvoir juger et classer le cas Teilhard au vu de ces objections, toutes péremptoires qu'elles puissent nous sembler? Après tout, Teilhard a fait ses preuves en paléontologie, et d'avoir été mêlé d'assez près à la découverte du Sinanthropus Pekinensis n'est pas précisément un certificat d'ignorance. Les titres qu'il peut faire valoir dans son activité scientifique au sens courant du mot, invitent peut-être quand même à ne pas lui refuser a priori notre audience lorsqu'il prétend élargir et renouveler la recherche scientifique. Lui opposer d'emblée une fin de non-recevoir, parce que sa synthèse englobe des domaines que notre compartimentement scientifique a soigneusement séparés, c'est contester assez gratuitement la possibilité de modifier en quoi que ce soit la situation actuelle. Teilhard n'ignore pas qu'il sort des conceptions généralement acceptées, mais il croit avoir de bonnes raisons pour le faire. La vraie attitude critique dès lors n'est pas de lui tourner le dos, ne voulant rien entendre de ce qui choque nos conceptions courantes, mais bien d'examiner si vraiment sa conception nouvelle apporte du valable. Au même titre, il ne suffit pas de dénoncer l'inspiration religieuse de sa pensée, pour la discréditer automatiquement. Comme homme de science, Teilhard était bien conscient de cette connivence, chez lui, entre sa conviction religieuse et sa pensée scientifique. Loin de penser que la première gênerait la seconde, il était profondément convaincu que la perspective religieuse l'avait aidé à découvrir scientifiquement les dimensions synthétiques du réel. Ceux qui lui font grief de son inspiration religieuse et y voient d'emblée une raison suffisante pour ne pas le prendre au sérieux, risquent fort de céder à une réaction affective aussi peu scientifique que celle qu'ils prétendent rejeter. Peut-être un examen critique révèlera-t-il l'insuffisance méthodologique – et donc scientifique – du projet teilhardien. En tout cas, ce n'est qu'après un examen critique qu'on pourra en décider, et il serait singulièrement anti-scientifique de rédiger un certificat d'insuffisance sans avoir pris la peine d'examiner de plus près le bien-fondé de ses prétentions. Teilhard prétend parler en scientifique. Il présente son œuvre maîtresse comme un "mémoire scientifique" (cf. Le Phénomène humain, Avertissement, p. 21). Accordons lui au moins le bénéfice du doute, et tâchons de voir comment il entend justifier cette prétention.

Nous savons déjà que sa conviction religieuse l'a sensibilisé aux problèmes de signification et de synthèse. Mais, sans vouloir ignorer ce facteur, tâchons de voir l'éclosion au plan même de la recherche scientifique, de la nouvelle conception qui va faire sauter les cadres des méthodes scientifiques généralement adoptées.[4]

Deux facteurs surtout, qu'il tient de sa recherche paléontologique, me semblent avoir joué ici un rôle déterminant: la perspective évolutionniste et l'anthropocentrisme. Inutile d'insister que la paléontologie implique la perspective évolutionniste. Ce qui veut dire que chaque élément de la recherche est automatiquement vu dans le contexte d'un immense mouvement de synthèse, l'évolution n'étant autre chose que cette synthèse en train de se faire.

[4] La synthèse élaborée par Teilhard mêne jusqu'aux problèmes théologiques. Sans vouloir minimiser cet aspect de son œuvre, nous en faisons abstraction dans ces pages, limitant le sujet aux dimensions qui entrent plus spécialement dans nos compétences.

Or, à travers son travail de paléontologue, Teilhard découvre l'homme comme le point de convergence de cet immense processus évolutif. Et, notons-le bien, cet homme n'est pas seulement celui dont il rencontre les restes fossilisés, dans telle ou telle fouille. C'est également, et même davantage, l'homme qui vit aujourd'hui. Car, ceci est pour Teilhard une conviction inébranlable, si nous pouvons déchiffrer les fragments que le passé nous a légués, c'est en fonction du présent que nous avons sous les yeux. Le passé et le présent ne sont que deux stades d'un même mouvement évolutionniste. C'est pourquoi le paléontologue qui se concentre sur ses fossiles et oublie de considérer le présent, tronque singulièrement les possibilités d'explication de sa science.[5]

L'étude minutieuse de restes fossiles pourra être si exacte que l'on veut, jamais elle ne nous apprendra ce qu'un simple coup d'œil sur l'homme actuel nous révèle. La cohérence des choses devient donc, non simplement un principe heuristique, mais encore un puissant moyen d'explication de la réalité, qui, par structure intime, est un immense processus de synthèse. Et cette synthèse que l'évolution nous présente en train de se faire, n'est pas une pure somme arithmétique, ou un agencement tout mécanique; c'est un processus dans lequel – comme dans chaque organisme individuel – un sens devient visible. Voilà donc deux principes méthodologiques majeurs que Teilhard va mettre en œuvre: le principe de la cohérence; le principe du sens, qui se révèle dans l'homme.

Avant de nous attacher à un examen un peu plus approfondi de ces deux principes, esquissons brièvement l'éclosion de cette pensée, telle que nous pouvons la suivre dans les notes et les œuvres de Teilhard.

Dans une lettre du 14 janvier 1924, Teilhard formule chairement l'idée d'une science plus synthétique que ne l'est la géologie reçue. Ecoutons-le: "C'est encore l'âge d'or pour les recherches géologiques, en Chine... Tout de même, ici comme ailleurs, l'inventaire des

[5] La synthèse de Teilhard embrasse même, comme on le sait, l'avenir. C'est d'ailleurs surtout dans cette direction qu'interviennent ses considérations théologiques. C'est pourquoi, conformément à ce que nous disions dans la note précédente, nous n'aborderons pas cette dimension – d'ailleurs la moins scientifique – de la synthèse teilhardienne.

couches et des fossiles ne tardera pas à être terminé dans ses grandes lignes, et je me dis souvent que dans une génération, la Science dont je m'occupe commencera à se faner si on n'arrive pas à agrandir son objet, à changer ses méthodes. Il doit y avoir un moyen d'aborder l'étude de la Terre d'une façon plus profonde et plus synthétique en la considérant comme *un tout*, doué de propriétés mécaniques, physiques, chimiques, *spécifiquement terrestres*. Ce sont ces propriétés qu'il faudrait arriver à dégager, me semble-t-il, au lieu de s'acharner à suivre, dans le détail, l'action de causes minimes qui n'influencent que d'infimes parties de la Terre. Si j'avais à refaire ma vie, je m'orienterais vers la géo-dynamique ou la géochimie." (cf. CUENOT, *Pierre Teilhard de Chardin*, p. 73). Ce texte est bien révélateur de la direction dans laquelle Teilhard cherche. Il espère peu d'une *recherche de détail* toujours plus poussée. Pour faire avancer sa science et empêcher qu'elle ne *se fane*, il prévoit qu'il faudra *agrandir l'objet* et *changer les méthodes*, arriver à une étude *plus profonde et plus synthétique*. Nous avons ici déjà clairement esquissées les grandes perspectives dans lesquelles va s'élancer la pensée de Teilhard. Une seule chose, à vrai dire la plus importante, manque encore à ce projet d'élargissement et de synthétisation de sa science, c'est la signification centrale de la réalité humaine. Pour le moment Teilhard pense encore à une synthèse qui se ferait en fonction des propriétés mécaniques, physiques, chimiques spécifiques de la Terre considérée comme un tout. Il ne tardera pas à découvrir que seule la dimension humaine permet d'organiser autour d'elle tous les processus de l'univers matériel dans une immense synthèse. Dans une lettre du 31 décembre 1926, nous voyons se dessiner cette nouvelle perspective: "Intellectuellement, je suis toujours très intéressé et absorbé par les recherches techniques de géologie, dans un domaine et dans un pays encore pleins de choses à découvrir. Pourtant, depuis deux ans surtout, j'ai l'impression d'être graduellement attiré vers l'étude de l'Humanité, non pas préhistorique, mais *présente*. Je conçois de mieux en mieux l'Homme comme *le grand phénomène terrestre*, celui en lequel culminent les plus grands phénomènes géologiques, et le plus vaste mouvement de la vie. Autrement dit, je découvre des prolongements humains à la Géologie... Les géologues, je ne sais pourquoi, considèrent toutes les sphères concentriques dont est formée la Terre, sauf une: celle formée par la couche humaine pensante; et ceux qui s'intéressent à l'Homme sont généralement étrangers à la Géologie. Il faudrait unir les deux points de vue." (cf. Cuénot, o.c. p. 95–6). Les lignes de force commencent à se dessiner plus clairement. Ce n'est plus seulement en introduisant des considérations plus synthétiques en géologie que Teilhard compte réaliser la synthèse, dont il a entrevu la nécessité impérieuse, mais en ouvrant la géologie sur l'homme, en découvrant "des prolongements humains à la géologie". On sent cette idée à l'œuvre; dès à présent, elle oriente ses recherches: "J'ai en projet un travail sur l'Homme – non pas l'Homme préhistorique précisément, mais l'Homme regardé comme le plus grand événement tellurique et biologique de notre planète. Je suis de plus en plus persuadé que nous sommes aussi aveugles sur la couche terrestre humaine que nos aïeux l'étaient sur les montagnes et les océans. Et cette considération devient graduellement la dominante de mes réflexions scientifiques. Il y a, autrement dit, des prolongements humains de la géologie qu'il faudrait commencer à dégager". (Cuénot, o.c. p. 96, note 1).

C'est autour de cette idée centrale que va s'organiser la synthèse Teilhardienne, telle qu'elle a été élaborée dans son "phénomène humain". Notons que, en 1928 déjà, Teilhard avait rédigé quelques feuilles dactylographiées sous ce titre. En 1930, c'est devenu un article publié dans la Revue des questions scientifiques (**18** (1930) p. 390–406, publié dans La vision du passé p. 227–243), pour devenir de 1938 à 1940 le livre que plusieurs considèrent comme son chef-d'œuvre. L'idée d'abord entrevue d'un prolongement humain de la science géologique a fait son chemin, à tel point que le "phénomène humain" non seulement est intégré à la science, mais en devient la pièce maîtresse, puisque seulement en lui et à travers lui la lecture synthétique du réel devient possible. Cette vue cohérente de tout le réel dans et à travers la réalité humaine est certainement la contribution originale de Teilhard à la science. C'est ici évidemment que se posent les problèmes de méthode que la réalisation d'un tel projet impliquent.

Tâchons donc d'insister sur ce point pour découvrir la démarche de sa pensée et nous faire une idée sur sa valeur.

D'emblée Teilhard, par la façon dont il a entamé le problème, a sauté l'obstacle qui, à ses yeux barrait le chemin à une synthèse scientifique du réel. L'homme, comme réalité spirituelle, était considéré comme irréductible au monde physique et organique. Par le fait même, toute idée de science embrassant la réalité matérielle et l'homme dans une même vue synthétique semblait exclue. Or, ce qui apparaissait ainsi comme impossible, devient pour Teilhard nécessité impérieuse. Puisque la synthèse n'est possible qu'à ce prix, et puisque, d'autre part, elle est nécessaire si nous voulons comprendre la cohérence du réel, il faut donc que cette intégration de l'homme et du monde physique et organique soit possible. Cette possibilité pour Teilhard se fonde sur ce qui lui apparaît comme une évidence fondamentale d'une simplicité presque déroutante: l'homme est un phénomène au même titre que tous les autres phénomènes que les sciences ont coutume d'étudier. De fait, c'est le phénomène de loin le plus marquant qui nous frappe dans notre monde. Un premier impératif sera donc d'accueillir le phénomène humain comme un élément irrécusable de notre réflexion scientifique. "Pour rendre acceptable le Phénomène humain, et lui permettre de manifester sa fécondité scientifique, la première condition est de ne pas biaiser avec lui, ni de le minimiser. L'Homme n'est aussi troublant pour la science que parce que celle-ci hésite à l'accepter avec la plénitude de sa signification, c'est-à-dire comme l'apparition, au terme d'une transformation continue, d'un état de la Vie absolument nouveau. Reconnaissons franchement, une bonne fois, que, dans une perspective réaliste de l'histoire du Monde, l'avènement du pouvoir de penser est un événement aussi réel, aussi spécifique et aussi grand que la première condensation de la Matière ou la première apparition de la Vie: et nous verrons peut-être, au lieu du désordre redouté, une harmonie plus parfaite s'étendre sur nos représentations de l'Univers". (article Le Phénomène humain, La vision du Passé, p. 234).

Longtemps la science s'est arrêtée devant cette "barrière" de la pensée, ne voulant pas l'accepter de plein pied comme un phénomène irrécusable. Ecoutons le diagnostic de Teilhard: "Soit par peur chez les uns de tomber dans la Métaphysique, soit par crainte chez les autres de profaner l'âme en la traitant comme un objet de simple Physique, l'Homme, dans ce qu'il a de spécial et de révélateur pour notre expérience, c'est-à-dire, dans ses propriétés dites 'spirituelles' est encore exclu de nos constructions générales du Monde. D'où ce fait paradoxal: il y a une Science de l'Univers sans l'Homme. Il y a aussi une connaissance de l'Homme en marge de l'Univers, mais il n'y a pas encore une Science de l'Univers étendue à l'Homme, en tant que tel. La Physique actuelle (en prenant ce mot au large sens grec de 'Compréhension systématique de toute la Nature') ne fait encore aucune place à la Pensée; ce qui veut dire qu'elle est construite entièrement en dehors du plus remarquable des phénomènes présentés à notre observation par la nature" (l.c. p. 228).

Teilhard ne se contente pas d'un tel diagnostic. Si vraiment la situation est telle qu'il l'a décrite, il est impérieux de réagir et de procéder sans biaiser à la construction d'une science synthétique du réel. Il n'hésite donc pas à tirer la conclusion: "Nous voudrions, dans ces pages, réagir contre une situation aussi anti-scientifique, en esquissant . . . les contours possibles d'un Univers où les propriétés spécifiquement humaines (Réflexion et Pensée) seraient introduites, comme une sorte de dimension nouvelle" (l.c. p. 229).

Nous croyons ce texte très révélateur quant à la position du P. Teilhard. S'il nomme la situation dans laquelle il trouve les sciences "anti-scientifique", c'est évidemment en vertu d'un critère qui lui permet de juger de ce qui est scientifique et de ce qui ne l'est pas. Quel est ce critère?

Il est clair, tout d'abord, que Teilhard ne conteste pas le caractère scientifique des sciences existantes. Bien au contraire. Non seulement ce caractère vraiment scientifique n'est jamais mis en doute, mais encore Teilhard fait constamment appel au paradigme de la physique pour documenter l'idéal d'une vraie science. Ce n'est donc pas par ce qu'elles sont et font que les sciences sont dans une situation anti-scientifique, mais plutôt par ce qu'elles ne font pas. Et ce péché d'omission consiste précisément à exclure de la science ce qui selon les règles méthodologique devrait y être intégré. Si donc Teilhard n'est pas satisfait de la situation en science et s'il veut la changer, ce n'est pas qu'il conteste la validité des canons méthodiques en vigueur. Au contraire. C'est en se basant sur ces canons méthodologiques qu'il veut montrer la nécessité pour la science d'englober des éléments du réel, qu'elle avait jusque là négligés. Il ne s'agit donc nullement d'une réforme de la science en recourant à de nouvelles méthodes, mais bien d'un élargissement en allant au bout des méthodes en vigueur. Plus spécialement, pour concrétiser immédiatement cette pensée: la science ne doit pas recourir à une nouvelle méthode pour englober l'homme dans ses recherches; elle doit tout simplement appliquer ses méthodes à la réalité humaine. Ceci implique évidemment que cette réalité humaine est atteignable à travers les méthodes admises en science. C'est bien ce que prétend Teilhard, et ce qu'il veut souligner en parlant du "phénomène humain". Il note explicitement avoir délibérément choisi ce terme "pour affirmer que l'Homme, dans la Nature, est véritablement un fait, relevant (au moins partiellement) des exigences et des méthodes de la Science". (Le Phénomène humain p. 29). Ou encore, dans son article de 1930: "Par l'expression "Phénomène humain", nous entendons ici le fait expérimental de l'apparition, dans notre Univers, du pouvoir de réfléchir et de penser" (Vision du Passé, p. 227).

Un objet de science doit être un fait expérimental, un phénomène. Teilhard identifie: Objet de Science pure et phénomène (Vision du Passé, p. 228). Rappelant le caractère scientifique de son œuvre (ce qu'il ne cesse de faire), il insiste de préférence sur le caractère expérimental de ses démarches. "Je ne quitte à aucun moment le terrain de l'observation scientifique" (Avenir de l'Homme, p. 161) ... "sans quitter le domaine des faits scientifiques" (Energie humaine, p. 69–70). Et en présentant son Phénomène humain "uniquement et exclusivement comme un mémoire scientifique" il justifie cette revendication par sa limitation au phénomène: "Rien que le Phénomène. Mais aussi tout le Phénomène" (Le Phénomène humain, p. 21). C'est d'ailleurs ce qu'il estime être sa grande trouvaille, grâce à laquelle une science synthétique de l'Univers, homme compris, peut enfin être envisagée: la science ayant, de par sa nature, le phénomène pour objet, elle se doit d'englober l'homme dans sa synthèse, puisque ce dernier est un phénomène au sens le plus strict du mot. Il n'est pas étonnant de voir Teilhard insister sur ce caractère phénoménal de l'homme, puisque c'est là l'affirmation la plus centrale et aussi la plus révolutionnaire de son système.

Et pour qu'on ne s'y méprenne pas, pour qu'on n'entende pas cette affirmation de la pure apparition extérieure de l'homme, il spécifie clairement qu'il s'agit de l'homme en ce qu'il a de plus propre: sa pensée, son pouvoir de réflexion. Il parle du "fait expérimental de l'apparition, dans notre Univers, du pouvoir de réfléchir et de penser" (Vision du Passé, p. 224). "Pendant longtemps, il n'y a pas eu de Pensée sur terre. Maintenant il y en a, et tellement que la face des choses se trouve entièrement changée. Nous nous trouvons vraiment là en présence d'un objet de Science pure, d'un phénomène" (ibid. p. 227-8).

Mais déjà ici un doute s'annonce. Est-ce la pensée elle-même qu'on peut dire phénomène, ou est-ce son empreinte sur les choses matérielles? Teilhard n'hésite pas un instant. Sa façon de s'exprimer montre bien à quel point il propose la pensée en tant que telle comme phénomène expérimentalement observable. Citons quelques-uns des passages les plus caractéristiques. "On a remarqué que, vue à très grande distance, la Terre, couverte de ses végétaux et de ses océans doit paraître verte et bleue. Pour un observateur lointain qui saurait mieux la déchiffrer, elle paraîtrait, en ce moment, lumineuse de Pensée" (l.c. p. 231; cf. texte parallèle dans: Le Phénomène humain, p. 202). L'ambiguité d'une telle position ressort encore davantage de la suite du texte: "Du point de vue le plus froidement positiviste qui soit, le Phénomène humain ne représente rien moins qu'une transformation générale de la Terre, par établissement, à la surface de celle-ci, d'une enveloppe nouvelle, l'enveloppe pensante, plus vibrante et plus conductrice, en un sens, que tout métal; plus mobile que tout fluide; plus expansive que toute vapeur; plus assimilatrice et plus sensible que toute matière organisée" (l.c. p. 231). A lire un pareil texte, on comprend que Teilhard ait été qualifié de poète. Seulement la poésie n'est pas de mise à un point si critique de la structure de sa synthèse. D'ailleurs, Teilhard n'entend nullement faire de la poésie. Il estime parler " du point de vue le plus froidement positiviste qui soit". La chose n'en devient que plus inquiétante. Si sympathique que puisse être l'idée d'une science élargie, débordant les cadres trop étroits de disciplines cloisonnées à l'extrême, si la nouvelle synthèse doit être bâtie sur une notion si peu différenciée de phénomène, si massivement univoque, le remède risque bien d'être pire que le mal que l'on veut combattre.

Il semble bien que Teilhard, emporté par son idée d'une science universelle, à la mesure du processus universel que constitue l'évolution de l'univers, n'ait vu d'autre issue qu'une univocisation de tout le réel. En admettant ainsi une homogénéité de base il rendait possible du fait même l'" explication cohérente et homogène du monde" (lettre du 11 octobre 1936, Cuénot, o.c., p. 264–5), qu'il cherchait. Cette science universalisée devient une Physique généralisée, puisque l'homme est devenu un phénomène, assimilable en principe à n'importe quel phénomène physique. En 1938, il écrit par exemple: " Je serais heureux de vous voir (comme je m'y essaie moi-même), pénétrer ... dans les questions spirituelles et humaines avec les méthodes de la Science, de manière à substituer aux Métaphysiques dont nous mourons une Ultraphysique (la vraie Physikè des Grecs, j'imagine) où Matière et Esprit seraient englobés dans une même explication cohérente et homogène du Monde" (Lettre du 11 oct. 1936, cf. Cuénot, o.c., p. 264–5). En septembre 1952, il se dit " préoccupé de l'urgence qu'il y aurait à opérer une 'réforme' de l'Anthropologie en séparant enfin celle-ci des 'Lettres' ou de la 'Philosophie' pour en faire, par son essence (analyse et étude de l'Humain), – comme on dirait 'de l'Hydrogène', ou 'de l'Uranium'-, un prolongement authentique de la Physique et de la Biologie" (cf. Cuénot, o.c., p. 428). Et encore: " Il faut à tout prix dégager enfin une véritable discipline étudiant l'Homme, comme 'une force de la Nature' (étudiant le 'Réfléchi', comme on étudie le 'Nucléaire')", ibid. note 4. Malgré toute la compréhension que l'on peut tâcher d'avoir pour le dessin teilhardien d'une science universelle, on reste perplexe devant de telles formules. Que peut bien être cette physique de l'esprit? Comment traiter l'humain comme l'Hydrogène ou l'Uranium? On a nettement l'impression que le désir de synthèse a conduit ici à une nivellation indue du réel, qui, au fond, n'est que verbale.

Il est assez curieux que Teilhard, qui se sait lui-même "naturaliste plus que physicien" (Le Phénomène humain, p. 33), préfère appeler sa synthèse une Physique (cf. Grenet, o.c., p. 192), une Hyperphysique (Lettre du 29 avril, Cuénot, p. 264; lettre du

3 décembre 1954, Cuénot, p. 428), une Ultraphysique (Lettre du 11 octobre 1936, Cuénot, o.c., p. 264), un prolongement de la Physique (Lettre du 11 février 1952, Cuénot, p. 425; lettre du 21 septembre 1952, Cuénot, p. 428). (Notons d'ailleurs que parfois il propose Hyper-biologie comme alternative, p. ex. lettre du 29 avril 1934). Comment expliquer chez un paléontologue cette préférence pour la physique? Il me semble qu'on peut avancer deux raisons. D'abord, pour Teilhard, comme pour beaucoup de ses contemporains, la physique est et reste *le* paradigme de la science expérimentale. Une discipline est science pour autant qu'elle s'inspire de ce paradigme et se construit selon des méthodes semblables. Or, Teilhard veut être scientifique: c'est pour lui plus qu'une consigne méthodologique, c'est un point d'honneur! On comprend donc qu'il tient à ce que sa synthèse ait vraiment le caractère d'une physique, c'est-à-dire qu'elle soit une vraie science.

Mais une autre raison semble avoir joué. Le terme "physique" permet le rapprochement avec la "Physikè" des Grecs. Il conçoit son Hyperphysique comme "le vraie Physikè des Grecs" (lettre du 11 octobre 1936). Il explique qu'il veut prendre Physique "au large sens grec de compréhension de toute la Nature" (Vision du Passé, p. 229). Quelle est cette Physikè des Grecs, et en quoi Teilhard y voit-il un modèle pour sa synthèse. Il semble bien qu'il faille surtout penser ici à la Physique d'Aristote, avec, sans doute, comme toile de fond, la réflexion philosophique des Ioniens autour de la notion de Physis. Nous trouvons là en effet un "effort de compréhension de toute la nature": une conception globale et synthétique de la nature, basée sur une observation, évidemment rudimentaire.

On peut s'étonner de voir Teilhard s'inspirer d'idées si divergentes que celles de la physique moderne et celle de la philosophie naturelle des Grecs. Manifestement, c'est "l'effort de compréhension de toute la nature" (Vision du Passé, p. 229) qui l'attire chez les Grecs. Mais la physique moderne, avec ses exigences méthodologiques d'exactitude et de vérification expérimentale minutieuse, n'est-elle pas aux antipodes de ces synthèses généreuses, qui n'étaient possibles qu'aussi longtemps que les exigences expérimentales étaient encore rudimentaires? Sans doute les exigences méthodologiques ont-elles amené la physique, et les sciences expérimentales en général, à se limiter à l'observable. Mais précisément, Teilhard estime que la science a été trop craintive et qu'elle a laissé de côté une partie importante de l'observable; du phénomène comme il aime le nommer.

Mais il y a plus. Toute science de l'observable qu'elle est, la Physique, ni aucune vraie science, ne saurait se limiter à l'enregistrement de purs faits. Une science sans mise en œuvre d'éléments de synthèse est une absurdité. Les physiciens réfléchissant sur leur science, ont été les premiers à le souligner. La critique des sciences a fortement relevé l'élément synthétique (d'ailleurs diversement interprété) dans la construction de la science.

Teilhard le sait bien, et il ne manquera pas d'exploiter cette idée au profit de sa synthèse. S'il veut être disciple du phénomène, il prend soin de spécifier que c'est "tout le phénomène" qu'il entend capter dans sa synthèse. "Depuis quelque cinquante ans, la critique des Sciences l'a surabondamment démontré, il n'y a pas de fait pur; mais toute expérience, si objective semble-t-elle, s'enveloppe inévitablement d'un système d'hypothèses, dès que le savant cherche à la formuler. Or, si à l'intérieur d'un champ limité d'observation cette auréole subjective d'interprétation peut rester imperceptible, il est fatal que *dans le cas d'une vision étendue au Tout*, elle devienne presque dominante", (Le Phénomène humain, p. 22). Et encore: "Ç'aura été une candeur, probablement nécessaire, de la Science naissante, de s'imaginer qu'elle pouvait observer les phénomènes en soi, tels qu'ils se dérouleraient à part de nous-mêmes. Instinctivement, physiciens et naturalistes ont d'abord opéré comme si leur regard plongeait de haut sur un Monde que leur conscience pouvait pénétrer sans le subir ni le modifier. Ils commencent maintenant à se rendre compte que leurs observations les plus objectives sont toutes imprégnées de conventions choisies à l'origine, et aussi des formes ou habitudes de pensée développées au cours du développement historique de la Recherche. Parvenus à l'extrême de leurs analyses, ils ne savent plus trop si la structure qu'ils atteignent est l'essence de la Matière qu'ils étudient, ou bien le reflet de leur propre pensée. Et simultanément, ils s'avisent que, par choc en retour de leurs découvertes, eux-mêmes se trouvent engagés, corps et âme, dans le réseau des relations qu'ils pensaient jeter du dehors sur les choses, pris dans leur propre filet" (Le Phénomène humain, p. 26). Ce dernier passage pourrait faire allusion aux conceptions exposées par Eddington dans sa "Philosophy of Physical Science". Teilhard n'a pas tort de couvrir son effort de synthèse par des témoignages qui lui viennent de la physique, la science la plus exacte et la plus positive qui soit. Et beaucoup de scientifiques qui reprochent à Teilhard ses synthèses "subjectives" feraient bien de réfléchir un peu au statut assez précaire de leur propre science. Ceci étant dit, il faut quand même constater combien Teilhard reste dans le vague dans cette justification de son effort de synthèse.

Déjà en parlant de sa fidélité au phénomène, il s'était bien gardé de spécifier plus exactement ce qu'il fallait entendre par "phénomène", ce qui lui permet de confondre sous un même terme l'observation d'un fait extérieur et la réalité de notre propre pensée; la constatation d'un fait isolé et la "saisie" d'un mouvement d'ensemble, tel que la "dérive" évolutive vers l'Esprit. Nous retrouvons ici le même à peu près. Il parle d'un "système d'hypothèses", d'une "auréole subjective d'interprétation", d'observations objectives "imprégnées de conventions choisies à l'origine, de formes ou d'habitudes de pensée" (cf. les textes cités). On avouera que tout cela est peu précis et ne nous renseigne pas outre mesure sur la nature et la valeur de l'élément de synthèse qu'il entend mettre en œuvre dans sa construction. Si nous ajoutons que tout cela nous est présenté comme du "subjectif" qui vient auréoler l'expérience "objective", la situation épistémologique, il faut l'avouer, n'en devient pas plus limplide.

Tout ceci aurait pu être un point de départ valable, à partir duquel, par une réflexion critique exigeante, on aurait pu dégager des règles de méthode, concernant les possibilités d'extrapolation et de généralisation admissibles dans les différents domaines de la science. Cet examen critique, Teilhard ne l'a pas fait. C'est ce qui explique qu'il se lance, tête baissée, dans des généralisations, dont le moins qu'on puisse dire, est que leur gratuité saute aux yeux. Qu'on pense, p. ex. à sa fameuse loi méthodologique d'extrapolation: découvrir l'universel sous l'exceptionnel, qu'il formule lui-même: "Bien observé, fût-ce en un seul point, un phénomène a nécessairement, en vertu de l'unité fondamentale du Monde, une valeur et des racines ubiquistes". Et l'application bien connue qu'il en fait: "La conscience apparaît avec évidence dans l'Homme ... donc, entrevue dans ce seul éclair, elle a une extension cosmique" (Le Phénomène humain, p. 52).

Qu'on analyse de près la démarche de la pensée teilhardienne tout le long de cette extrapolation géante. En vain y chercherait-on une consigne méthodologique précise. Outre l'exemple du Radium, et la remarque assez péremptoire – "nous l'avons trop souvent expérimenté dernièrement pour pouvoir en douter encore – : une anomalie naturelle n'est jamais que l'exagération, jusqu'à en devenir sensible, d'une propriété partout répandue à l'état insaisissable" – auncune justification, aucune précision n'est fournie. Ce qui ne l'empêche pas de conclure: "Je suis incapable de voir comment en bonne analogie avec tout le reste de la Science, nous saurions y échapper" (Le Phénomène humain, p. 52). Il est permis de douter si vraiment l'analogie est tellement bonne. On aurait de la peine de trouver dans "tout le reste de la Science" une extrapolation, basée sur des indices si minces que ceux dont dispose Teilhard ici. A vrai dire, il n'y a même aucun indice. En dernière analyse, le tout repose sur le postulat d'une "unité fondamentale du Monde" (l.c.) Or, on veut bien qu'un essai d'interprétation globale du monde, parte forcément de l'idée que le monde constitue un système cohérent. Mais quant à prétendre que cette cohérence entraîne nécessairement une homogénéité, du style de la "conscience, dimension universelle", ceci est une affirmation absolument gratuite, un postulat théorique que rien dans l'expérience ne justifie. Que Teilhard continue à parler ici d'un "point de vue phénoménal, qui est "*le* point de vue de la Science" (Le Phénomène humain, p. 54), qu'il ose même écrire: "Considérée sous l'angle purement expérimental, la Conscience se manifeste comme une propriété cosmique de grandeur variable, soumise à une transformation globale" (o.c., p. 56), montre à quel point il est lui-même victime de ce qu'il pense être une application de la méthode scientifique. A dire vrai, il se rend très bien compte qu'il va plus loin que n'importe quel homme de science ne l'a fait avant lui. Mais il pense rester intégralement dans la ligne de la méthodologie scientifique.

Il est vrai qu'aussi bien dans l'article sur le Phénomène humain que dans le livre du même titre, il présente sa synthèse comme une tentative toute provisoire. Et il ajoute: "Elle risque de paraître, à certains, développement poétique plutôt que système de faits solidement assemblés. Mais qui saurait dire jusqu'à quel point une séduisante harmonie n'est pas le charme naissant et le signe avant-coureur de la plus rigoureuse vérité?" (Vision du Passé, p. 229). Mais, chemin faisant cette "séduisante harmonie" devient bientôt pour Teilhard critère de vérité; il identifie: "l'ordre, l'homogénéité, c'est-à-dire la vérité" (Le Phénomène humain, p. 194).

Dans son "Esquisse d'un Univers personnel", Teilhard a mis en lumière, comment il conçoit cette cohérence comme sonsigne méthodologique, menant à la saisie de la vérité: "Il semble y avoir 'un vent d'esprit' enregistrable à travers le Monde. Comment établir définitivement ce fait, qui, bien prouvé, nous donnerait la preuve cherchée d'un mouvement défini de l'Univers?'

En l'acceptant, répondrai-je, et en cherchant si, poussé à ses dernières conséquences, il vérifie l'Univers autour de nous. La Physique ne connaît pas d'autre critère que cette réussite à la vérité de ses développements.... La vérité n'est pas autre chose que la cohérence totale de l'Univers par rapport à chaque point de lui-même.... 'Don't chat, but try? Laissons les discussions vaines et voyons en vrais positivistes si l'Univers se cohère..." (Energie humaine, p. 70–71).

On voit à quel point Teilhard est convaincu d'être dans la ligne de la science positive – même positiviste –, de la physique. Force nous est de constater que la physique, ou n'importe quelle science expérimentale, est autrement exigeante quant à la vérification de ses hypothèses que ne l'est Teilhard. Certes, ses hypothèses sont plus audacieuses, puisque beaucoup plus synthétiques. Sans doute, faut-il ici courir des risques qu'une science plus limitée peut s'épargner. Au moins faudrait-il le faire en connaissance de cause, et ne pas oublier toute la marge qu'il y a entre une théorie physique, vérifiable dans des faits bien concrets et mesurables, et les vues synthétiques proposées par Teilhard, invérifiables par principe dans les faits (p. ex. la dimension consciente de la Matière, le caractère psychique de toute énergie) (Le Phénomène humain, p. 62). Ici la seule vérification est dans la cohérence du système tel qu'il est conçu. Certes, ce n'est pas rien, et nous serions les derniers à dénier toute valeur à de tels essais. Mais il est évident que le seul critère de la cohérence ne saurait suffire, puisqu'il y a manifestement plusieurs façons de penser le monde de manière cohérente. Ceux qui ne suivent pas Teilhard, p. ex. dans l'attribution de la conscience aux réalités matérielles, ne renoncent pas pour autant à concevoir le monde comme un système cohérent. Il faut donc de toute nécessité un critère ultérieur pour départager les différentes conceptions. Teilhard a beau nous inviter à voir les choses comme il le fait, pour apprécier la valeur de sa synthèse. Il ne pourra nous empêcher de lui opposer par exemple que nous ne voyons nullement la dimension consciente de la matière. Comment fera-t-il pour nous obliger, contre l'évidence même de notre expérience, à admettre sa théorie pour la seule raison qu'elle est cohérente? Malgré toute la grandeur – et certainement la valeur – de la vision que nous propose Teilhard, nous lui reprochons de vouloir nous l'imposer à travers un cautionnement scientifique, auquel elle n'a aucun droit. Sans doute, l'intention de Teilhard ne saurait être refusée. Il faut de toute nécessité dépasser la compartimentement et l'émiettement du savoir scientifique. Déclarer d'emblée que tout problème de synthèse est par définition en dehors des limites de la science, serait une position désastreuse, et après tout, défaitiste. D'ailleurs précisément les faits d'évolution, comme Teilhard l'a bien montré, ne nous permettent pas d'éluder la question. Qu'il ait eu le courage de s'attaquer à ce problème, c'est sans aucun doute un mérite qu'on ne saurait lui dénier. Mais il est tout aussi évident qu'on ne peut se contenter de la façon, somme toute assez désinvolte, dont Teilhard s'y est pris.

Dire que tout ceci est œuvre de pure science est un leurre dangereux, qui risque de masquer le travail immense qu'il nous reste à faire pour explorer les possibilités vraiment scientifiques de synthèse.

Dans ce contexte, nous devons soulever une question ultérieure. Depuis toujours la philosophie s'est mise sur les rangs lorsqu'il s'agissait de poser le problème du réel total. Comment se fait-il que Teilhard, qui avait reçu une formation philosophique, n'ait pas pensé à chercher dans cette direction? Cette question se pose d'autant plus que Teilhard fait souvent intervenir la catégorie de "sens" dans l'élaboration de sa synthèse. Or, saisir, comprendre le sens d'une chose, d'un événement, n'est-ce pas déjà une intention qui dépasse la pure constatation expérimentale, pour s'attacher à la compréhension, à l'intelligence des choses, tâche qui a été de tout temps considérée comme plus spécialement philosophique?[6]

On pourrait certes répondre, qu'on ne peut exclure a priori un élargissement de la notion d'expérience, de façon à y inclure la *signification* du fait observé. Plus spécialement, la biologie semble bien devoir introduire de telles considérations au niveau même de sa recherche expérimentale. Il y a ici manifestement un problème sur lequel le dernier mot n'a pas été dit.[7] La prépondérance du paradigme de la physique dans l'élaboration de la méthodologie scientifique a certainement entraîné un rétrécissement indû de la notion d'expérience scientifique. On ne saurait donc reprocher sans autre à Teilhard de faire intervenir des considérations de "sens" dans sa synthèse. Poser la question même de l'orthogénèse, par exemple, implique déjà la notion du "sens" de l'évolution, et on ne voit pas à quel titre on pourrait défendre a priori à l'homme de science de la poser. Encore faudrait-il alors le faire selon des critères bien définis. La "cérébralisation" croissante en est certainement un, qui, à une échelle limitée, peut donner des indications valables.[8] Teilhard n'est d'ailleurs pas seul à l'appliquer à une certaine hiérarchisation de monde animal. Mais, lorsqu'il en fait sa fameuse loi complexité-conscience (cf. Le Phénomène humain, p. 58), il procède à une extrapolation géante, dans laquelle la notion de "sens" perd toute signification contrôlable, pour devenir une hypothèse non seulement

[6] Voir aussi sur ce problème, C. Soucy, Teilhard de Chardin est-il un philosophe?, in Recherches et Débats, n. 40, pp. 24–44. Egalement P. Grenet, *Teilhard de Chardin, le philosophe malgré lui.*

[7] J'ai consacré quelques réflexions à ce problème dans les contributions suivantes: Psychologie und Philosophie, in Studia Philosophica, Jahrbuch der Schweizerischen Philosophischen Gesellschaft **16** (1956) pp. 20-37 et Das Verhältnis zwischen Wissenschaftskritik und Naturphilosophie, in Tragweite und Grenzen der wissenschaftlichen Methoden (Naturwissenschaft und Theologie, Heft 5), pp. 150–175.

[8] Cf. Le Phénomène humain, ch. Le fil d'Ariane.

gratuite, mais même contraire aux faits les mieux établis. Peut-on imposer un "sens" aux phénomènes qui soit en contradiction avec leurs caractères observables les mieux établis?

Mais revenons à notre question essentielle: pourquoi Teilhard ne met-il pas en jeu ici des catégories d'ordre philosophique, qui sembleraient plus adéquates au problème posé? Surtout que sa passion d'absolu semblerait le prédisposer davantage à la philosophie qu'à la recherche scientifique.

Il y a là d'abord un problème assez complexe de psychologie personnelle du P. Teilhard, sur lequel nous ne pouvons pas insister ici. Remarquons seulement en passant que sa passion d'absolu a dès le début le besoin de se concrétiser: dans le fer, dans la pierre. Teilhard est un intuitif, qui a besoin de voir, de sentir. Il parle de son intuition fondamentale "la conscience d'une dérive profonde, totale, ontologique de l'Univers", comme d'un "sentiment" (cf. Le cœur de la Matière, p. 2 cité par Grenet, o.c. p. 73). Annonçant son projet encore vague d'un livre à écrire (on y devine déjà l'idée fondamentale du *Phénomène humain*), il spécifie: "Dans ces pages, où je ne chercherais à m'accorder avec aucun des courants d'idées reçues, mais seulement à traduire ce que je sens..." (Cuénot, o.c., p. 104.)

Et encore, avec plus d'insistance, vers la fin de sa vie, dans l'Introduction à son Essai sur "L'Etoffe de l'Univers"; "Ainsi, une fois de plus, je veux essayer d'atteindre et d'exprimer un peu plus outre, le fond, toujours fuyant, de ce que je sens, de ce que je vois, de ce que je vis". (L'Activation de l'Energie, p. 397).

L'ascèse d'une pensée abstraite n'est pas dans sa ligne. Il pense tout en images; lorsqu'il a trouvé l'image, il croit avoir compris et expliqué. Qu'on pense, par exemple, à son "explication" de la conscience par l'image d'une énergie radiale (Le Phénomène humain, p. 62 sv.). Mieux encore, que l'on relise son "explication" de l'avènement de la Réflexion: "à l'Anthropoïde, porté 'mentalement' à 100 degrés, quelques calories encore ont donc été ajoutés. Chez l'Anthropoïde, presque parvenu au sommet du cône, un dernier effort s'est exercé suivant l'axe. Et il n'en a pas fallu davantage pour que tout l'équilibre intérieur se trouvât renversé. Ce qui n'était encore que surface centrée est devenu centre. Par un accroissement 'tangentiel' infime, le 'radial' s'est retourné, et a, pour ainsi dire, sauté à l'infini en avant" (Le Phénomène humain, p. 185).

Une avalanche d'images, l'une plus suggestive que l'autre, mais auxquelles il est difficile de faire correspondre un contenu intellectuel bien défini. On comprend que la philosophie, surtout dans sa forme austère scolastique, ne devait avoir rien de très attirant pour un tel "imaginatif".

Un autre facteur personnel qui a décidé de l'orientation franchement "scientifique" de son œuvre, est aussi paradoxal que cela puisse paraître à première vue, son sens apostolique. Teilhard souffre de constater le divorce entre l'idéal religieux, incarné dans l'Eglise, et un idéal humain s'appuyant sur des valeurs terrestres, dont la science est l'aile marchante. Il est profondément convaincu que "au nom de notre foi, nous avons le droit et le devoir de nous passionner pour les choses de la Terre" (Le Milieu divin, p. 61). La science est une valeur humaine suprême que le croyant ne peut pas laisser en marge de sa vie: ce n'est qu'à travers la science que l'homme trouvera son plein épanouissement en Dieu. Pour Teilhard, son effort scientifique doit déboucher tout droit dans une apologétique et même une mystique (cf. Texte cité chez Grenet, o.c., p. 192). Le prestige de la Science, qui a trop longtemps écarté les hommes de Dieu, doit être mis au service d'une divinisation de l'Humanité. (cf. Le Milieu divin, p. 59 sv.).

Tout ceci nous fait comprendre, sans doute, l'attrait de Teilhard pour la science. Mais, puisqu'il veut faire la synthèse entre Science et Religion, pourquoi ne tâche-t-il pas de même à faire le pont entre Science et Philosophie? Or, on ne peut pas s'y méprendre, Teilhard a gardé pendant toute sa vie une certaine méfiance pour ne pas dire une hostilité à l'égard de la philosophie. Il est vrai que parfois il consent à laisser, à côté de la synthèse qu'il présente, une place à la philosophie qu'il qualifie même une fois de "essentielle et béante" (Le Phénomène humain, p. 21). Mais comme cette expression se trouve dans l'"Avertissement" écrit après coup et probablement à l'intention de ses censeurs, il convient de ne pas lui accorder trop d'importance. Plus souvent, Teilhard prend soin de se distancer de la philosophie, en soulignant que, malgré certaines apparences, il ne veut, à aucun prix, faire de la philosophie. Tout en admettant que l'ampleur de sa synthèse "risque de donner aux vues que je suggère *l'apparence* d'une philosophie", il nous conjure aussitôt: "regardez-y seulement de plus près; et vous verrez que cette Hyper-physique n'est pas encore une Métaphysique" (Le Phénomène humain, p. 22). Et il nous invite avec insistance: "Qu'on ne cherche donc pas ici une explication dernière des choses, une métaphysique" (o.c., p. 29). Ou encore: "Malgré certaines apparences, ou confluences, tenant à l'ampleur de l'objet étudié (au voisinage du Tout, Physique, Métaphysique et Religion convergent étrangement ...), je tiens et je maintiens, qu'au cours des pages qui suivent, je ne quitte à aucun moment le terrain de l'observation scientifique. Non pas une spéculation philosophique, mais une extension de nos perspectives biologiques, – rien de plus, mais rien de moins: voilà ce que cet essai prétend apporter". (La Planétisation humaine. Avenir de l'Homme, p. 161).

Pourquoi cette allergie envers la philosophie? Nous croyons en découvrir la raison profonde dans certains passages de ses lettres, où il semble livrer sans ambages, le fond de sa pensée.

En 1934, il écrit: "Il me semble présentement qu'il y a deux espèces de 'savoir': un savoir abstrait, géometrique, extra-durée, pseudo-absolu (= le monde des idées et des principes) auquel je ne me fie pas, instinctivement, et un savoir réel, qui consiste en l'actuation consciente (c'est-à-dire la création prolongée) de l'Univers autour de nous. Le premier de ces deux savoirs ne change ni le monde ni le sujet connaissant. Le deuxième coïncide avec le perfectionnement du monde et une croissance ontologique du sujet ... Je me méfie de la métaphysique (au sens habituel du mot) parce que j'y flaire une géométrie. Mais je suis prêt à reconnaître une autre espèce de métaphysique qui serait réellement une Hyper-physique, ou une Hyper-biologie" (Lettre du 29 avril 1934. Cuénot, o.c., p. 264). Vers la fin de sa vie une autre lettre dit: "Je crois que l'ancienne métaphysique (per descensum) a fait son temps et est en train de faire place au développement d'une Hyperphysique (aboutissant à l'établissement d'une Weltanschauung 'per ascensum') par co-réflexion générale de l'Humanité (co-réfléchissant sur le Phénomène" (Lettre du 3 décembre 1954, Cuénot, o.c. p. 428).

Notons qu'ici l'attitude de Teilhard envers la philosophie ou métaphysique est nettement négative. Non seulement il éprouve envers elle une aversion "instinctive", mais il croit qu'elle "a fait son temps" et qu'elle sera avantageusement remplacée par son Hyperphysique. Il espère "substituer aux métaphysiques dont nous mourons, une Ultra-Physique" (lettre du 11 octobre 1936, Cuénot, o.c., p. 264). Le jugement n'est pas flatteur pour la philosophie. Mais il ne l'est peut-être pas davantage pour Teilhard. Car dans tout le contexte il est clair qu'il conçoit la philosophie comme un savoir a priori purement déductif (per descensum) à partir d'un "monde des idées et des principes". On comprend qu'il n'attendait rien d'un tel savoir, qui à son avis, se construisait complètement en dehors de tout contact avec le réel. Quoiqu'ayant été enthousiasmé par Bergson et Blondel, et malgré aussi ses contacts personnels avec Le Roy, Teilhard semble avoir gardé pendant toute sa vie une image plutôt négative des philosophes et de la philosophie. Qu'on lise les extraits de lettres de la fin de sa vie, où il se déclare "préoccupé de l'urgence qu'il y aurait à opérer une réforme de l'Anthropologie en séparant enfin celle-ci des 'Lettres' ou de la 'Philosophie'" (Lettre du 21 septembre 1952, Cuénot, o.c., p. 428; voir aussi lettre du 11 janvier 1952, Cuénot, o.c., p. 425 et lettre du 18 avril 1953, Cuénot, o.c., p. 426). Or, si son préjugé d'une philosophie "a priori" pouvait se comprendre face à une scolastique rationalisante, on voit moins bien comment Teilhard peut mettre les philosophes "phénoménologistes" ou "existentialistes" dans le même sac (Lettre du 18 avril 1953, Cuénot, o.c., p. 426). Une certaine incompréhension fondamentale semble avoir commandé chez lui une attitude affective de refus. C'est d'autant plus dommage que, surtout dans certains courants phénoménologiques, pour ne pas parler de l'école néo-thomiste, la réflexion philosophique mettait au premier plan le souci de son enracinement dans le réel. Son refus, plutôt passionné, de la philosophie, a empêché Teilhard d'opérer des confrontations qui auraient pu féconder et rectifier son effort de synthèse. Mais, peut-être, la confrontation entre la pensée de Teilhard et la philosophie aurait-elle abouti à un dialogue de sourds. Certaines indications, plus spécialement la comparaison qu'il fait lui-même entre sa "phénoménologie" et celle de Husserl et de Merleau-Ponty (cf. lettre du 11 avril 1953, Cuénot, o.c.,

p. 311), vont dans ce sens.[9] Quoiqu'il en soit, nous devons constater que Teilhard aime marquer la différence entre ce qu'il fait, lui, et ce que font les philosophes. Mais, comme on peut souvent le voir, on ne chasse la philosophie que par la philosophie. Les problèmes philosophiques se posent avec une trop grande insistance à la réflexion humaine, pour que celle-ci puisse les éluder par de pures déclarations verbales. Il ne suffit pas de déclarer bien haut qu'on ne veut pas faire de la philosophie, pour ne pas en faire de fait. Malgré ses déclarations reitérées, Teilhard, par la logique immanente des choses, semble être amené à faire de la philosophie, puisque les problèmes qu'il aborde, impliquent tout naturellement des dimensions philosophiques.

Il semble être difficile de décrire la naissance de l'Esprit à partir de la Matière, par exemple, sans toucher d'aucune façon au problème philosophique de la nature propre de ces deux formes de réalité. Nombreux sont les points dans la synthèse teilhardienne, où des problèmes philosophiques se posent avec plus ou moins d'insistance. Or rien n'est plus dangereux que de remuer des problèmes philosophiques, sans s'en douter ou en voulant l'ignorer. C'est la meilleure manière de ne les poser ni de les résoudre de façon adéquate. C'est là un reproche grave qu'on ne peut épargner à la méthode de Teilhard. On l'a nommé, non sans raison, le philosophe malgré lui (P. Grenet a écrit un livre portant ce titre). D'ailleurs Teilhard lui-même parle parfois de "sa philosophie" (cf. par exemple, lettre du 16 juin 1935, Cuénot, o.c., p. 180; P. Grenet, P. Teilhard de Chardin, p. 192), tellement il est évident que la synthèse qu'il présente ne peut s'affirmer qu'en prenant au moins l'allure d'une philosophie.

Que conclure de tout ce que nous a livré ce rapide examen de la méthode chez Teilhard? A première vue, le bilan semble être bien négatif. Tant au niveau des exigences méthodologiques de la science, qu'à celui de la philosophie, des reproches graves ne peuvent être épargnés à Teilhard. Allons-nous en conclure que notre auteur n'a rien à nous apprendre, et que nous pouvons écarter ses idées comme dépourvues de consistance et de valeur? Je crois que ce serait une

[9] L'article cité plus haut de Malevez (cf. note 1) étudie plus spécialement ce point.

grave erreur. Nous avons peut-être une tendance à surestimer les questions de méthode parce que nous pensons trop dans le cadre de sciences toutes faites. Si nous regardons l'histoire des sciences, nous constatons que bon nombre d'idées fondamentales étaient acquises, avant que les canons de la méthode n'aient été formulés. [10]

La science qui se fait suit souvent davantage les chemins de l'intuition que ceux d'une lente élaboration méthodique. A travers des procédés méthodologiques parfois exécrables, Teilhard pourrait bien nous fournir des indications du plus haut intérêt pour l'avenir de la science, ne fût-ce qu'en posant, de façon brutale, le problème d'une synthèse de notre savoir. Certes, il peut nous exaspérer pas ses procédés parfois hautement discutables, pour ne pas dire nettement anti-scientifiques. Et néanmoins, gardons-nous de refuser à cause de cela, en bloc, tout son apport. On pourrait bien appliquer à toute l'œuvre de Teilhard, ce qu'il écrivait en 1917 à propos d'un de ses essais: "la partie philosophique est évidemment très approximative et même d'apparence manichéenne. Je l'ai laissée telle quelle, faute de pouvoir m'exprimer mieux et parce qu'il m'apparaît que sous des termes un peu faux ou contradictoires, il se cache une "dérive de vérité" qu'appauvrirait un langage plus correct dans sa logique et son orthodoxie de surface" (Lettre du 24 mars 1917).

Tâchons de lui faire la justice de découvrir, sous des procédés méthodologiques défectueux, une pensée qui ne manque pas de grandeur, et constitue un appel à une réflexion ultérieure.

Albertinum, Fribourg, Suisse

[10] Teilhard lui-même attache d'ailleurs une grande importance à cet aspect de la science en train de se faire et par là à une certaine "efficacité" comme critère de la valeur d'une méthode scientifique. Le reproche qu'il fait à la philosophie de ne changer ni le philosophe lui-même, ni le monde (cf. lettre du 29 avril 1934, Cuénot, o.c. p. 264) a comme pendant l'éloge de sa science à lui qui "coïncide avec un perfectionnement du Monde et une croissance ontologique du sujet" (*ibid*). On ne saurait y voir un pur "pragmatisme". L'homme étant pour Teilhard activement engagé dans le processus de l'Evolution, la science doit être un des facteurs de cette promotion évolutive. Il y aurait ici un point sur lequel il faudrait insister davantage. Nous ne pouvons le faire dans le cadre de cet article et nous proposons d'y revenir dans une prochaine publication.

CHAPTER 17

N. A. VASIL'EV AND THE DEVELOPMENT OF MANY-VALUED LOGICS

GEORGE L. KLINE

1. Introduction

Credit for the development of three-valued, and more generally, n-valued, logics, is customarily given to the Polish logician Jan Łukasiewicz [1], the American mathematician E. L. Post [3], and Łukasiewicz and the Polish-American logician Alfred Tarski [2]. Post's paper was written independently of Łukasiewicz's; it is a much fuller and more systematic discussion of topics which Łukasiewicz adumbrates with extreme brevity and conciseness.

The purpose of the following brief paper is to rewrite a chapter, or section of a chapter, in the history of formal logic, in order to give to the Russian N. A. Vasil'ev (1880–1940), a professor of philosophy at Kazan University, his due as originator of the general idea of a three-valued logic, an idea set forth in four papers published between 1910 and 1913, the principal statement dating from 1912. Vasil'ev's work anticipated that of Łukasiewicz and Post by a full decade. That his work is not better known [1] appears

[1] Vasil'ev's work is not mentioned in most histories of logic. Even the Soviet logician A. A. ZINOV'EV, in his interesting monograph, *Philosophical Problems of Many-Valued Logic* (trans. from the Russian by David D. Comey and Guido Küng; Dordrecht: Reidel, 1963) does not refer to Vasil'ev. Neither do J. BARKLEY ROSSER and ATWELL R. TURQUETTE in their *Many-Valued Logics* (Amsterdam, 1952). The only study of Vasil'ev so far published makes no reference to his anticipation of the idea of a three-valued logic. (See V. A. SMIRNOV, "Logičeskie vzgljady N. A. Vasil'eva" [N. A. Vasil'ev's Logical Views] in *Očerki po istorii logiki v Rossii* [Essays on the History of Logic in Russia], Moscow, Izdatel'stvo MGU, 1962, pp. 242–257.)

Vasil'ev's three major papers (Nos. [4], [6] and [7] in the Bibliography at the end of this article) are listed by ALONZO CHURCH as Nos. 321*1*, 321*0,1*, and to be due chiefly to the fact that his papers have been available only in Russian [2] and that files of the journals in which they were published are to be found only in a handful of major research libraries.

2. Non-Euclidean geometry and "non-Aristotelian" logic

It can hardly be accidental that one of the first formulations,[3] if not the first, of a three-valued, "non-Aristotelian" [4] logic, should

321o.2 in "A Bibliography of Symbolic Logic" (Journal of Symbolic Logic **1** (1936) 121–216; **3** (1938) 178–192.) Vasil'ev's work is referred to briefly in a paper by the Polish historian of logic ANTONI KORCIK ("Przyczynek do historii klasycznei teorii opozycji zdan asertorycznych" [A Contribution to the History of the Classical Theory of the Opposition of Assertoric Propositions], Roczniki filozoficzne **4** (1955) 33–49). Otherwise Vasil'ev's work has been neglected on all hands.

[2] An abstract of Vasil'ev's 1912 paper appeared in 1924: "Imaginary (non-Aristotelian) Logic," *Atti del V Congresso internazionale di filosofia*, Napoli, 5–9 maggio 1924. Naples, 1925, pp. 107–109. However, this version is worse than useless: it is written in awkward, ungrammatical, and unidiomatic English, which distorts Vasil'ev's meaning at crucial points. That Western logicians should have paid no serious attention to this garbled presentation of Vasil'ev's ideas is quite understandable.

[3] It has been claimed that such medieval thinkers as Duns Scotus, and especially William of Ockham, provided the basic conception of a three-valued logic in their discussion of the "propositio neutra" as something distinct from the "propositio vera" and "propositio falsa". See, for example, KONSTANTY MICHALSKI, "Le problème de la volonté à Oxford et à Paris au XIVe siècle," Studia philosophica (Lemberg [Lwow] **2** (1937); and the critical remarks of PHILOTHEUS BOEHNER, O.F.M., in his edition of *The* Tractatus de praedestinatione et de praescientia Dei et de futuris contingentibus *of William of Ockham, with a Study on the Medieval Problem of a Three-Valued Logic* (Franciscan Institute Publications, No. 2; St. Bonaventure, N.Y., 1945).

[4] Łukasiewicz has pointed out that *n*-valued logics (where $n > 2$) should not, strictly speaking, be called "non-Aristotelian," since Aristotle himself admitted a class of propositions ("future contingents") which are neither true nor false. Łukasiewicz suggests that such logics might better be called "non-Chrysippean," since it was Chrysippus who first asserted categorically that all propositions are either true of ralse. (Cf. JAN ŁUKASIEWICZ, "Philosophische Bemerkungen zu mehrwertigen Systemen des Aussagenkalküls,"

have come from Kazan University. It was there, in the mid-nineteenth century, that N. I. Lobachevsky had formulated his non-Euclidean geometry.

Lobachevsky himself had called his geometry "imaginary" (*voobražaemaja geometrija*); the term 'non-Euclidean' became current somewhat later. Vasil'ev calls his logic "imaginary" (*voobražaemaja logika*),[5] and by analogy with non-Euclidean geometry, "non-Aristotelian." A given logic, Vasil'ev points out, rests on several independent axioms, not just one,[6] so that, by omitting different axioms, we get different logics ([6], p. 211). In non-Euclidean geometry – whether Lobachevskian or Riemannian – Euclid's fifth ("parallel") postulate is omitted; in non-Aristotelian logic Aristotle's Law of Noncontradiction is omitted, or at least the scope of its application is drastically limited ([6], p. 208). (Vasil'ev's claim presupposes that the Law of Noncontradiction is explicitly included among the axioms of a logical system, which is seldom the case.)

According to Vasil'ev, Euclidean and non-Euclidean geometries have in common those Euclidean theorems which do not depend on the parallel postulate; similarly, Aristotelian and non-Aristotelian logics have in common those propositions which do not depend on the Law of Noncontradiction. Just as in Lobachevskian geometry straight lines may be either:

(1) intersecting (*svodnye*),
(2) non-intersecting (*razvodnye*), or
(3) parallel,

Comptes rendus des séances de la Société des Sciences et des Lettres de Varsovie, classe III **23** (1930) 51–77. This remark is cited, with apparent approval, by ZINOV'EV, *op. cit.*, p. 16 [fn. 1 above].) Vasil'ev, after referring to DeMorgan, Peirce, Peano, Russell, et al., asserts that "the whole contemporary movement in logic is a revolt against Aristotle" ([7], p. 81).

5 In one place he suggests – somewhat playfully, I think – a number of other "imaginary" disciplines: "imaginary zoology" – which deals with centaurs, etc.; "imaginary sociology" – which deals with utopias; "imaginary history" – which deals with uchronias. In this connection, Vasil'ev refers the reader to RENOUVIER ([6], p. 222).

6 "Mathematical logic is an elegant demonstration of this; it is based upon several axioms and postulates" ([6], p. 211*n*1).

so, in non-Aristotelian logic propositions (*suždenija*) may be either:

(1) affirmative,
(2) negative, or
(3) "indifferent."

We shall return presently to Vasil'ev's notion of an "indifferent" proposition. For the moment, we need only note that for him propositions of type (3) as well as (2) are "negative" in the (limited) sense of asserting the corresponding affirmative proposition to be false. (Perhaps Vasil'ev's discussion at this point would have been clarified by a distinction between object-language and meta-language; he does not make such a distinction, but then, neither did his more celebrated contemporaries, ca. 1912).

Vasil'ev concludes that the "true-false" dichotomy of standard logic, like the "intersecting-parallel" dichotomy of standard geometry, gives way to a *trichotomy* in the "imaginary" disciplines. Vasil'ev admits that Aristotelian logic and Euclidean geometry are the "simplest" systems; but he insists that non-Aristotelian logic has just as much right to be called logic as non-Euclidean geometry has to be called geometry ([6], p. 233).

3. Incompatibility, negation, and the Law of Noncontradiction

According to Vasil'ev, the Law of Noncontradiction (*zakon protivorečija*) is a special case and consequence of the Law of Excluded Middle, rather than vice versa. It expresses the "incompatibility (*nesovmestimost'*) of affirmation and negation" [6], p. 212).[7] Vasil'ev adds that "incompatibility is the only logical ground for negation" ([6], p. 214), so that the Law of Noncontradiction follows directly from the definition of negation.

In Aristotelian logic the Law of Noncontradiction is never violat-

[7] Cf. the Peirce-Sheffer stroke function '|' ("alternative denial"), an operator which may be interpreted as meaning "not both". In *Principia Mathematica* notation:

$$A \mid B \;.{=}.\; \sim A \vee \sim B \;.{=}.\; \sim(A \,.\, B)$$

(See WHITEHEAD and RUSSELL, *Principia Mathematica*, second edition, p. xvi, for a statement of the fundamental importance of this operator).

ed; any alleged violation is regarded as not being a genuine case of negation. "The Law of Noncontradiction is just as unshakeable as the truth that the earth rotates on its axis in twenty-four hours (*v tečenie sutok*). Whether the rotation is faster or slower, one complete rotation will take exactly twenty-four hours, since 'twenty-four hours' is defined as the period of a complete rotation of the earth on its axis" ([6], p. 215). Vasil'ev denies that either case is merely tautologous: the proposition that the earth rotates on its axis in twenty-four hours presupposes the fact that the earth does rotate; the Law of Noncontradiction presupposes the "fact" that there are incompatible predicates. (Vasil'ev appears to assume that these "facts" are on the same logical level, a controversial assumption at best. The general question of the relation of logic to the empirical world will be treated below).

Vasil'ev insists that negative predicates are not primitive but are arrived at inferentially from positive predicates. Similarly, negative propositions are inferentially derived from affirmative propositions. According to Vasil'ev, we do not have negative sense-perceptions – e.g., of non-white; we cannot directly confirm the absence of a property. All sense-perceptions are positive; negativity is introduced through comparison with other perceptions. For example, there is nothing negative in the sense-impression "red"; but we compare it to (sensed) white and label it "non-white." "Not-to-see x" is, in fact, "to-see y"; and, if y is incompatible with x, we can infer non-x ([6], pp. 213f). In general, negative propositions about perceived objects and properties are inferences from propositions about incompatible properties. "I saw [*videl*] that the object was red; I inferred [*vyvel*] that it was not white, knowing that red cannot be white." According to Vasil'ev, we do not think of this process as an inference because we ordinarily carry it out rapidly and automatically. But in fact we can deny that an object has property P only by asserting that it has property N, which excludes P. This inference can be expressed as a syllogism:

$$\begin{array}{l} N \text{ is incompatible with } P \\ S \text{ is } N \\ \hline S \text{ is not-}P \end{array}$$

There are thus two kinds of negative propositions: "N excludes (is incompatible with) P" – a premise; and "S is not-P" – a conclusion.

In Aristotelian logic, which applies to the "ordinary" world, a given object cannot provide the ground for both affirmative and negative propositions, since – as we have seen – "ordinary" Aristotelian negation is based on incompatibility. But a non-Aristotelian logic, without the Law of Noncontradiction, would not utilize such "ordinary" negation. According to Vasil'ev, we can imagine a world in which "experience itself, without any inference, would convince us that S is not P" ([6], p. 216), because we would be able to sense negative properties directly. For example, empirical fact a would make the proposition "S is A" true; fact b would make the proposition "S is not A" true. But facts a and b would not be incompatible, as such facts are in the world we know. In the imagined world to which "imaginary logic" applies, a given sense-object might thus simultaneously ground both affirmative and negative propositions about the sensed properties of the object. In such an "imaginary logic" we could assert the contradictory or "indifferent" (i.e., neither affirmative nor negative) proposition: "S is and is not A." Such a proposition asserts that an object or event both has and does not have a given property, i.e., is both red and not-red, or both discrete and non-discrete.

Such considerations lead us directly to the question of three-valued logics and the "Law of Excluded Fourth."

4. Three-valued logics and the "Law of Excluded Fourth"

In a three-valued logic negative propositions, while asserting the falseness of the corresponding affirmative propositions, are not based on incompatibility ([6], p. 217). The Aristotelian Law of Noncontradiction is replaced by a "Law of Absolute Difference of Truth and Falsity," which asserts that no proposition can be both true and false at the same time. This may also be called a "Law of Nonselfcontradiction" (*zakon nesamoprotivorečija*) ([6], pp. 218, 221). Vasil'ev goes on to say that the Aristotelian Law of Noncontradiction has "objective" reference, while the non-Aristotelian Law of Nonselfcontradiction has only "subjective" reference. Thus, in an imagined world which is objectively different from our world, the "objective" law could be rejected, but – presumably because our conceptual apparatus would remain the same – the "subjective" law would retain its force.

Although Vasil'ev does not try to develop a logical calculus using "indifferent" propositions (and in general, seems to have no clear idea of what is involved in such a calculus), he does assert that his "imaginary logic" is a consistent system, and he is at pains to specify the rules which govern the truth-values of the various kinds of propositions. Thus, an affirmative proposition is false when either the corresponding "indifferent" proposition or the corresponding negative proposition is true. More generally, each of the three kinds of proposition is false when one, but not both, of the other two kinds is true. Vasil'ev offers three rules, which we may reformulate as follows:

(1) If an affirmative proposition is false, then either the corresponding negative proposition, or the corresponding "indifferent" proposition, is true, but not both.

(2) If a negative proposition is false, then either the corresponding affirmative proposition, or the "indifferent" proposition, is true, but not both.

(3) If an "indifferent" proposition is false, then either the corresponding affirmative proposition, or the corresponding negative proposition, is true, but not both.

These rules may be generalized in this form: Where the affirmative and negative propositions have the *same* truth-value, the corresponding "indifferent" proposition is *true*; where they have *different* truth-values, the corresponding "indifferent" proposition is *false*.

Vasil'ev insists that ordinary logical rules, including the rules of inference (and, in particular, syllogistic inference) apply to the "indifferent" proposition. In the first figure of the syllogism (which, he claims, does not depend upon the Law of Noncontradiction), two valid modes are added to the traditional four: *Mindalin* – universal "indifferent", and *Kindirinp* – particular "indifferent." Thus:

Mindalin	*Kindirinp*
All M are and are not P	All M are and are not P
All S are M	Some S are M
All S are and are not P	Some S are and are not P

Vasil'ev offers two further interpretations of three-valued logic; the first is based on a distinction between "absolute" and "simple" or "relative" negation.[8] The distinction is as follows (I have slightly formalized Vasil'ev's argument, introducing an arbitrary notation for absolute [$\bar{A}$] and relative [$\sim A$] negation, but have added nothing essential).

Assume that concept A includes properties $p_1, p_2, \ldots, p_n$. Then

(a) The absolute negation of A is formed by the *conjunction* of the negations of all of these properties:

$$\bar{A} = \sim p_1 \cdot \sim p_2 \cdot \ \ldots \ \cdot \sim p_n$$

(b) The relative negation of A is formed by the *alternation* of the negations of all of these properties:

$$\sim A = \sim p_1 \vee \sim p_2 \vee \ldots \vee \sim p_n.$$

Vasil'ev claims that (b) reproduces the "ordinary" sense of negation.[9] However, he does not consider the possibility that, under non-exclusive disjunction (v), absolute and relative negation might coincide, i.e., that *every* negation in alternation (b) might hold. This difficulty could be avoided by using exclusive disjunction ($\not\equiv$), but this would greatly weaken the concept of relative negation. Post's distinction between "complete" and "incomplete" falsity is much more elegant. Post generalizes the notion of degrees of

[8] He notes that Bolzano had distinguished between absolute and relative negation (cf. *Wissenschaftslehre*, I, § 89). In fact, Bolzano speaks of a *rein oder durchaus verneinende Vorstellung*, as opposed to a *teilweise verneinende Vorstellung*. In other words, he distinguishes – as Post did later – between "complete" and "incomplete" negation. However, he also speaks of *das absolute Nichts*. (BERNARD BOLZANO, *Wissenschaftslehre*, Salzbach, 1837 [reprinted Leipzig, 1914], Vol. I, pp. 417, 423).

[9] Vasil'ev does not distinguish between negation of a property and negation of a concept. Presumably there would be only one kind of negation ("absolute"?) of properties, unless properties in turn were analyzable into component sub-properties – as concepts are analyzable into component properties.

negation (or falsity) in terms of a truth-function P, composed of (subordinate) truth-functions $p_1, p_2, \ldots, p_{m-1}$, and proposes that P be said to have the truth-function t_1 "if all the p's are true, t_2 if all but one are true, etc." ([3], p. 184).

On Vasil'ev's view, the proposition "S is M" is *true* if S has all of the properties in question, i.e., is characterized by the conjunction $p_1 \cdot p_2 \cdot \ldots \cdot p_n$. The proposition is *absolutely* false if S has none of these properties, i.e., is characterized by the conjunction $\sim p_1 \cdot \sim p_2 \cdot \ldots \cdot \sim p_n$. The proposition is *relatively* false if S lacks at least one of these properties, i.e., is characterized by the alternation $\sim p_1 \vee \sim p_2 \vee \ldots \vee \sim p_n$.

The difference between Vasil'ev's "properties" and Post's "truth-functions" seems to me significant, but not decisive, in this context.

Vasil'ev notes that a three-valued logic based upon the truth-values "true," "absolutely false", and "relatively false" would differ slightly from one based upon affirmative, negative, and "indifferent" propositions. Unfortunately, he did not work out the differences in detail. He did remark that the first figure of the syllogism could be interpreted in terms of absolute negation in the minor premise:

$$\begin{array}{l} A \text{ is } P \\ S \text{ is non-}A \\ \hline S \text{ is non-P} \end{array}$$

– where the minor premise, and presumably the conclusion, are to be read "A [and, in the conclusion P] is absolutely negated with respect to S" ([6], p. 241).

The distinction between absolute and relative negation (and falsity) would appear to be better adapted to the construction of three-valued and many-valued logical calculi than that of affirmative, negative, and "indifferent" propositions.

Vasil'ev's final interpretation of three-valued logics centers on the Law of Excluded Fourth, as applicable not to objects or events, but to concepts (*ponjatija*). The Law of Excluded Middle applies to objects and events – e.g., the lamp either burns or does not burn, *tertium non datur* – but a given predicate may be (1) required by, (2) incompatible with, or (3) compatible with but not required by, a given concept ([4], pp. 42, 44). E.g., for the concept "triangle" the predicate 'closed' is required (or "necessary"); the predicate 'round' (Vasil'ev's own, rather misleading, example is "virtuous") is excluded; the predicate 'isosceles' is neither required nor excluded ([6], p. 234). Required predicates appear in traditional "universal affirmative" propositions; excluded predicates in "universal negative" propositions; compatible predicates in what Vasil'ev calls "accidental" propositions – what are now usually called sentences in modal logic. Vasil'ev characterizes "accidental" (modal) propositions as a special synthesis of affirmative and negative propositions. Thus

$$S \text{ may be } P = \text{Some } S \text{ are } P \text{ and some } S \text{ are not } P.$$

Vasil'ev suggests in this context that modal logic applies only to concepts, but he does not develop the suggestion. In 1910 he had distinguished between conceptual propositions (which state rules or laws, "*was gilt*") and factual propositions (which state "*was ist*"). The Law of Excluded Middle and the Square of Opposition apply to the latter, the Law of Excluded Fourth and the "Triangle of Opposition" to the former.

Vasil'ev also envisages the possibility of n-valued logics, where $n > 3$. Just as Spinoza conceived of Substance as having an infinite number of attributes of which we know only two (extension and thought), so – Vasil'ev writes – we may conceive logical systems (calculi?) with an indefinite number of kinds of propositions of which we know only two (affirmative and negative). "We can conceive a logical system with n qualitatively different kinds of propositions; we will call such a system a logical system of order n, or an n-dimensional system" ([6], p. 229). Such a system, Vasil'ev continues, is analogous to n-dimensional geometry.[10] It would have a special "law of excluded $n + 1$st" – e.g., in three-dimensional (i.e., three-valued) logic, a "Law of Excluded Fourth," asserting that there can be no fourth kind of proposition in addition to affirmative, negative and "indifferent."

[10] A similar comparison is made by Post (see [3], p. 164).

5. Conclusion

The principal difference between Vasil'ev's anticipation (1910–1913) and Łukasiewicz' and Post's formulation (1920–1921) of the idea of three-valued and many-valued logics is that the Russian did not, and the Pole and American did, work out propositional *calculi* based on three, and, more generally, n, truth-values. In this respect the work of Łukasiewicz and Post has a definite advantage over that of Vasil'ev. However, in other respects: in the general idea of a non-Aristotelian (or "non-Chrysippean") logic paralleling non-Euclidean geometry, in the replacement of the Laws of Noncontradiction and Excluded Middle,[11] in the distinction between absolute and relative negation and falsity (anticipating Post's distinction between "complete" and "incomplete" falsity) – Vasil'ev set forth the essential ideas of three-valued and many-valued logics. His contribution deserves to be better known.

Bryn Mawr College, Bryn Mawr, Penn., USA

[11] Of course, L. E. J. Brouwer had published strictures on the Law of Excluded Middle as early as 1908. However, I have found no evidence that Vasil'ev was familiar with Brouwer's work.

Vasil'ev's fundamental objections would appear to be directed against the Law of Excluded Middle rather than the Law of Noncontradiction. Thus his persistent attacks on the latter Law are somewhat misleading.

Bibliography

[1] ŁUKASIEWICZ, JAN, O logice trójwartościowej [On Three-Valued Logic], Ruch filozoficzny **5** (1920) 170–171.
[2] JAN ŁUKASIEWICZ and A. TARSKI, Untersuchungen über den Aussagenkalkül, Comptes rendus des séances de la Société des Sciences et des Lettres de Varsovie, classe III **23** (1930) 30–50.
[3] POST, E. L. Introduction to a General Theory of Elementary Propositions, American Journal of Mathematics **43** (1921) 163–185.
[4] VASIL'EV, N. A., O častnyx suždenijax, o treugol'nike protivopoložnostej, o zakone isključënnogo četvërtogo [On Particular Propositions, the Triangle of Opposition, and the Law of Excluded Fourth], Učënie zapiski Kazan'skogo Universiteta (Oct. 1910) 47 pp.
[5] VASIL'EV, N. A., Voobražaemaja logika. Konspekt lektsii [Imaginary Logic. Abstract of a Lecture], Kazan', [1911), 6 pp.
[6] VASIL'EV, N. A., Voobražaemaja (nearistoteleva) logika [Imaginary (Non-Aristotelian) Logic], Žurnal Ministerstva Narodnogo Prosveščenija **40** (Aug. 1912) 207–246.
[7] VASIL'EV, N. A., Logika i metalogika [Logic and Metalogic], Logos **3**, 2–3 (1912–1913) 53–81.