Sprachphilosophie
Philosophy of Language
La philosophie du langage

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75. One or many logics?
Arguments relevant to the philosophy of language

1. Introduction

The philosophy of language is centrally concerned with meaning, truth and reference. Truth arises from referential connections and the way things are; and meaning is intimately tied to conditions of truth and of reference. Logic is the study of how truth is preserved regardless of how changes in referential connections may be rung. A correct philosophy of language in general, or of any one (kind of) language in particular must, therefore, have something to say about ›the‹ logic (should there be one) of language in general, or of any one (kind of) language in particular. But is there just one correct logic?

Given the variety of logical systems developed in the twentieth century, or even in the last decade, a philosopher or logician trying to make a case for one logic over all others is in danger of looking like a knight searching for the Holy Grail. One or many logics? — of course there *are* many, and the descriptive task of surveying them is not trivial. But which among them is the right one? — here we encounter controversies right from the start, and the normative task of settling them is at best contentious, at worst impossible. In this paper I try first (2.) to give a map of the ways in which classical logic can be varied or expanded so as to get other systems, and then (3.) sketch the general issues surrounding one's choice of logic. Next (4.) I discuss, with the necessary background, some of the most important innovations that have occurred in logical research over the past decade or so. I say a good deal about the differences between classical and intuitionistic logic, and describe other departures from classical logic of a more radical kind, such as relevance logic. I also describe recent work on the modal logic of provability, as an example of increasingly focussed constraints on the interpretation of the modal operator. In 5. I mention briefly other important recent developments that lack of space prevents me from discussing. But by mentioning them, I believe, the present survey can aspire to a degree of qualitative completeness as far as developments in the last two decades or so are concerned. In 6. I discuss the most important criteria that I believe should be involved in the choice of a logic. I try to make the reader aware of the many other developments that I do not have space here to discuss. The point of a survey paper is also to suggest directions for further research. I accomplish this last task, if at all, only from a biased perspective (7.). Readers will of course make the corrections necessary from their own. My suggested directions are ones that have not yet developed much momentum in current work, but which I believe it would be fruitful and rewarding to explore.

2. The most notable supplementations of, and deviations from, classical logic

2.1. 'Logic' is a vague term of art. But logicians tend to associate with it a cluster concept that helps one to classify logics along different dimensions of variation. I shall first describe these dimensions of variation that go to make up the cluster. The best way to start is by describing the origin, so to speak — the system of classical first order logic. Logics tend to be mapped by the ways in which they differ, in various dimensions, from the classical case. The dimensions, their sub-dimensions, and so on, are listed below by catchwords and phrases. In italics are the options conventionally collected under the rubric 'classical first-order logic'. In distributing the emphases in italics I have assumed a countable language, and, for definiteness of illustrative purpose, have assumed also that the system is one of natural deduction, as done by me (Tennant 1978), for a universally free logic with the description operator primitive.

Language
   semantically open/semantically closed
   first order/higher order/type-theoretic/combinatory
   Logical vocabulary
      Connectives: *negation, disjunction, conjunction, implication*
      Quantifiers: *universal, existential*
      Combinators
      Variables
   Extra-logical vocabulary
      finite/*countable*/uncountable
      Names
      Predicates
      Functions
   Rules of sentence formation
      finitary/infinitary
      allowing variable binding to form terms? *Yes*/No
      linear/branching quantifier prefixes
Semantic interpretation
   Space of truth values:
      $\{t,f\}$; some other Boolean algebra; a finite matrix; an infinite matrix (e. g. a Lindenbaum algebra); the rational interval $[0,1]$; the real interval $[0,1]$; a topological space
   Extensional
      Domain D
         Unitary
         Admitting an inner/outer (non-existent; fictional) distinction
      Extensions for extra-logical primitives
         Denotations for names:
            everywhere defined/*not everywhere defined*
         For function signs:
            total functions/*partial functions*
         For predicates:
            partitions of D^n
            extensions and anti-extensions within D^n
      Method of evaluation of expressions:
         Truth tables for connectives
         Satisfaction method for quantifiers
         Supervaluation? Yes/*No*
   Intensional:
   Proof-based (constructions)
   Possible worlds semantics (frames)
      kind of accessibility relations?
      domain relations?
      kind of indices?
   Other: Game theoretic
      material game
      dialogical games
      Truth-value semantics
      Probabilistic semantics *(all possible if desired)*
      Transfinite iterations of stages of evaluation (especially for semantically closed languages)
Semantic notions:
satisfiability/finite satisfiability (finite models property)
logical consequence: *single conclusions* or multiple conclusions?

closure properties of the Cn relation (double turnstile):
   compactness
   dilution on left
   dilution on right
   reflexivity
   transitivity (cut)
   interpolation
Mathematical properties to be investigated:
algebras of propositions (equivalence classes of sentences)
models: elementary extensions; prime models; ultraproducts

Proof theoretic considerations
   What kind of proof-system?:
      Hilbert-style proofs
      Natural deductions
         normalization techniques
         extraction techniques
      Sequent proofs
         cut-elimination
         dilution-elimination
      Tableau (tree)/resolution method
   Axiomatizability of Cn or Completeness (strong vs weak) of the proof system with respect to the chosen semantics
Important meta-properties of the logic
   Logical consequence compact? *Yes*/No
   Theoremhood and deducibility from finite sets of premisses
   decidable? Yes/*No*
   Countable models theorem (Löwenheim-Skolem): *Yes*/No
   Joint consistency property (Robinson): *Yes*/No
   Definability property (Beth): *Yes*/No
(Important meta-properties of theories: completeness, decidability, categoricity, model completeness, stability, finite axiomatizability, mutual interpretability)

2.2. Dimensions of variation for the classification of logics I

Within the conceptual space spanned by these dimensions of variation, classical logic lies at the origin. Some of the variation envisaged in the list above may turn out not to be essential or intrinsic to the location of a logic in the space of possibilities. For example, a great many logics admit of proof-theoretic formulations of all the kinds listed as alternatives above. Indeed, as soon as one has identified in some way a new system of, say, modal logic, the search will be on to find as many different possible syntactic presentations of it as one and the same logic. Logics are most broadly individuated as their deducibility relations: a logic is a set of ordered pairs $\langle \Delta, \Gamma \rangle$ for which Δ provably implies Γ (or, equivalently, Γ is deducible from Δ). On the single-conclusion construal of a logic, Γ

is restricted to be a singleton. On a multiple-conclusion construal of a logic, Γ may in general have more than one member (see Smiley/Shoesmith 1978 for an investigation of multiple-conclusion counterparts of single-conclusion calculi; see also Boričić 1985 for interesting results about multiple-conclusion versions of systems of natural deduction). Provided only that the ›output‹ of a proof system is the right collection of ordered pairs $\langle \Delta, \Gamma \rangle$, that proof system has captured the logic concerned. Thus the choice of any particular style of proof could be regarded as inessential to the identity of the logic concerned, even though it may affect the ease with which we acquire insights into the system and prove results about it. Despite all this, however, it *could* turn out that a logic with a presentation as a Hilbert system, say, could not be provided with an exactly co-extensive Gentzen sequent formulation for which cut-elimination would be provable. An example of this may be Anderson and Belnap's system R of relevance logic, which was originally presented as a Hilbert system. R. K. Meyer (1966) was able to Gentzenise only a proper subsystem of R, now called 'LR'; LR needs the distributivity axiom added to it to yield R. — Similarly, the various kinds of semantic treatments that have been invented for different logics could turn out not to be essential or intrinsic to the location of a logic in the space of possibilities. A semantics has traditionally served two purposes. One is to encapsulate, in formal or algebraic form, a philosophical analysis of how language connects with the world, and how its sentences, under interpretation, make true or false claims about the world. Thus, in the classical or realist case we assign denotations to names and extensions to predicates within some fixed domain of discourse. This is the way we ›hook language onto‹ the world. Then, using the truth tables for the connectives, and the method of satisfaction by assignments of individuals to free variables of open sentences, we account for the truth and falsity of more complex sentences built up from that logical and extra-logical vocabulary. By appealing to the preservation of truth (so analysed) from premises to conclusion, one attains a notion of logical consequence, which it is the task of soundness and completeness theorems to match with the deducibility relation generated by a chosen system of proof. And here one encounters the second aim of semantics. This is to provide countermodels to any arguments

that are not provable within the logic. To this end it matters not if the formal semantics that delivers the models provides no good philosophical schematism for the connection between language and the world. All that matters is that should any argument 'Δ, therefore Γ' not be provable, there be a ›model‹ that ›verifies‹ Δ but does not ›verify‹ Γ. The model need not, in Susan Haack's words (Haack 1978, chap. 10), provide any "philosophical patter" on the language of the logic. Despite this more technically mundane aim of formal semantics, the fact remains that for almost all the semantics so far offered for classical and intuitionistic logics at least, some sort of philosophical insight or illumination can be claimed for the way in which sentences are rendered true or false (or ›valued‹ in some other way). For more on this point, see 3.; see also H. C. M. de Swart's interesting comparison of the main kinds of semantics for intuitionistic logic in his (1977). — The remarks just made about the variety of proof systems and semantic treatments make it appropriate — to continue the geometric metaphor — to regard these respects less like ›dimensions‹ in which variation of a logic can occur, and more like ›choices of coordinate system‹ against which the real variation can be measured.

2.3. Dimensions of variation for the classification of logics II

Now in Haack's familiar terminology, logics can display real variation from classical logic (that is, be *rivals* to classical logic) either by *supplementing* it, or by *deviating* from it. When devising a *supplementary* logic, one retains classical logic as a subsystem while expanding one's expressive resources. That is to say, the deducibility relation of the supplementary logic, when restricted to the language of classical logic, contains the deducibility relation of the latter. This can occur, for example, when we introduce quantifiers of higher order, or new cardinality quantifiers; when we introduce modal operators (necessity, possibility etc.); or when we introduce new connectives (such as the counterfactual conditional). When devising a *deviant* logic, however, one usually cuts back on the axioms and/or rules of inference of classical logic so as to obtain a proper subsystem of it. That is, every pair $\langle \Delta, \Gamma \rangle$ in the deducibility relation of the deviant logic is in that of classical logic, but not *vice versa*. Devising a deviant logic involves, first, identifying objectionable

theorems or inferences of classical logic that are thought not to be generally valid (either on one's preferred pre-theoretical understanding of the language, or because of wider scientific considerations — see the list below); then, secondly, framing a subsystem that excludes what is objectionable in the deducibility relation of classical logic but that retains what is not. The project is both philosophical and technical: philosophical in grounding one's objections to the classical features that are to be excluded, and in providing the rationale for the subsystem that is retained; and technical in showing precisely what the properties of the new subsystem are. — I said above that this is the *usual* way to obtain a deviant logic. But there is a further development made possible by cutting down to a subsystem of classical logic. This is to expand again, but with principles that are *rejected* in classical logic. Provided this expansion takes place on a small enough subsystem of classical logic, one does not necessarily collapse the resulting system into a Post-inconsistent one (that is, one in which every sentence is a theorem). An example of this technique is the expansion of a relevant subsystem of classical logic with Aristotle's thesis $\sim(A\rightarrow\sim A)$, to get the variety of logics known as connexive logics (see Routley 1978). Another is the expansion of the subsystem $R-W$ with the ›principle of relativity‹ $((A\rightarrow B)\rightarrow B)\rightarrow A$ (see Meyer/Slaney 1979), which generalises the double negation axiom $((A\rightarrow\bot)\rightarrow\bot)\rightarrow A$.

Once one has a well-established deviant logic as a subsystem of classical logic, questions immediately arise concerning systems intermediate between the two. Indeed, the term 'intermediate logic', used without qualification, has come to have the entrenched meaning of 'intermediate between intuitionistic and classical logic'. There are continuum-many of these systems, and a large literature has developed around them. One of the better-known of these systems is Michael Dummett's *LC*, obtained by adding to the usual axiomatic formulation of intuitionistic logic the axiom schema $(A\supset B)\vee(B\supset A)$. It has no finite characteristic matrix, but does have a denumerable one. — Below is a list of classical theorems and inferences that have caused controversy. I use the single arrow '→' instead of the material conditional '⊃' to bring out the point that the controversy has to do with whether '⊃' adequately represents our pre-theoretic notion of 'implication' or 'entail-

ment'. The double arrow '⇒' is the sign of deducibility.

Controversial for intuitionists:

$\sim\sim A \Rightarrow A$ law of double negation
$\Rightarrow A \vee \sim A$ law of excluded middle

Controversial for relevantists in general:

$A, \sim A \Rightarrow B$ Lewis's first paradox (*ex falso quodlibet*)
$B \Rightarrow A \vee \sim A$ Lewis' second paradox

Controversial for some, but not all relevantists:

$A \vee B, \sim A \Rightarrow B$ Disjunctive syllogism
$\Rightarrow B\rightarrow(A\rightarrow B)$
$A\rightarrow(A\rightarrow B) \Rightarrow A\rightarrow B$ Contraction
$\Rightarrow (A\rightarrow B)\vee(B\rightarrow A)$

Controversial for quantum logicians:

$A\rightarrow(B\vee C) \Rightarrow$ Distributivity
$(A\rightarrow B)\vee(A\rightarrow C)$

Controversial for counterfactual theorists:

$A\rightarrow C \Rightarrow (A\&B)\rightarrow C$ Strengthening the antecedent

2.4. Controversial classical theorems and inferences

In some cases of logical ›reform‹ by an erstwhile deviant logician, a controversial principle may end by not being controversial in the new system. In retrospect, it proves to have served merely as a schema that provided initial inspiration for the search for a suitable *supplementation* of classical logic, rather than a deviation from it. Thus, for example, the counterfactual theorist can point to the principle of strengthening the antecedent, and say, 'This does not hold if the arrow is read as counterfactual implication'. The classicist can reply, 'Very well then, read the arrow as material implication and introduce a different arrow for counterfactual implication. You may then avoid committing yourself to the principle of strengthening the antecedent for counterfactual implication, but retain it for material implication'. And this is what counterfactual theorists — most notably, David K. Lewis (1973 a) — have now done. Similarly, Clarence Irving Lewis had earlier found the so-called 'paradoxes of material implication' objectionable as principles governing what he understood (pre-theoretically) as *implication*. This prompted him to search for a new connective (the so-called fishhook of *strict implication*) which would be governed by the 'correct' principles. That the paradoxes of material implication then ›re-arose‹ as paradoxes of strict implication was, of course, unfortunate; but the point in the present context is that C. I. Lewis was following a policy of supplementation rather than reform. — But can one always deal with objections to

classical principles by thus turning deviation into supplementation? Is ›the‹ correct system — should there be one — to be discovered by adding layer upon layer of new items of logical vocabulary (modal operators, connectives for relevant implication, strict implication, counterfactual implication and the like), and making the principles of each successive fresh layer capture the intended pre-theoretic meanings assigned to the new logical symbols? This strategy raises the worry that as layers are added, the earliest symbols dealt with — material implication, say — appear more and more to be devoid of any satisfactorily accurately apprehended pre-theoretical meaning. One is left, say, with the diverse implication connectives of the outer layers — relevant, indicative, counterfactual — invested now with satisfactorily systematised specific meanings. The original ›implication‹ connective (material implication) of the innermost core turns out to be but a first crude approximation to a general but inchoate idea of implication. The latter has now broken down upon closer analysis, and proliferated into a variety of distinct (and unitary) types of implication, at last, supposedly, severally and properly understood. One is left contemplating the conclusion that there is no such implication as material implication, except in an artificial, overly simple system invented by logicians. Alternatively, one might try retrospectively to isolate a new ›core‹ notion of implication, as done by B. F. Chellas (1975), whose ›notion of conditionality likely corresponds to no actual notion, its logic to that of no actual conditional‹. But it may, as he claims, provide ›nonetheless a foundational setting for and unifying approach to the study of the subject‹.

3. Which logic is the right logic? — Is there an answer?

If there *is* only one correct logic, containing exactly the right principles governing each ultimately unitary notion, then perhaps only the strategy of proliferation and supplementation just sketched will enable us to attain it. In that event the difficulties are enormous: for there is an extraordinary variety of competing logics, even for such apparently unitary notions as relevant implication, or alethic necessity. Are these notions not yet unitary enough? Do we have to refine and split them further — allow them to ›speciate‹ some

more? Or have we at last reached a point where we can say that a *choice* now has to be made, from within such-and-such a variety of logical systems, of exactly one of them as the (sub)system of correct principles governing the unitary notion that we believe we now have — a notion that will resist further ›speciation‹, as it were? — Historically, the ›speciation‹ of logical notions that has occurred has been *a priori*. That is, it has resulted from conceptual analysis, reflection upon meaning, and intuitions about logical connections among sentences of our natural language. The English locution 'if ... then ...', for example, has long been a target for this sort of analysis. Occasionally the investigations have seemed to be somewhat *a posteriori*, or empirical, when theorists have offered data about what people normally say; but even this can be understood as analytic reflection by proxy, as it were. The real challenge, however, to the *a priori* method has come, predictably, from those philosophers of science and logic who, with Willard V. O. Quine, reject the 'analytic/synthetic' distinction (and with it the 'a priori/a posteriori' distinction) (s. art. 86). There is now a serious body of opinion to the effect that one's choice of logic could depend on how we discover the world to be. On this view, logic is no longer a system of *a priori* principles governing thought as expressed in language. No longer may we take our language to be understood (its meanings to be grasped) in such a way as to deliver its logical structure without any particular experiences of the world. Rather, even the most fundamental ›logical‹ connections between sentences of our language can turn out to be *a posteriori* — based on experiences whose simplest and most explanatory and economical systematisation could call for the revision of various classical logical principles. The best-known example of such a view is Hilary Putnam's, to the effect that the phenomena of quantum mechanics (in particular, the two-slit experiment) are best accomodated by rejecting the classical principle of distributivity. Another is the view of D. H. Mellor (1974) that one's choice of a correct temporal logic is affected by whether relativity theory is true. — One who holds the Quinean view cannot find much of interest in the question 'Which logic is the right logic?' For it is now on a par with the question 'What is the correct theory about the world?'; and none of us can pretend to anticipate that Peircean endpoint of inquiry at which one could venture, for the

first time, a definite answer. The answer could at best be approached piecemeal. One could plump tentatively for the best ›local logic‹ in the case of each important notion or family of notions: one logic for counterfactuals, one for descriptions of quantum phenomena, one for time and tense, one for mathematical necessity, one for obligation, and so on. The provisional ›right logic‹ would then presumably be a blend of all the best ›local logics‹ chosen in this way. The assumption would be that a sufficient measure of compositionality obtains: the various ›local‹ logics will be capable of being blended for the language that results from pooling all the respective ›logical vocabularies‹. But a word of caution is called for here: one has to exercise care, for example, when accounting for how modal and temporal notions interact. — This approach would allow a pluralism of sorts within a view which is really globally partisan. I take it that a *partisan* is one who believes that there is a fact of the matter as to which logic is the right logic, even if *a posteriori* investigation is needed to identify it. A *pluralist* is one who believes that there are different logics for different purposes, applications or fields of discourse. A *relativist* is one who believes that there can be no final answer as to which logic is the right logic. Relativism and partisanship can come in degrees: one could be partisan about the logic of mathematics, say, while being a relativist about temporal logic. — Both deviance from, and supplementation of classical logic can be either pluralist or partisan in motivation (or both: remember the pluralist who is globally partisan!). Likewise, both deviance from, and supplementation of classical logic can be motivated with allegiance to a purely *a priori* method, or with allegiance to the Quinean *a posteriori* viewpoint. One can welcome a proliferation of logical systems with any one of a number of different *mélanges* of philosophical leanings and logical inclinations.

4. Recent developments in the main areas of rivalry to classical logic

I promised in the *Introduction* that in this section I would "discuss, with the necessary background, some of the most important innovations that have occurred in logical research over the past decade or so". To a certain extent I have already begun that task in 3. I shall continue to use the map of 2.1.

to locate points of departure from classical logic — be they deviant or supplementary. The ›axes‹ or ›dimensions‹ (and of course ›choice of coordinates‹) of the map given are ones along which there has been variation for at least some systems of logic in the past. But of course not every historical departure from classical logic was conceived in such a way as to make every dimension immediately relevant in locating the resulting system. As further research was subsequently carried out, however, the picture was filled in, so to speak, with the use of more varied coordinate systems, and by fixing coordinates on more and more of the characterising dimensions.

4.1. Intuitionistic logic

Thus, for example, when Arend Heyting (1930) first formulated intuitionistic logic it was by selecting certain axioms in a Hilbert-style presentation, and commenting rather loosely on how they captured what was essential to the notion of mathematical construction. Only much later did Saul Kripke (1963) provide an intensional (possible worlds based) semantics that allowed the first completeness proofs (s. art. 88). In the period in between, various logicians investigated the relationship between intuitionistic logic and classical logic, with such results as the Gödel-Glivenko theorem (see Glivenko 1929) to the effect that one can embed classical logic into intuitionistic logic upon a suitable translation (the so-called 'double negation interpretation'). There have since been numerous other interpretations that allow similar embeddings (see Minc/Orevkov 1963, Prawitz/Malmnäs 1968, Friedman 1973, Leivant 1985; and for a full survey, see Tennant 1989.) Some of these also concern set theory as developed using intuitionistic or classical logic. Two of the main by-products of such embedding results are relative consistency proofs and the transfer of undecidability results (such as Church's theorem for first order classical logic) from the classical to the intuitionistic case. — Soon after Heyting's purely syntactic characterisation of intuitionistic logic, S. Jaśkowski (1936) provided an infinite sequence of ever larger finite matrices with the property that every non-theorem of intuitionistic logic would be falsified by some matrix in the sequence; Kurt Gödel (1932) had earlier shown that it is impossible to provide a characteristic finite matrix for intuitionistic logic. Matrix methods sufficed, however, for showing that all the intuitionistic connectives were inde-

pendent of one another (Wajsberg 1938). Ger-
hard Gentzen (1934/35) provided his now
justly famous analysis of intuitionistic and
classical reasoning by means of the systems
of natural deduction and sequent proof. In
this setting the difference between classical
and intuitionistic logic turned out to consist
in the so-called classical rules of negation (in
natural deduction), or the use of multiple
succedents (in sequent proofs). The new for-
mulations were important for the develop-
ment of a general theory of the structure of
proofs, and attempts to provide consistency
proofs for arithmetic and real analysis.
Gentzen proved the cut-elimination theorem
for his sequent system, and subsequently Dag
Prawitz (1965) proved its ›analogue‹, the nor-
malization theorem, for the system of natural
deduction. The latter, however, holds in the
classical case only if '∃' and '∨' are omitted
from the language. The natural question con-
cerning the eliminability of dilutions in se-
quent proofs was raised and settled by Ten-
nant (1984), with ramifications for an analysis
of the notion of 'relevance' of premises to
conclusions of (intuitionistically or classi-
cally) valid arguments. Corresponding to di-
lution elimination in sequent proofs is the
extraction theorem (Tennant 1980; 1987 a;
1987 b) for natural deduction, stating that
applications of the absurdity rule can be elim-
inated in a particular way in order to ›rele-
vantise‹ the deduction. — Other develop-
ments in our understanding of intuitionistic
logic could perhaps be cited, but those above
are sufficient to make my general point. It is
that we are now historically placed in a po-
sition from which we can survey the accu-
mulated differences between the two systems,
without regard for the order of historical dis-
covery, and trace the variations along each of
several dimensions in the map above. Indeed,
the features on which I have already focussed
virtually exhaust the variations. Although in-
tuitionistic logic is, both historically and phil-
osophically, one of classical logic's most for-
midable rivals, the variations are confined to
just a few philosophically contentious fea-
tures. These have, by and large, to do with
classical negation and the concomitant dual-
ities and interdefinabilities that it induces.
There are still many features that the two
systems have in common. Both are based on
semantically open languages. Both have for-
mulations at first order, higher order, within
type theory, and as combinatory logics. Both
have the logical vocabulary listed above, with

the difference now that in intuitionistic logic
the smallest expressively complete set of op-
erators consists of all those mentioned. At
higher order, however, just '→' and '∀' are
enough, and are required (see Ballard 1985).
The systems do not differ in restrictions or
allowances in respect of extra-logical vocab-
ulary, and they enjoy the same rules of sen-
tence formation. Sentences are finitary and
linear, and one is free to choose whether to
have variable-binding term-forming opera-
tors (such as the definite description opera-
tor). — The first major difference between
the two systems non-chronologically encoun-
tered on the map above concerns semantic
interpretation. In this respect intuitionistic
logic has a semantics best characterised as
proof-based or intensional. The latter is based
on an accessibility relation among possible
worlds that is reflexive and transitive. Within
worlds, primitive predicates do not in general
form partitions of the appropriate Cartesian
product of the domain; rather, each primitive
predicate is assigned a positive extension, and
its negative extension at any world consists
of just those individuals that do not find their
way into its positive extension in any acces-
sible world. In this way bivalence fails for
primitive predication; and its failure on prop-
ositional variables is similar. Its failure for
complex formulae is further guaranteed by
the evaluation rules for the conditional and
the universal quantifier. Both of these involve
›forward looking‹ search down the accessibil-
ity relations; so that what is held to be true
at any given world depends at least in part
on what is true at other worlds accessible
from the given world. — When we consider
the other varieties of semantic interpretation
that are to be found in the literature, it turns
out that both classical and intuitionistic logic
can be equally served. Both have game-the-
oretic semantics: Jaakko Hintikka (1973) pro-
vided material game semantics for classical
logic, Tennant (1979) for intuitionistic logic.
The major difference for intuitionistic logic is
that the winning strategies have to be effec-
tive. Paul Lorenzen and Kuno Lorenz (1978)
provided dialogue game semantics for both
systems. The major difference for intuition-
istic logic has to do with permitted repetitions
of defence moves. Hughes Leblanc et al.
(1971) have provided truth-value semantics
for both systems. H. Field (1977) provided
probabilistic semantics for classical logic;
C. G. Morgan and Leblanc (1983) and J. A.
Paulos (1981) its analogue for the intuition-

istic case. In addition there have been other special developments in our understanding of intuitionistic logic. I have not so far mentioned Evert W. Beth's tree semantics and tableau method of proof (the latter nicely presented in Fitting 1979); or the more recent development of intuitionistically acceptable notions of model, and completeness proofs for intuitionistic logic carried out in an intuitionistic metalanguage. W. Veldman (1976) and de Swart (1976) were the first to succeed in this regard. Lately, J. T. Kearns (1978) has proposed a ›justification value‹ semantics for intuitionistic logic, and J. P. Burgess (1981 a/b) has shown intuitionistic logic to be correct on the interpretation according to which any theorem φ is such that one should find intuitively correct any mathematical sentence that results by uniformly substituting mathematical sentences for propositional variables in φ.

It may seem that the discrepancy over double negation has done little more than force one (in the intuitionistic case) to intensionalise the semantics, help oneself to a bigger stock of independent logical operators, and be more frugal in one's proof methods. Results about completeness and undecidability appear to run parallel (with the exception that monadic intuitionistic logic is undecidable), and we have numerous equiconsistency results concerning theories based on the two logics (for a good survey of which, see Sieg 1984; 1985). Both logics have the compactness property, the joint consistency property (Robinson), the interpolation property (Craig) and the definability property (Beth). — But recently some exciting results have been proved concerning what happens when one is rigorously intuitionistic *in the metalanguage*. Exploiting a general interpretation that he has developed between classical recursive mathematics and intuitionistic set theory, D. C. McCarty (1984; 1987; 1988) has been able to prove some notable results about intuitionistic model theory. The main ones are that the countable models theorem (also known as the downward Löwenheim-Skolem theorem) fails in any intuitionistic set theory, no matter how strong (see McCarty/Tennant 1987); and that intuitionistic first order arithmetic is countably categorical! These results seem to show, for the first time, that there are pronounced foundational and philosophical benefits to be had from intuitionism quite apart from the motivation to confine oneself to intuitionism that is provided by an anti-

realistically acceptable theory of meaning for the logical operators. The benefits are that in theorising about the real numbers the intuitionist is no longer bedevilled by Skolem's paradox that the intended model, in all its uncountable riches, is ineffable; and in theorising about the natural numbers one is no longer bedevilled by the possibility of nonstandard interpretations of what one is saying. It is as though there is a trade-off to be had between deductive and expressive power. By foregoing some of the (illicit?) deductive manœuvres of classical logic, the intuitionist is thereby placed in a position from which the natural numbers and the continuum can be better apprehended and described. In the remainder of this section I turn to other logics that deviate from classical logic, and some logics that supplement it.

4.2. Relevance logics: the Anderson-Belnap approach

After intuitionistic logic, perhaps the best known and most extensively investigated kind of deviant logic are the logics of relevance and entailment. These have been given encyclopaedic coverage in Anderson and Belnap's volume (1968). Systems of propositional logic have received more attention than first order logic. The main systems are E (of entailment) and R (for relevance), and proximate restrictions and extensions of these, such as T (ticket entailment), R-Mingle, and R-W. All have the characteristic feature that entailment or relevant implication is represented by a connective arrow '$\rightarrow$' which is construed as distinct from material implication '$\supset$'. They are systems for which the principle of transitivity of deduction holds unrestrictedly, but disjunctive syllogism fails. They all lack the Lewis paradoxes. They are also based on the guiding ideal that relevance of premises to conclusion of a correct argument involves variable-sharing. Intensional analogues of conjunction and disjunction, called fusion and fission, can be introduced by definition or by expansion of primitive vocabulary, and the interplay among these operators, their standard extensional analogues, and the new arrow of relevant implication or entailment are a matter of delicate detail. Despite the decidability of various subsystems $E_\rightarrow$ and $R_\rightarrow$ (Kripke 1959), $E_{\rightarrow,\sim}$ and $R_{\rightarrow,\sim}$ (Belnap/Wallace 1965), RM and LR (Meyer 1966), TW_+ and RW_+ (Giambrone 1985), giving rise, perhaps, to the overly optimistic publication (Vojshvillo 1983), the systems $R,\ E$ and T are all undecidable (Urquhart 1984). They can, however,

be given possible worlds semantics based on a ternary accessibility relation and a so-called ›star operator‹ on worlds. $\sim A$ is true at a world w just in case A is true at w^*; $A{\rightarrow}B$ is true at world w just in case for all worlds u, v such that $Rwuv$, if A is true at world u then B is true at world v. Just as in the case of standard modal logics, differences among systems can be registered as differences in the algebraic conditions governing the (two-place) accessibility relation among possible worlds, so too in the case of relevance logic the differences among the various systems can be registered as differences in the algebraic conditions governing the new three-place relation and the star operation. It is controversial at least how illuminating such ternary world (or ›set-up‹) semantics is from a philosophical point of view. Some writers, such as Copeland (1979; 1980), are unimpressed by the supposed philosophical clarification it affords; others, such as Routley/Routley/Meyer/Martin (1982), have disagreed.

4.3. A new approach to relevance logic

One other criticism that can be made of relevance logics in this style is that they conceive of the problem and its solution as having to do with the choice of a connective arrow in the object language. A different approach (favoured by the present writer) is to try to solve the problem of relevance as one concerned first and foremost with the deducibility relation, even in a language restricted, say, to the connectives '$\sim$', '$\vee$' and '&'. This way it can reasonably be expected that when we come to characterise a relevant conditional '$\rightarrow$' in the object language by means of some sort of rule of conditional proof, the prior analysis of relevant deducibility will ensure that the new connective will have relevance as an ingredient of the meaning conferred on it by that rule. To this end the natural deduction and sequent formulations of intuitionistic and classical logic have proved especially natural in providing background motivation for various restrictions and reformulations of rules (governing the usual logical operators) when in pursuit of relevance. This new approach, based on an analysis of those features of *proof* that ensure relevance, has led to systems of relevance logic markedly different from R and its cousins. It has turned out to be especially fruitful to give up insistence on unrestricted transitivity of deduction (thereby enabling one to recover disjunctive syllogism as a valid mode of relevant reasoning). One does this in

the sequent system by not having cut or dilution as structural rules. Thus apart from the rule $A{:}A$ of initial sequents, the only other rules are those for introducing logical operators on the right and the left of sequents. Corresponding to these measures in the natural deduction formulation, one basically requires all proofs to be in normal form, to have no applications of the absurdity rule, and to contain no vacuous applications of certain discharge rules. A vacuous application is one for which there are no assumptions of the indicated form actually to be discharged by that application. — The failure of transitivity of deduction that results from these restrictions is confined to cases where the newly combined premises are inconsistent, or the sought conclusion is a logical truth. Thus when transitivity fails we have net epistemic gain rather than logical loss. On consistent sets of premises and conclusions that are not logically true the new relevant deducibility relation coincides with that of the parent ›non-relevant‹ logic (classical or intuitionistic, as the case may be). Moreover, all inconsistencies and all logical truths of the parent logic remain provable in the relevant fragments just described. Another comparative virtue that this approach to relevance can claim is that one can define a natural semantic notion of entailment with respect to which the proof system is adequate. For the classical system based on '$\sim$', '$\vee$' and '&', an *entailment* is defined as any substitution instance of a *perfectly valid* sequent — that is, a valid sequent that has no valid proper subsequents. Thus for example the sequent $A, \sim A{:}B$ is *not* an entailment because every valid sequent of which it might be a substitution instance has a valid proper subsequent, and so is not perfectly valid. The sequent $A, \sim A{:}B$ itself has a valid proper subsequent in $A, \sim A{:}\emptyset$ — taking care of the identity substitution; and the only other valid sequents that we have to consider in which substitution would yield $A, \sim A{:}B$ are those that would produce $A, \sim A{:}B$ by reletterings of atoms, and to these sequents the same consideration applies. On the other hand, $A \vee B, \sim A{:}B$ *is* an entailment, because it (is a substitution instance of itself and) is perfectly valid (that is, it has no valid proper subsequents). As a final example, $A\&\sim A{:}A$ is an entailment, because it is a substitution instance of $A\&B{:}A$, which is perfectly valid. This semantic notion of entailment involves no new interpretation of the logical operators themselves. It turns solely on considerations

of ›tightness of connection‹ between premises and conclusion, and the idea (of long standing) that any good notion of validity or deducibility should be preserved under uniform substitutions.

4.4. Paraconsistent logics

Relevance logics involve variable-sharing, and do not have the Lewis paradoxes. The absence of the first Lewis paradox means that one cannot, in a relevance logic, deduce B from the premises $A, \sim A$. This allows for the possibility of inconsistent but non-trivial theories. Theories are sets of sentences closed under deducibility. In standard systems (such as classical or intuitionistic logic) there is only one inconsistent theory, namely the whole language. This is because the Lewis paradox ›blows up‹ any inconsistent set so that its logical closure contains every sentence. Not so in relevance logic, however: here the absence of the Lewis paradox raises the interesting possibility that inconsistencies in theories need not be ›infectious‹. We may be able to contain or localise them, and work with parts of the theory that are ›quarantined off‹ from the source of the trouble — exactly how being an interesting further topic in the development of a theory sensitive to the fine structure of proofs in the system.

This prospect of *paraconsistent* theorising has appealed in particular to those who would like to retain the naive formulation of set theory, in which the only axiom schema is that of comprehension (abstraction). It has also been suggested by G. Priest (1984 a; 1984 b) that the bold approach to the semantic paradoxes is the best: that is to regard semantic paradoxes, such as the Liar, as both true and false, and to be paraconsistent in one's reasoning with them. The logics to be investigated in these applications have been called 'paraconsistent'. The result is that we have now two labels — 'paraconsistent' and 'relevant' — that need to be distinguished. A paraconsistent logic simply allows there to be non-trivial inconsistent theories, where theories are sets of sentences closed under the deducibility relation of the logic. Thus all relevance logics are paraconsistent. But not all paraconsistent logics are relevant; for it is possible to avoid Lewis's first paradox, while admitting the second ($A:B \vee \sim B$), or admitting other ›fallacies of relevance‹. — In pursuit of paraconsistent naive set theory, much attention was paid to relevance logics in which contraction fails. This is because even though Russell's paradox can be ›contained‹ by the paraconsistency of the logic, nevertheless a ›Curry-type‹ instance of naive comprehension renders every sentence p provable. The proof, set out below, shows the role played by contraction — here, in the ability to discharge two occurrences of an assumption for conditional proof. The naive comprehension schema, for a language in which all set abstracts are taken to denote, is

$$\forall \, y \, [y \in \{x|Fx\} \leftrightarrow Fy]$$

which has the ›Curry instance‹

$$\forall \, y \, [y \in \{x|x \in x \rightarrow p\} \leftrightarrow (y \in y \rightarrow p)]$$

Let C abbreviate the set abstract $\{x|x \in x \rightarrow p\}$. By instantiation we get

$$C \in C \leftrightarrow (C \in C \rightarrow p)$$
which we abbreviate further to

$$A \leftrightarrow (A \rightarrow p)$$

We now show that this implies A, by means of a proof in which contraction is conspicuous:

$$
\begin{array}{c}
(1) - \\
(1) - \dfrac{A \quad A \leftrightarrow (A \rightarrow p)}{\dfrac{A \qquad A \rightarrow p}{\dfrac{p}{\dfrac{A \rightarrow p \quad A \leftrightarrow (A \rightarrow p)}{A}}(1)}}
\end{array}
$$

Call this proof Π. Two more steps yield p:

$$
\dfrac{\Pi}{\dfrac{\Pi \quad A \quad A \leftrightarrow (A \rightarrow p)}{\dfrac{A \qquad A \rightarrow p}{p}}}
$$

Getting rid of contraction, however, has not succeeded in avoiding this sort of result. Naive set theories based on relevance logics without contraction have been shown not to be ›Curry paraconsistent‹. The most central case, for R without (the axiom W of) contraction but with the law of excluded middle added, was established by Slaney (forthcoming a). It is an open problem whether the presence of excluded middle is necessary for this result — that is, whether naive set theory based on the logic R-W is Curry paraconsistent. (R-W, though it contains the law of double negation $\sim \sim A \rightarrow A$, does not contain the law of excluded middle $A \vee \sim A$.) R. T. Brady (1983) shows that the logic CSQ (an ›intensionalized Łukasiewicz three-valued predicate logic‹) allows one to develop a sim-

ply consistent naive set theory; Brady (1984; 1988) shows the same for the system T-W of ticket entailment without contraction. Thus, even though the usual proof of Russell's paradox delivers a conclusion of the form $A \leftrightarrow \sim A$, the logic T-W is too weak to allow one to derive from this an explicit contradiction of the form $B \& \sim B$. The difference between R and T (the system of ticket entailment) consists mainly in the presence or absence respectively of the permutation axiom $(A \rightarrow (B \rightarrow C)) \rightarrow (B \rightarrow (A \rightarrow C))$. It is questionable, however, whether the simple consistency of naive set theory based on T-W or on CSQ will be fully satisfying to the paraconsistent theorist who wishes to recover most of the strength of ordinary ZF while yet employing some method of blocking derivations of arbitrary sentences. T-W and CSQ may simply be too weak to develop enough set theory, naive though it may be. — There are nonetheless other paraconsistent logicians who have hoped, by truncating logic in still different ways, to realise the dream of a paraconsistent naive set theory. Yet other systems — most prominently, those of N. C. A. da Costa (1982) and A. J. Arruda (1980); but also, the anti- or dual-intuitionistic system of N. D. Goodman (1981) — have been devised, in which, it is hoped, the Curry-style trivialisation will be blocked. Some of these systems are unusual in that they contain the law of double negation ($\sim \sim A \Rightarrow A$) but lack its intuitionistically unexceptionable converse ($A \Rightarrow \sim \sim A$). They also achieve paraconsistency without the full strictures of relevance.

4.5. Modal logics

Turning now to logics that *supplement* classical logic, rather than deviate from it, I shall single out only one type — modal logic — for comments on recent developments. When C. I. Lewis and Langford (1959) first invented their modal logics, they were set up by means of axioms and rules of inference. They eluded interpretations by means of finitely many truth values; and indeed J. Dugundji (1940) proved that no Lewis system has a characteristic finite matrix. Only recently (Kearns 1981) has a ›Jaskowski style‹ method of valuation by infinitely many finitary matrices which yielded soundness and completeness proofs for the well-known systems T, $S4$ and $S5$. The first uniformly successful method of semantic interpretation employed possible worlds ordered by an accessibility relation (Kripke 1963). Corresponding to this approach was

the ›syntactic-looking‹ method of saturated model sets, due to Hintikka (1961) and Stig Kanger (1957 b). Possible worlds semantics has now become the dominant paradigm with only relatively minor variation, such as that of J. L. Humberstone (1981) and Barwise/ Perry (1983), in which incompletely specified possible worlds are used. In possible worlds semantics, the various distinctions between extant modal systems were found to correspond to different conditions on the accessibility relation among possible worlds. Only recently have the limitations of this method become apparent, for those aspiring to complete generality of treatment of all modal logics. It has been shown that some modal systems cannot be characterised by any possible worlds semantics employing first-order conditions on their accessibility relations (Goldblatt 1975; van Benthem 1984 c). There is even a finitely axiomatized normal modal propositional logic which is not characterised by *any* class of frames (Cresswell 1984). The vast majority of philosophically interesting modal logics, however, are now much better understood in terms of the characteristic frames of worlds that have been supplied for them. The paradigm prevails, despite these minor anomalies. It has also, recently, started to yield interesting modal *theories* designed to characterise various essentialist ontologies; the papers by Kit Fine (1978 a; 1978 b; 1980; 1981; 1982 c) are a prodigious pioneering effort in this connection. — Before the possible worlds paradigm, attempts had been made to show that certain ›modalities‹ were well captured by certain of the Lewis systems. In particular, the system $S5$ seemed to correspond nicely to a reading of the modal operator '□' as 'It is analytically true that'; while $S4$ seemed to correspond to the reading 'It is mathematically provable that'. Recently, however, attempts have been made to render the latter interpretation much more precise, with respect to provability within some formal system such as Peano arithmetic. Two readings for '□' have been suggested. Each is relativised to a *translation* φ of the language of the propositional modal logic into the language of first order arithmetic. φ maps sentence letters of the propositional modal logic to sentences of arithmetic, and is extended to complex sentences in the obvious way. The only step at which care is needed is with the φ-translation of sentences of the form $\square A$: here, for what is known as the ›provability translation‹ we set $\varphi(\square A) = \text{Prov}(\varphi(A))$,

where 'Prov' is the provability predicate for arithmetic, and $\varphi(A)$ is the numeral for the Gödel number of the φ-translate of A; while for what is known as the ›truth interpretation‹, we set $\varphi(\Box A) = \text{Prov}(\varphi(A))$ & $\varphi(A)$. Four questions now arise, with the answers indicated after each:

(1) Which modal logic Λ is such that for all translations φ, A is a theorem of Λ iff the provability-translate of A is provable?
[The system G, Solovay 1976]

(2) Which modal logic Λ is such that for all translations φ, A is a theorem of Λ iff the provability-translate of A is true (in the standard model)?
[The system G^*, Solovay 1976]

(3) Which modal logic Λ is such that for all translations φ, A is a theorem of Λ iff the truth-translate of A is provable?
[The system $S4Grz$, Goldblatt 1978; Boolos 1980 a]

(4) Which modal logic Λ is such that for all translations φ, A is a theorem of Λ iff the truth-translate of A is true (in the standard model)?
[The same system $S4Grz$, Boolos 1980 b]

G and $S4Grz$ are both normal modal systems: that is, they contain as axioms all tautologies, all sentences of the form $\Box(A \supset B) \supset (\Box A \supset \Box B)$, and are closed under the rules of *modus ponens*, necessitation and substitution. K is the smallest normal system thus defined. The axioms of G are those of K plus all sentences of the form $\Box(\Box A \supset A) \supset \Box A$. The axioms of $S4Grz$ are those of $S4$ plus all sentences of the form $\Box(\Box(A \supset \Box A) \supset A) \supset A$. The system G^* is *not* normal, however, as it is not closed under necessitation. G^* is the closure, under *modus ponens*, of G plus all sentences of the form $\Box A \supset A$. G is sound and complete with respect to the class of finite frames whose accessibility relations are transitive and irreflexive. $S4Grz$ is sound and complete with respect to the class of finite frames whose accessibility relations are transitive, reflexive and anti-symmetric. The new modal systems are located in the partial ordering by inclusion with respect to the other well-known systems as follows:
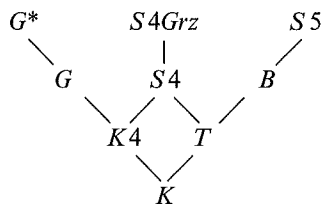
We therefore see how the original philosophical claim that the system $S4$ captures for '$\Box$' the modal reading 'It is mathematically provable that' has been modified and refined by the study of G and related systems.

5. Important developments not discussed in this paper

In 4. I have been able to cover briefly only some of the most important recent developments in logic; lack of space prevents me from covering more. I have, however, concentrated on the three areas that I believe are central, concerned as they are with the nature of proof and provability: intuitionism, relevance and the modal logic of provability. I have said nothing about recent developments in the following areas listed below. On those marked with an asterisk, however, I shall make some remarks in the remaining sections of this paper. These omissions could be remedied only by writing several books. I therefore confine myself here to giving selected references to what I take to be the most significant or informative recent sources on these topics:

(1) Logic of non-existent objects
(Lambert 1981; Parsons 1980 a; Routley 1980)

(2) Logic of demonstratives
(Kaplan 1979 b; Stalnaker 1981)

(3) Propositional dynamic logic (the logic of programs)
(Csirmaz 1985; Davis 1980; Hájek 1983; Manders/Daley 1982; McDermott/Doyle 1980; Meyer et al. 1981; Mirkowska 1981; Parikh 1981; Pratt 1979; 1980; Segerberg 1980; Stolboushkin/Taitslin 1983; Street 1982)

(4) Quantum logic
(Cutland/Gibbins 1982; dalla Chiara 1986; Erwin 1978; Goldblatt 1974; Holdsworth/Hooker 1983; Nishimura 1980; Stachow 1976)

(5) Logic of vagueness (fuzzy logic)
(Baldwin 1979; Baldwin/Guild 1980; von Kutschera 1984)

(6) Deontic logic
(Hilpinen 1971; Jackson 1985)

(7) Logic of counterfactuals
(Burgess 1981; van Benthem 1984 a)

(8) Logic of various problematic constructions in natural languages: attributive adjectives, plurality quantifiers, mass terms, adverbs, verbs of propositional attitude, comparatives, branching quantifiers
(Kamp 1981; ter Meulen 1981; van Benthem 1984 b)

(9) Intensional logic of properties, relation and propositions
(Bealer 1983)

(10)   Many-valued logics
         (Belnap 1977; Urquhart 1986)

(11)   Theory of ›arbitrary objects‹ as a new form of semantics
         (Fine 1985)

(*12)   Dynamics of theory change (Alchourrón et al. 1985; Alchourrón/Makinson 1985; Gärdenfors 1982; 1984; 1985 a; 1985 b; 1986; Makinson 1985)

(*13)   Attempts to characterise the notion of verisimilitude
         (Miller 1974 a; 1974 b; 1976; Oddie 1986; Pearce/Rantala 1983; Tichy 1974; 1976)

(*14)   ›Fixed point‹ semantics, ›stability‹ semantics and ›stage‹ semantics for semantically closed languages
         (Fitting 1986; Gupta 1982; Gupta/Martin 1984; Gupta/Martin 1985; Herzberger 1982 a; 1982 b; Kripke 1975; Martin/Woodruff 1975; Woodruff 1984; Yablo 1982; 1985)

(15)   ›Conservative extension‹ arguments for anti-Platonism in science
         (Field 1980; Shapiro 1983)

(16)   Use of definability theorems in the debate about reductionism in the sciences
         (Bealer 1978; Hellman/Thompson 1975; Tennant 1985)

(*17)   Unification of proof methods, and the study of highly general forms of inference rules; advances in providing sequent formulations for interesting logics
         (Belnap 1982; Schroeder-Heister 1984; 1987; Slaney forthcoming b)

(18)   Denotational semantics for lambda calculus
         (Barendregt 1981; 1984; Hindley 1983 a; 1983 b; Hindley/Longo 1980; Meyer 1982; Scott 1973; 1975; 1976)

(19)   Theory of stability in classical model theory
         (Baldwin 1979; Cherlin 1979; Cherlin et al. 1985; Pillay 1983; Shelah 1978; 1982 a; 1982 b; 1985; Shelah et al. 1984)

(20)   Results in ›soft model theory‹ concerning extensions of first order classical logic: especially failure of the Beth property and the Craig property
         (Barwise 1974; Barwise/Feferman 1985; Cowles 1979; Friedman 1976; Gregory 1974; Lindström 1973; Makowsky/Shelah 1979; Malitz 1971; Mundici 1981 a; 1981 b; 1982 a; 1982 b; 1983 a)

(21)   Finitary consistency proofs for relevant arithmetic (so-called R#)
         (Dunn 1979 a; 1979 b; Meyer/Mortensen 1984; Meyer/Urbas 1986)

(22)   Largely negative results about feasible decidability (i.e. decidability in polynomial time) of mathematical theories
         (Ferrante/Rackoff 1979; Lewis 1980; Manders 1980; Mundici 1983 b; Wohl 1979; Young 1985)

(23)   Exploration of the expressive limitations of first-order logic for mathematical practice; exploitation of the resources of second-order logic, both for greater expressive power and for proof ›speed-up‹
         (Boolos 1975; Shapiro 1985)

(24)   Reverse mathematics: the study of the equivalence *modulo* a given system of second order arithmetic, of stronger set existence assumptions with important theorems of ordinary mathematics
         (Brown/Simpson 1986; Friedman 1974; 1975; Friedman et al. 1983; 1985; Harnik 1985; Simpson 1984; 1985 a; 1985 b)

(25)   Equivalence of classical recursive mathematics and intuitionistic mathematics
         (McCarty 1984; 1987; 1988)

6. What criteria should be involved in the choice of a logic?

My tentative answer to this question is neutral between the *a priorist* and *a posteriorist* views discussed in 2. It is that logic should be based largely (if not wholly) on considerations of *meaning* of the logical operators or of whatever other logico-linguistic constructions are in question. The reason why this answer is neutral between the two positions mentioned is that it is a matter of further debate whether the theory of meaning is itself a purely *a priori* discipline. In its fullest development it could be based not only on educated speakers' intuitions about normative connections, criteria for assertion and so forth, but also on empirical theories of language acquisition, in which the importance of the *molecularity* of meaning for learnability was stressed. Meaning, according to anti-realists, features most accessibly in the theoretical notion 'X knows the meaning of E'; and knowledge of meaning (understanding) is a psychological state that must be exhaustively manifestable (cf. Dummett 1975; 1976; Prawitz 1977; Tennant 1987 a) (s. art. 68). The requirements of molecularity and manifestability can be regarded as quasi-methodological constraints on the construction of any theory of meaning (and of knowledge of meaning), without making that theory cease to be sensitive to empirical facts about language in use. My own view is that these requirements, on a theory of meaning for mathematical discourse at least, go a long way to force a choice of correct logic in the neighbourhood of as constrained a system as *intuitionistic relevant logic* (for details of which, see Tennant 1987 a, Part II). — But what, it may be asked, about areas of meaning (such as tensed verbs and temporal discourse) where speakers' intuitions may be in precise agreement, ›logical‹ principles apparently unassailable, grasp of meaning fully manifested

in their use of temporal language, meaning-contribution of temporal vocabulary wholly molecular, but the resulting logic actually *wrong* — because, say, it is based on an incorrect physics (or metaphysics) of space and time? Which is *the* correct temporal logic in such a case? Folk tense logic? or scientific tense logic? In the face of such a challenge I have considerable sympathy for the sophisticated view: that one may have to turn to science to put our ›logic‹ right. But that is because the word 'logic' is rightly in scare quotes here. Temporal operators may well be equipped with logical-looking axioms and rules in a deductive system; they may well be furnished with Tarskian or Kripkean clauses in a definition of ›truth-at-an-index‹ in some ›possible worlds‹ or ›possible histories‹ model; they may well bear a strong family resemblance in this fashion to the usual logical and modal operators; but it is a deceptive similarity, and one that should not necessarily lead us to believe that we are dealing with a *logic* of *logical* notions. For, deeply embedded in the structure of the model, and ultimately reflected in the ›deductive‹ relationships among sentences on which speakers agree, is a nest of assumptions about the *topology and geometry of time*. As J. P. Burgess (1979) has put it, even for a ›simple, purely propositional tense logic, which inferences are to be recognised as valid depends on one's cosmology‹. — I would rather take the view that the valid formulae or inferences of a tense logic are really theoretical statements about the structure of time (or of space-time) in the syntactic guise of sentences containing logical-looking operators. These cosmological assumptions are (or should be) drawn from the best current physical theory about the behaviour of matter in space and time. Not that temporal logicians have been lazy; to quote Burgess again, ›The question of time's local structure is as old as Zeno, but still unsettled. Tense logicians have surveyed all the alternatives‹. The resulting systems deal with tense operators such as ›it will always be the case that‹, ›it has always been the case that‹, and ›it is necessary, given the past and present, that it will be the case that‹. Time can be taken as having various structures. Conceiving of time as a set of instants ordered by the 'earlier than' relation, one can consider time with or without beginning, with or without end, dense or discrete, Dedekind-continuous, forward or backward convergent, and homogeneous metrizable. By contrast, conceiving of time as a set of

branches (histories) in a tree-structure (a ›garden of forking paths‹), one can formulate an indeterministic ›Ockhamist‹ tense logic, exploiting the multiplicity of possible paths into the future. All the propositional tense logics obtained in these ways are axiomatizable, and, in all cases but the last, finitely so. All are decidable (see Burgess 1980 for the last case). Conceiving of time, finally, as bound up with space in a Minkowskian frame of point-events, one can distinguish two senses of ›it will be the case that‹: either ›there is some absolutely future point from here/now at which it is the case that‹, or ›to every inertial observer there is a point in his/her future at which it is the case that‹; where ›future‹ here means ›in the forward light cone‹. Axiomatizability of the resulting logic is an open problem. — If tense ›logic‹ is really only cosmology in syntactic guise, what parts of logic are logic proper? The straightforward answer is that it is those parts (i. e. sentences and inferences) that count as valid by virtue only of the meanings of the *logical* operators. Circularity threatens unless we can isolate the logical operators by means of some independent criterion. One criterion that has been proposed is proof-theoretic: a logical operator is one governed by inference rules that deal with *it alone*. In the statement of the rules one is not allowed to mention explicitly any other operator. One is allowed to mention only the schematic constituents (of the appropriate syntactic categories) that are contained in the conclusion (or major premiss) of the rule, or that are placeholders for items of cognate categories (such as parameters in the statement of quantifier rules). It is difficult to make more precise the constraints on what sorts of rules may thus count; but a good attempt to characterise the most general possible form of such a rule in a natural deduction setting is by Peter Schroeder-Heister (1984). In general, out of respect for the molecularity of meaning expected of a logical operator, a proof-theoretic approach to the meanings of logical operators would be searching for *either* (i) a natural deduction formulation for which a normalization theorem holds, or for which a principle of harmony between introduction and elimination rules can be formulated; *or* (ii) a sequent formulation for which a cut-elimination theorem holds. Whether, and, if so, how the alethic modal operators of necessity and possibility can be accommodated within this general format is a controversial problem (see

Hacking 1976; Došen 1985). I am inclined to count them, like tense operators, as non-logical in character, even if only because modal logical relations are determined at least in part by a *theory* (albeit a metaphysical one) as to the structure of alternative possibilities, or ways things might be. — If the proof-theoretic constraints mentioned afford a principled distinction between logical operators on the one hand, and merely quasi-logical operators (such as alethic modalities) or straightforwardly non-logical operators (such as tenses) on the other hand, then one can press the question with its presupposition of partisanship: which logic is the right logic? The answer has to satisfy certain demands of adequacy. First, there are the philosophical demands, as mentioned above, of molecularity of meaning and manifestability of grasp of meaning; and exactly how these translate into preferred formats for logical rules is a difficult question. Secondly, there is the demand of what might be called 'instrumental adequacy': is the logic strong enough to deliver what is needed for the hypothetico-deductive method in science, for truth theory, for (at least constructive or intuitionistic) mathematics, and for any other purpose for which one believes that logic is essential? For the hypothetico-deductive method, all one really needs is the logical strength to prove inconsistencies wherever they are; and for intuitionistic mathematics all one needs, in addition, is the strength to prove those non-logical truths that follow logically from consistent sets of axioms. I have argued at book length (Tennant 1987a) that intuitionistic relevance logic meets the philosophical demands, and also proved that it meets the instrumental demands. But whether and how it should be supplemented by various modalities, implications and other logico-linguistic constructions is a question that has yet to be pursued. The final position might well be one of overly deviant partisanship on the logical core, surrounded by theoretical alternatives (systems of modality, tense etc.) whose peripheral adoption as ›logics‹ of these notions may undergo continual revision.

7. Some promising and important areas of future research

I turn finally to the task of indicating what I believe are some promising and important areas of future research. With increased proliferation of logics comes the need to consolidate our understanding of them. Understanding is often improved by finding a fundamental theme on which the different systems are variations. Thus, for example, we have the basic idea of possible worlds and accessibility relations, with different systems corresponding to variations in conditions on the accessibility relation. Alternatively, on the proof-theoretic side, we have the basic format of natural deduction, with different systems corresponding to variations in conditions on discharge of assumptions, constraints on subordinate deductions etc. There has been interesting work recently (Slaney 1985 and forthcoming; Schroeder-Heister 1987; Belnap 1982) on uniform proof-theoretic formats within which variations produce different deductive systems. It is to be hoped that these will be perfected to the point where we have a scheme of comparison that will permit us to translate philosophical arguments for and against the choice of a particular system into adequacy conditions on variations within the format, variations that will focus the technical choice on the philosophically preferred system. This need is especially pressing with quantum logic. Ian Hacking once remarked that if there were a Nobel Prize for logic, it should go to the first person to provide a sequent formulation of quantum logic that admitted cut-elimination. A sequent formulation is available (Nishimura 1980); but it does not admit cut-elimination. — Change of logic brings change of metatheory; and cutting down on classical logic to a highly constrained fragment brings with it the prospect of connecting two fascinating areas of current metalogical research: the *theory of verisimilitude*, and the logic of belief change, or *theory dynamics*. I shall digress at some length about verisimilitude, before reaching a point where I can bring the two areas into contact. The basic relation that needs to be explicated in the theory of verisimilitude is 'Theory U is closer than theory V to theory T' (abbreviated as $[U,V,T]$). In the traditional setting, theories U and V are false but consistent classical theories, and theory T is the complete (hence classical) theory describing some subject matter. T is the ›target theory‹, the ›aim of inquiry‹ about that subject matter. The falsity of a theory such as U (or V) consists in its containing sentences not contained in T. Intuitively, the progress doctrine (cf. Oddie 1986) has it that U and V can be interestingly distinct, while yet $[U,V,T]$. Notoriously, no

formal characterisation of verisimilitude has yet vindicated the intuition (see Miller 1974 a; 1974 b; 1976; Tichy 1974; 1976). But as soon as one poses the problem formally, one sees various degrees of freedom in attacking it. Why should U, V and T be classical? Why not take them more generally as closed under the deducibility (or consequence) relations of various non-classical logics? Indeed, if one does this in a relevantist setting, one can even consider relaxing the requirement of consistency on U and V before entertaining statements of the form $[U, V, T]$. I would urge other relaxations than relevantist ones. As an anti-realist, or intuitionist, I claim to believe in the truth doctrine that "the aim of an inquiry, as an inquiry, is the truth of some matter" (cf. Oddie, 1986, ix). *Of course* the aim of an inquiry is to find out the truth! But who is to say that truth must be bivalent? and that the target theory must be complete? As an anti-realist I may also believe in the progress doctrine. Theories that have been refuted in the past have been superseded by apparently better theories (that is, ones closer to the ideal theory) which have themselves subsequently been refuted. Moreover, my intuitions as to which theories (or hypotheses or propositions) are advances on which others in this respect may largely coincide with those of a classicist such as Oddie. So we could start off with the same intuitive raw materials for our explication, but with very different philosophical presuppositions. I am also intrigued by the prospect that an adequate explication of verisimilitude might turn out to be possible *only when the theories involved are intuitionistic*. A theory is intuitionistic only if it consists only of warrantedly assertable sentences relative to some basis. Suppose it turns out that $[U, V, T]$ holds only when U, V and T are intuitionistic, but that, for some strange reason, the classical added extra of excluded middle that yields their classical closures so messes up matters that the explicated notion cannot coherently extend to those classical closures. What would the classicist make of this? Would allegiance to the progress doctrine force him seriously to re-consider his realism? — So much for verisimilitude. Now in theory dynamics, the basic problem is to explicate notions of contraction and revision of a theory T by a sentence S. If T contains S then the *contraction* of T with respect to S is a new theory $(T\text{-}S)$ ›as much like T as is possible‹ except insofar as it does not contain S. If T contains $\sim S$, then the *revision* of T

with respect to S is a new theory (T^*S) ›as much like T as is possible‹ except insofar as it ›replaces‹ $\sim S$ by S. It turns out that it is no simple matter to characterise contraction and revision functions (see Alchourrón/Makinson 1980; 1982; 1985; Alchourrón/Gärdenfors/Makinson 1985; Gärdenfors 1982; 1984; 1985 a; 1986; 1988; Makinson 1985). How does this connect with verisimilitude?

Suppose T is the true theory aimed at, and that U is a false theory containing a sentence S not in T. One would expect $[U\text{-}S, U, T]$ to hold. Suppose further that $\sim S$ is in T. One would expect $[U^*\sim S, U, T]$ to hold. That is, contractions and revisions are undertaken in order to *improve* one's theory: in order better to approximate the target theory. It would seem reasonable, then, to impose *simultaneous adequacy constraints* on sought explications of contraction, revision and verisimilitude, like the two just given above. One is bound to learn something about verisimilitude from the study of contraction and revision, and *vice versa*. — Verisimilitude and theory change connect also with considerations of paraconsistency. Suppose one is aiming at theory T, but one holds at present the faulty theory U (faulty in that the union T, U is inconsistent). Suppose the sentence S is in T but U, S is inconsistent. Now in a classical (indeed any non-paraconsistent) setting, the theory U, S would be trivial — the whole language. It would therefore be otiose to require that $U\text{-}S$ or $U^*\sim S$ be contained in U, S. But in a paraconsistent setting these requirements would be non-trivial. The deductive closures would now be with respect to a paraconsistent logic. The theory U, S would therefore not necessarily be the whole language. So the requirements just stated would, in effect, prevent inadvertent ›blow-up‹ of (contractions and) revisions. Paraconsistency provides a link with yet another area of foundational concern for at least a century — the logical and semantical paradoxes. The new transfinite inductive semantics of (Martin/Woodruff 1975; Kripke 1975; Herzberger 1982 a; Woodruff 1984; Gupta 1982; Yablo 1982; 1985) have thrown new *semantic* light on the phenomenon of paradox. But there is no corresponding syntactic grip on the notion, akin to that afforded by soundness and completeness results for ordinary validity. In (Tennant 1982) I proposed a proof-theoretic criterion of paradox, which amounts to an axiomatization of some notion that I conjecture to coincide with that specified by one of

the transfinite inductive semantic notions just mentioned. The criterion is this: to tell whether a set Δ of sentences in a semantically closed language is paradoxical, write down the proof of $\bot$ (absurdity) corresponding to the intuitive derivation of paradox using Δ. The proof will not be in normal form. Try to normalize it by means of the well-known reduction steps for the logical operators. One finds that the reduction sequence does not terminate, but enters into a loop after finitely many reduction steps. Thus paradoxes can be ›axiomatized‹ by enumerating proofs, applying reductions, and listing those (finite) Δ for which a proof of $\bot$ has a finitely looping reduction sequence. Conjecture: There is a transfinite inductive semantics such that Δ is thus paradoxical-on-the-basis-of-a-proof-the-oretically-identified-vicious-circle if and only if Δ is paradoxical according to the semantics.

List of logical symbols

$\rightarrow$	implication arrow
$\leftrightarrow$	two-way implication arrow
$\sim$	negation tilde ($\neg$ elsewhere)
$\vee$	'vel' for disjunction
$\&$	ampersand for conjunction ($\wedge$ elsewhere)
$\bot$	›bottom‹ symbol for falsity
$\emptyset$	empty set
$\in$	epsilon of membership
$\supset$	hook for material implication ($\rightarrow$ elsewhere)
$\Rightarrow$	double arrow for deducibility ($\vdash$ elsewhere)
$\forall$	universal quantifier ($\wedge$ elsewhere)
$\exists$	existential quantifier ($\vee$ elsewhere)
$\square$	modal operator for necessity

8. Selected references

Anderson/Belnap 1968, *Entailment*, Vol. 1.
This is the standard encyclopaedia for relevance and entailment logicians.

Boolos 1979, *The Unprovability of Consistency: An Essay in Modal Logic.*
This is an elegant presentation of the link between arithmetical self-reference and modal theoremhood discussed above.

Burgess 1979, Logic and time, in *Journal of Symbolic Logic* 44.
This is an excellent article covering recent developments in temporal logic.

Gabbay/Guenthner (eds.) 1986, *Handbook of Philosophical Logic, Volume III: Alternatives to Classical Logic.*
This survey volume contains essays on many-valued logic, relevance and entailment logic, intuitionistic logic, free logic and quantum logic, with good bibliographies.

Gärdenfors 1988, *Knowledge in Flux: Modelling the Dynamics of Epistemic States.*
This book consolidates a wealth of recent literature on theory dynamics.

Lewis, D. K. 1973, *Counterfactuals.*
This is the *locus classicus* for possible worlds semantics for counterfactuals.

Smiley/Shoesmith 1978, *Multiple Conclusion Logic.*
This book breaks impressive new ground in metalogic, and gives a new perspective on a great variety of logical systems, including systems of many-valued logic.

Tennant 1987 a, *Anti-Realism and Logic.*
This book gives a detailed account of *intuitionistic relevance logic*, and argues a philosophical case for its being the right logic.

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76. Mereology and set theory as competing methodological tools within philosophy of language

1. Introduction

Nothing would seem more natural than that the formal concept 'part' and its cognates should play a key role in the description and theory of language. At all levels of language we come across part-whole relations: the phoneme /p/ is part of the spoken word 'part' as the grapheme 'p' is part of the written word; the morpheme 'logy' is part of the word 'mereology' and the word 'Teil' is part of the sentence 'Der Teil ist kleiner als das Ganze'. The meaning ›male‹ is a common part of the meanings of 'roi', 'Vater', 'toro' and 'him'.

This article is divided into several parts, and so on. The Latin word for a basic part, 'elementum', probably derives from the letters LMN, and early philosophers like Plato (s. art. 14) and Aristotle (s. art. 15) frequently employ linguistic examples as paradigms of part-whole relations. Yet the concepts of mereology (part-whole theory) today find almost no systematic use among philosophers of language, whereas the concepts of set theory, which *prima facie* appear more remote from linguistic application, are widely used. This article considers why this is so and whether it should be so. To anticipate the conclusion: it is so mainly, though not exclusively, for historical reasons, not all of which still hold good, and it is an unbalanced state of affairs: mereology can and should be used in some places where set theory is now used, and we shall consider places where such use is or may be appropriate. So this paper is less a report on an existing controversy, since the supremacy of set theory is virtually unchallenged, than itself a challenge to the extent of this supremacy. The polemic is however muted, both because there are areas where mereology cannot and would not aspire to replace set theory, and because there are signs in recent literature that some concepts of mereology are finding use in conjunction with set-theoretical methods, which indicates that the ideal of peaceful cooperation is attainable, an ideal better promoted by gentle reminders than fierce accusations.

2. Historical

2.1. The divergence of mereology and set theory

Our controversy could not have arisen until this century, because the conceptual distinctions necessary to distinguish set theory and mereology did not emerge until the late nineteenth century. Until then, the subsumption relation among classes was regarded, not without reason, as just one part-whole relation among others. The divergence came about because mathematicians, above all Georg Cantor, developed set theory from a simple algebra of classes into a much more powerful theory capable of encompassing most of mathematics. The difference can best be explained using the idea of types, although most set theories are type-free. If we envisage sets of individuals (urelements) as being of type one higher than individuals, sets of sets

of individuals as being two types higher, and so on, we see that set formation may proceed up the types to infinity. In a type-free set theory with the axiom of foundation, the notion of type may be replaced by that of the rank of a set. In mereology, however, the wholes formed by the summing together of individuals are themselves individuals, and we do not thus get a type or rank hierarchy. Just this explains the appeal of mereology to those of nominalist persuasion, who use it with an eye to ontological economy. Two of the most notable mereologists, Stanisław Leśniewski and Nelson Goodman, developed their mereologies expressly as nominalistically acceptable alternatives to set theory (Leśniewski 1916, 1 ff; Leonard/Goodman 1940, 45 ff; Goodman 1977, 33 ff). Consequently, mereology is not only ontologically more modest and ideologically less powerful than set theory; it is widely associated with extreme nominalism and extensionalism in logic and ontology, and is thus thought not to be of use to philosophers not sharing these predilections. This historical perspective is partly incorrect, as there have been mereologists, such as Edmund Husserl, Alfred North Whitehead, and Roderick M. Chisholm, who do not fit the standard picture. More importantly, Platonism and intensionalism are consistent with the use of mereology.

2.2. History of mereology

There is no connected account of the history of uses of 'part' and related words, though they found discussion both in Aristotle and the Scholastics. Late nineteenth century discussion derives from two sources: positively, the investigation of partial contents among psychologists such as Carl Stumpf (for an account of this tradition see Smith/Mulligan 1982, 15 ff), negatively, the expulsion of mereology from the garden of logic by Gottlob Frege (s. art. 34) in his critical review of Ernst Schröder's algebra of logic (Frege 1967, 193 ff). Stumpf's work on partial contents was refined and generalized by Husserl, who envisaged and sketched a few results of a theory of parts and wholes (Husserl 1984, 227 ff). Husserl's theory lacked the nominalistic motivation of later theories and was developed by him in conjunction with the theory of ontological dependence. Not only did Husserl immediately apply his theory to language (Husserl 1984, 301 ff), but his views also influenced Prague structuralists, especially Roman Jakobson (Jakobson 1973 a, 13 ff;