Notes on 'the contemporary conception of logic'

Peter Smith

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1 The 'contemporary conception'?

Warren Goldfarb, in his paper 'Frege's conception of logic' in *The Cambridge Companion to Frege* (2010), announces that his 'first task is that of delineating the differences between Frege's conception of logic and the contemporary one'. And it is not a new idea that there are important contrasts to be drawn between Frege's approach and some modern views of logic. But one thing that immediately catches the eye in Goldfarb's prospectus is his reference to *the* contemporary conception of logic. And that should surely give us some pause, even before reading on.

So how does Goldfarb characterize this contemporary conception? It holds, supposedly, that

> the subject matter of logic consists of logical properties of sentences and logical relations among sentences. Sentences have such properties and bear such relations to each other by dint of their having the logical forms they do. Hence, logical properties and relations are defined by way of the logical forms; logic deals with what is common to and can be abstracted from different sentences. Logical forms are not mysterious quasi-entities, à la Russell. Rather, they are simply schemata: representations of the composition of the sentences, constructed from the logical signs (quantifiers and truth-functional connectives, in the standard case) using schematic letters of various sorts (predicate, sentence, and function letters). Schemata do not state anything and so are neither true nor false, but they can be interpreted: a universe of discourse is assigned to the quantifiers, predicate letters are replaced by predicates or assigned extensions (of the appropriate arities) over the universe, sentence letters can be replaced by sentences or assigned truth-values. Under interpretation, a schema will receive a truth-value. We may then define: a schema is *valid* if and only if it is true under every interpretation; one schema *implies* another, that is, the second schema is a *logical consequence* of the

first, if and only if every interpretation that makes the first true also makes the second true. A more general notion of logical consequence, between sets of schemata and a schema, may be defined similarly. Finally, we may arrive at the logical properties or relations between sentences thus: a sentence is logically true if and only if it can be schematized by a schema that is valid; one sentence implies another if they can be schematized by schemata the first of which implies the second. (pp. 64–65)

Note an initial oddity here (taking up a theme that Timothy Smiley has remarked on in another context). It is said that a 'logical form' just *is* a schema. What is it then for a sentence to *have* a logical form? Presumably it is for the sentence to be an instance of the schema. But the sentence 'Either grass is green or grass is not green' – at least once we pre-process it as 'Grass is green $\lor$ $\neg$ grass is green' – is an instance of both the schema $P \lor \neg P$ and the schema $Q \lor \neg Q$. These are two different schemata (if we indeed think of schemata, as Goldfarb describes them, as expressions 'constructed from logical signs ... using schematic letters'): but surely no contemporary logician would want to say that the given sentence, for this reason at any rate, has two different logical forms. So something is amiss.

But let's hang fire on this point for the moment. Let's ask: is Goldfarb right that modern logic proceeds by defining notions like validity as applying in the first instance to schemata?

Some other writers on the history of logic take the same line about modern logic. Here, for example, is David Bostock, in his *Russell's Logical Atomism* (2012), seeking to describe what *he* supposes is the 'nowadays usual' understanding of elementary logic, again in order to contrast it with the view of one of the founding fathers:

> In logic as it is now conceived we are concerned with what follows from what *formally*, where this is understood in terms of the formal language just introduced, i.e. one which uses 'P', 'Q', ... as schematic letters for any proposition, 'a', 'b', ... as schematic letters for any reference to a singular subject, and 'F', 'G', ... as schematic letters for any predicate. So we first explain validity for such schemata. An *interpretation* for the language assigns some particular propositions, subjects or predicates to the schematic letters involved. It also assigns some *domain* for the quantifiers to range over .... Then a single schematic formula counts as *valid* if it always comes out true, however its schematic letters are interpreted, and whatever the domain of quantification is taken to be. A series of such formulae representing an argument ... counts as a *valid sequent* if in all interpretations it is truth-preserving, i.e. if all the interpretations which make all the premises true also make the conclusions true. ...
>
> We now add that an actual proposition counts as 'formally valid' if and only if it has a valid form, i.e. is an instance of some schematic formula that is valid. Similarly, an an actual argument is 'formally valid' if and it only if it has a valid form, i.e. is an instance of some schematic sequent that is valid. Rather than 'formally valid' it would be more accurate to say 'valid just in virtue of the truth functors and first-level quantifiers it contains'. This begs no question about what is to count as the 'logical form' of a proposition or an argument, but it does indicate just which 'forms' are considered in elementary logic.
>
> Finally, the task of logic as nowadays conceived is the task of finding explicit rules of inference which allow one to discover which formulae (or sequents) are the valid ones. ... What is required is just a set of rules which

> is both 'sound' and 'complete', in the sense (i) that the rules prove *only* formulae (or sequents) that are valid, and (ii) that they can prove all such formulae (or sequents). (pp. 8–10)

Bostock here evidently takes very much the same line as Goldfarb, except that he avoids the unhappy outright identification of logical forms with schemata. And he goes on to say that not only do we define *semantic* notions like validity in the first place for schemata but *proof-systems* too deal in schemata – i.e. are in the business of deriving schematic formulae (or sequents) from other schematic formulae (or sequents).

It isn't difficult to guess a major influence on Goldfarb. His one-time colleague W.V.O. Quine's *Methods of Logic* was first published in 1950, and in that book – much used at least by philosophers – logical notions like consistency, validity and implication are indeed defined in the first instance for schemata. Goldfarb himself takes the same line in his own later book *Deductive Logic* (2003). Bostock's book *Intermediate Logic* is perhaps a little more nuanced, but again takes basically the same line.

But the obvious question is: are Goldfarb and Bostock right that the conception of logic they describe, and which they themselves subscribe to in their respective books, is widely prevalent?

Philosophers being a professionally contentious lot, we wouldn't usually predict happy consensus about anything much! If we are going to find something like a shared 'party line', a shared modern conception, it is rather more likely to be held by the mathematical logicians, who might be disposed to speed past foundational niceties en route to the more interesting stuff. At any rate, what I propose to do here is to concentrate on the latter constituency. Let's take some well-regarded mathematical logic textbooks from the modern era, i.e. from the last fifty years – which, I agree, is construing 'contemporary' rather generously (but not, as we'll see, to Goldfarb and Bostock's disadvantage). And let's briefly consider e.g. how the various authors regard formal languages, what they take logical relations to hold between, how they regard the letters which appear in logical formulas, what accounts they give of logical laws and logical consequence, and how they regard formal proofs. To be sure, we might expect to find recurrent themes running through the different modern treatments (after all, there is only a limited number of options). But will we find enough commonality to make it appropriate to talk of 'the' contemporary conception of logic among working logicians, or to talk of how logic is 'nowadays conceived'?

Of course, I hope it will be agreed that this question is interesting enough in its own right: to be frank, I'm really just using Goldfarb and Bostock's remarks as a provocation to spur us on to doing the required trawl through the literature.

We'll take things chronologically, and begin by looking at three undisputed 'modern' classics from which generations of logicians have learnt.

2 Elliot Mendelson, *Introduction to Mathematical Logic* (1964)

We start with what would surely be widely regarded as the first modern textbook in mathematical logic.

(a) Mendelson opens his Chapter 1 by defining truth-functional negation, conjunction, inclusive disjunction and the material conditional and by introducing signs for these. Initially, these signs seem to be added to natural language for clarificatory purposes. He continues:

> Any sentence [of logicians' English] built up using these connectives has a truth-value which depends on the truth values of the constituent sentences. In order to make this dependence apparent, let us apply the name *statement form* to an expression built up from the *sentence letters* A, B, C, etc., by applications and reapplications of the propositional connectives. ...

So an expression like '$(((\sim P) \vee Q) \supset R)$', to use Mendelson's bracketing conventions, is what he calls a *statement form* (and lives at the level of metalogical enquiry).

Now, a statement form like this 'determines a truth-function', because each assignment of truth-values to the sentence letters yields a truth-value for the whole form computed in the obvious way. But note that *statement forms aren't themselves statements* (or sentences) – for sentence letters aren't sentences for Mendelson, but rather they are schematic letters or place holders which can be substituted for by genuine sentences (from a natural language like English, or mathematicians' English, or even from a formalized language though he doesn't say a lot about this third option). Mendelson's statement forms are therefore Goldfarb and Bostock's schemata. He speaks of them as 'indicating' or 'making apparent' the logical structure of sentences.

Mendelson goes on to define a *tautology* to be a kind of statement form, not a kind of statement: i.e., a tautology is a statement form which defines a truth-function which always yields the value true for any values as arguments. So, Mendelson's definition here is a variant of Goldfarb's second alternative. (Recall, Goldfarb said, disjunctively, 'sentence letters can be replaced by *sentences* or *assigned truth-values*'. Being a tautology for Mendelson is *not* defined in terms of replacement of sentence letters by *sentences* and then evaluation of the sentences: rather the story proceeds by *values* being assigned directly to sentence letters.)

Note that tautologies, being schemata, are again not themselves statements, so are not themselves the sort of thing that can be true or false, let alone can be logically true. However, an *instance* of a tautology got by replacing sentence letters by genuine sentences (same letter getting replaced by the same sentence of course) is indeed defined to be *logically true*, and 'such a sentence may be said to be true by virtue of its truth-functional structure alone'. Such a logically true sentence won't be a sentence of plain English, but will be a sentence of e.g. logician's English augmented with the signs for the truth-functional connectives, But it would be natural to add that a plain English sentence counts as logically true if it is equivalent to a logically true sentence of the augmented language.

Mendelson goes on to say that $\mathscr{B}$ is a *logical consequence* of $\mathscr{A}$ if $(\mathscr{A} \supset \mathscr{B})$ is a tautology, where his script letters stand in for statement forms, of course. (So note we have *two* levels of schemata here: a sentence letter like A is schematic for a genuine sentence, a letter like $\mathscr{A}$ is schematic for a statement form). And just as being a tautology is a property of a statement form, logical consequence is a relation between statement forms and not statements. Mendelson would say that an argument where the conjunction of the premisses and the conclusion respectively instantiate some $\mathscr{A}$ and $\mathscr{B}$, where $\mathscr{B}$ is a logical consequence (in his sense) of $\mathscr{A}$, is *logically correct*.

Now, all this is put in place in Mendelson (along with e.g. a proof of that every truth-function is generated by some statement-form involving at most the connectives $\sim$, $\wedge$, and $\vee$) over the first 17 pages of Chapter 1, *before* we get any talk at all of wffs, axioms or theorems of a formal theory. In other words, for him, talk of statement-forms, tautologies and logical consequence belongs, thus far, to an informal theory couched in mathematical English, a theory which can be applied to informal statements and inferences in some language augmented with explicitly truth-functional connectives.

However, in his §4, Mendelson moves from what has been introduced as the *infor-*

mal metatheory of a class of informal inferences to introduce a *formal* theory involving statement-forms. First he defines a class of wffs which coincide syntactically with the statement forms in $\sim$ and $\supset$ which we've previously added to informal mathematical English, but which are now treated as the sole wffs of a special formal syntax. Then we are introduced to some formal 'proof' apparatus – i.e. a specification of a class of wffs as axioms, a designated 'rule of inference', a definition of a 'proof' as a suitable sequence of wffs in the system, and of a theorem as the final wff in a 'proof'. It is then shown (in fact by Kalmàr's method) that the theorems of the formal 'proof' system are just those wffs, i.e. those statement forms in $\sim$ and $\supset$, which are tautologies.

Now, I've put 'proof' here in scare-quotes for the moment as a reminder that a formal 'proof' in Mendelson's system is a sequence of *statement-forms*. And sequence of wffs-qua-statement-forms which don't say anything isn't the sort of thing which features in mathematical proofs as ordinarily understood. Since the role of 'proofs' is to generate new statement-forms which are tautologies, and is entirely metalogical machinery, it is no criticism of the 'proof'-system that it doesn't reflect or regiment a mathematician's natural modes of reasoning with propositional connectives: the system's ambitions are not those of a Natural Deduction system.

Still, although Mendelsonian 'proofs' are not derivations of truths from truths, we *can* find genuine proofs which correspond (many-one) to 'proofs' by taking a well-constructed proof-sequence of statement-forms, and uniformly substituting statements for the sentence letters. Then we'll get a corresponding sequence which moves from logical truths, by logically correct reasoning, to establish more logical truths.

(b) In Chapter 2, Mendelson turns to quantificational logic. Now, if he were going to take the same approach as in Chapter 1, you might expect the discussion to start out something like this:

1. In clarifying mathematical English, as well as regimenting the truth-functional connectives, it is extremely useful to regiment expressions of generality by using prenex quantificational expressions of the kind 'all x are such that', 'there is some y such that' (handily abbreviated by '$\forall x$', '$\exists y$' or some such).

2. Just as we expose the truth-functional structure of sentences which contains the truth-functional connectives by replacing constituent sentences by sentence letters to give us (propositional) statement forms, so we can expose the quantificational-cum-truth-functional structure of sentences by replacing constituent constants and predicates by letters to give us (quantificational) statement forms. [To keep things simple, we'll ignore function expressions for the present purposes.]

3. Then, just as a (propositional) statement form defines a truth-function in the obvious way, a (quantificational) statement form also defines a function – i.e. a function *from* assignments of a semantic value to the quantifiers [a domain], of values to constant-letters [objects in the domain] and values to predicate-letters [extensions] *to* truth-values.

4. And again, just as a tautology is a (propositional) statement form which defines a truth-function that always takes the value true for all truth-values as arguments, so a quantificational validity [to coin a phrase] is a (quantificational) statement form which defines a function that always takes the value true for all assignments of semantic values. We can likewise extend the concept of logical consequence.

5. And just as instance of a tautology got by replacing sentence letters by sentences is logically true, and 'may be said to be true by virtue of its truth-functional

structure alone', an instance of a quantificational validity got by replacing constant-letters and predicate-letters by constants and predicates (and fixing a domain of quantification) is again logically true, and may be said to be true by virtue of its quantification and truth-functional structure alone.

And then, with these preliminaries in place, the next key task (corresponding to developing a 'proof' system for propositional schemata) will be to design a formal 'proof' system for wffs which are quantificational statement forms in $\forall$, $\sim$, $\supset$, a system which enables us 'prove' all and only the quantificationally valid wffs.

However, this isn't *quite* what happens. Steps (1) to (2) are certainly there, albeit done very quickly. But, instead of developing an informal notion of quantificational statement forms on a par with the propositional statement forms of Ch. 1, Mendelson goes straight to talking about formal wffs instead (and there are now also formal wffs which do not correspond to statement forms, i.e. those with free variables, which will come into their own in Mendelson's style of proof system). Still, it could reasonably be said that skipping straight to talk of formal wffs makes for welcome brevity, without essentially changing the story. And certainly, as in Ch. 1, the wffs of his formal system continue to officially serve as *schemata* whose instances are sentences of appropriate (augmented) natural languages, or perhaps of more formalized languages. Thus, at p. 54, Mendelson explicitly says that a sentence of either kind of language 'which is an instance of a logically valid wff is logically true (according to quantification theory)': wffs are still schemata, the sort of the schemata which have sentences as instances.

There are presentational wobbles, though, which suggest that Mendelson's view of his logical system as dealing in schemata isn't always held in absolutely steady focus. For example, on p. 49 he asks us to 'translate' some sentences into wffs. But schematizing isn't translating. And if $\forall x(Fx \supset Gx)$ translates all men are mortal, it must be because F and G mean respectively *is a man* and *is mortal*, so they are no longer schematic letters which lack a meaning, but part of an interpreted formalized language. Later, on p. 55, we are instructed: 'Introducing appropriate abbreviations, write the sentences of the following arguments as wffs . . . ', which again involves treating predicate letters not as schematic but as part of an interpreted language. This sort of talk suggests a different view where (closed) wffs belong to interpreted formalized languages, i.e. are *not* schemata but are true or false sentences, and quantificational validity would now be defined by saying that a wff is valid if it remains true however the domain and the semantic values of its constant letters and predicate letters are changed. (We'll soon be meeting a more consistent advocate of this approach.)

Still, I think it fair to say that those remarks of Mendelson's are careless talk: charitably, his more considered official view of the status of wffs is that they are schemata.

(c) I have described Mendelson's account of logic in some detail just because it does seem to fit Goldfarb and Bostock's description so very well. At the propositional level, tautologies are a kind of schema; logical consequence is defined as holding between schemata; Mendelson's formal theory is a theory for deriving schemata. Likewise, charitably read, for his treatment of quantificational logic. Moreover Mendelson avoids the unnecessary trouble that Goldfarb gets himself into when he talks of logical form: Mendelson too talks of logical structure, but *he* supposes that this is *made apparent* by using a statement form (i.e. schema), not that it is to be *identified* with some statement form.

So far then, so good for Goldfarb and Bostock: score 1/1. Still, one swallow doesn't make a summer. How do other the authors of our other two classic texts proceed?

3 Stephen C. Kleene, *Mathematical Logic* (1967)

This is 'Little Kleene', not to be confused with Kleene's epochal *Introduction to Metamathematics* (1952), but equally careful and lucid, conveying I think the same picture of logic as the earlier text, and like Mendelson's book a very widely read textbook in its time.

(a) Kleene from the outset is very careful and explicit in distinguishing (i) the *object language* under consideration at a given point, so-called 'because this language (including its logic) is an object of our study' from (ii) what he prefers to call the *observer's language* (as he thinks 'metalanguage' can have some unfortunate connotations). And for him, an object language is indeed a genuine *language* in which propositions are expressed: so it might e.g. be plain English (or some tidied-up fragment thereof), with sentences like 'John loves Jane', or perhaps it might be mathematicians' English with sentences like '$3 < 5$'. However,

> We save time, and retain flexibility for the applications, by not now describing any particular object language. ... Throughout this chapter, we shall simply assume that we are dealing with one or another object language.

But note: being silent about which particular object language is in question in a particular discussion doesn't mean that we have changed the subject and are talking e.g. about some uninterpreted syntax instead. (Warning: Kleene cheerfully talks about sentences as 'formulas' even when from a natural language – so, as we might put it, formula-talk isn't for him isn't talk about *mere* formulas.)

Focus first on propositional logic. We are going to be interested in object languages which are able to express, unambiguously, the operations of (truth-functional) negation, conjunction, inclusive disjunction and material implication. Perhaps this is because we are dealing with what Kleene calls 'a suitably restricted and regulated part of a [natural] language', or perhaps this is because the object language already has symbols for these truth-functions (think of mathematical English). Then, in talking (as 'observers') about sentences in such an object language, we'll use 'P', 'Q', 'R', 'P_1', 'Q_1', ... to represent distinct 'prime formulas' or atoms of the object language (i.e. sentences whose internal structure we will ignore for the moment). And we will use the symbols '$\sim$', '&', '$\vee$' and '$\supset$' to indicate whatever expressions in the object language express the familiar truth-functions. So: '$P \supset P$' might be our 'observer's language' expression for talking about the object-language sentence 'if Jack loves Jill, then Jack loves Jill', or (in another context) for talking about '$3 < 5 \to 3 < 5$'.

Now, atoms of the given object language in play are assumed to be either true or false (but not both). So, in a particular context, P may be true, and Q false. Note though that for Kleene (contrasting with Mendelson) this isn't a matter of an arbitrary assignment of values to schematic letters: the 'P's and 'Q's we've just used metalogically will get assigned a value which depends on 'the constitution of the atoms' which they represent in the context and on the facts 'to which they [the atoms] allude'. But now, whatever value that P in fact takes, $P \supset P$ (i.e., as it might be 'if Jack loves Jill, then Jack loves Jill') comes out as true. And such composite sentences which are 'always true, regardless of the truth or falsity of the truth or falsity of their prime components' are said to be *valid* or to be *tautologies*.

So, to emphasize, a tautology for Kleene is an *object-language* sentence which is always true (whatever the value of its propositional atoms). It isn't an expression like '$P \supset P$' that is a tautology but what the object language sentence it represents, given a context which fixes what the atoms represent. So $P \supset P$ might be the tautology 'if Jack loves Jill, then Jack loves Jill': in another context, $Q \supset P \vee Q$ might be the tautology '$3 < 5 \rightarrow 3 = 5 \vee 3 < 5$').

But of course, such claims generalize, and we use schemata to aid us making the generalizations. Thus, for example,

> For any choice of formulas (built up from P, Q, $R \ldots$) as the A and B, the resulting formula $B \supset A \vee B$ is valid. (p. 15)

But recall that 'formulas' for Kleene means object-language sentences, and note that Kleene does *not* describe the schema '$B \supset A \vee B$' itself as valid (or as a tautology). He is very clear that it is the object-language instances of the schema which are the valid sentences. Kleene's notion of valid consequence matches: it is a relation among object-language sentences.

When Kleene turns from model theory (his word) to a deductive system for the propositional calculus, he again uses schemata in presenting his axiomatic system (since he wants whole families of equiform axioms, rather than individual axioms plus a rule of substitution). He even outlines some proofs using a sequence of schemata. But, contrary to Mendelson's line, Kleene is clear that such a sequence *isn't* itself a proof, just a template for one. The real proofs in the described system are sequences of formulas – i.e. sentences – in the object-language (whatever object-language is in play at the time). True, he describes these as formal proofs – but for Kleene this doesn't mean strings of schemata:

> We said 'formal proof" and 'formal deduction' in the definitions ... to emphasize that these proofs and deductions are in the object language, which we are studying in the observer's language. From the standpoint of the observer's language, we look only at the form of the formulas (in contrast to their meaning or content) in determining under the definitions just given whether a given sequence of formulas $B_1, \ldots, B_i$ is in fact a (formal) proof, or a (formal) deduction from given assumption formulas $A_1, \ldots, A_m$. A sequence of formulas $B_1, \ldots, B_i$ is a (formal) proof, or a (formal) deduction from $A_1, \ldots, A_m$ only when it exactly fits the above definition .... This stereotyping of the operations that can be performed in constructing a formal proof or deduction makes the formal proofs and deductions (in the object language) definite enough in structure to serve as objects of our study. (p. 36)

So the point is that we need only look at the syntactic form of the sequence of object-language formulas (sentences) to determine whether it counts a proof in Kleene's system: but the sentences are not thereby divested of meaning or turned into schemata.

What does Kleene thinks his proof system is *for*? We remarked that Mendelson's 'proof' system is just metalogical apparatus for generating tautologous schemata, and it is no cause for complaint that *his* system doesn't reflect a mathematician's natural deductions in her object language. Kleene, by contrast, *is* describing genuine proofs to be carried out in the object-language – yet he too evidently isn't aiming, in the first instance, to describe the patterns in the inference-moves which mathematicians do actually carry out. Rather, he is describing proofs that they *could* carry out, if they explicitly axiomatize their logic – proofs which *would* warrant their ordinary mathematical deductive moves (and Kleene then shows, metatheoretically, that what we would now think of as various natural deduction rules are indeed derived rules of his system).

Many contemporary logicians would of course regard Kleene's axiomatic approach to the idea of logical derivation as misguidedly indirect. Logic, they would say, is fundamentally about what follows from what, so we really ought to take the idea of necessarily truth-preserving rules of inference as basic (and the logical truths are what come for free because they are deducible from no assumptions). Hence a direct approach to a theory of deduction ought to start by regimenting the basic rules of inference (and then treat the logical truths as spin-offs, generated by the rules) rather than go the other way about by first axiomatizing the logical truths (and then showing that certain derived rules of inference are necessarily truth-preserving). We will return to this theme.

Still, the point remains that for Kleene the notions of a tautology and of a logically valid inference for propositional reasoning are both defined for object-language sentences and deductions. Turning to quantification theory, the story remains the same. Kleene begins by talking about how we can regiment subject/predicate sentences of ordinary or mathematical language using informal quantifiers and associated pronouns/variables. As logical observers, we can adopt the notation of the predicate calculus in describing such regimented sentences, putting single letters for names and predicates. But thereafter, we can again 'remain silent here about what exactly the underlying object language is, both because we do not wish to be drawn into details about it now, and because we wish to leave the way open to various applications.'

Things then go exactly as you'd expect from Kleene's treatment of propositional logic. Sections on 'model theory' introduce the standard semantical analysis of a suitably regimented object-language, and validity for object-language sentences is directly defined a matter of truth for any choice of domain and any valuation of names and predicates. Then the proof-theory for the propositional calculus is extended to deal with arguments – object-language arguments – involving quantified sentences. We needn't go into further details.

(b) As with Mendelson, there are occasional wobbles where Kleene seems to not mind his Ps and Qs. For example, he writes (p. 59)

> Consider the following argument in words. Letters are suggested in brackets to symbolize prime components of the composite sentences. "I will pay them for fixing our TV [P] only if it works [W]. But our TV still doesn't work. Therefore I won't pay them." This argument can be symbolized thus:
>
> 1. $P \supset W, \neg W \therefore \neg P$.
>
> To say that this is correct reasoning ... should mean that whenever $P \supset W$ and $\neg W$ are both true, then $\neg P$ is also true. This is what we expressed exactly in §7 ... in symbols by
>
> 2. $P \supset W, \neg W \vDash \neg P$.

But the inference marker '$\therefore$' (therefore) should occur between *assertions*, while '$\vDash$' belongs to the metalanguage, expresses the relation of logical entailment, and should occur between *denoting terms*. In fact, as we've seen, Kleene's official line is that e.g. '$P \supset W$' is a term for talking about, in this instance, the object-language claim 'I will pay them for fixing our TV only if it works'. So (2) is fine but, strictly speaking, (1) is ill-formed, for *that* would need '$P \supset W$' to be a sentence of the observer's language. And earlier on the same page, Kleene talks of *translating* arguments of ordinary language into the symbolism of the propositional calculus in order to assess them, which again would require the likes of '$P \supset W$' to be meaningful *sentences* rather than apparatus for talking about sentences. But, as with Mendelson, I'm inclined to diagnose a bit of careless talk (and in any case, the apparent wobbles here don't really matter for our present concerns).

Kleene does just occasionally talk of 'form', e.g. when he says that $P \ \& \ \neg P \supset Q$ is of the form $A \supset B$. But there's *no* suggestion at all that we should 'first explain validity for ... schemata' (as Bostock puts it), or that 'logical properties and relations are defined by way of logical forms' (as Goldfarb puts it). Truth-functional validity, and then later being 'valid (in the predicate calculus)', are for Kleene defined directly as properties of certain classes of object-language sentences, without any detour through forms. And unlike for Mendelson and Goldfarb, validity – applying to contentful sentences not schemata – implies truth. Likewise for the validity of arguments.

Again it has been worth spelling out Kleene's approach in some detail. To be sure, expressions like '$P \supset Q$' (normally) stand for him at one remove from contentful object-language sentences, so there is a possibility of confusing them with schemata. But they are in fact contextually determined designators. And Kleene's account of logical notions does *not* chime with Golfarb and Bostock's 'contemporary conception' of logic.

4 Joseph R. Shoenfield, *Mathematical Logic* (1967)

Like Mendelson and Kleene, Shoenfield starts out fairly slowly, although his classic book eventually travels further, in particular by giving the first textbook presentation of forcing.

Overall the approach here to basic logic is like Kleene's. Shoenfield again directly defines notions like validity at the object-language level. However, he does focus on a more restricted class of object-languages. In fact, Shoenfield's object-languages are all *fully formalized first-order languages*, whereas Kleene casts his net rather wider to include any sufficiently disciplined portion of a language.

Let's dwell on this point of difference in just a bit more detail. Shoenfield assumes that we are always dealing with object languages whose primitive logical apparatus are the connectives $\neg$ and $\vee$, the quantifier $\exists$ and the identity predicate $=$, and his logical theory is developed to apply to arguments in such languages. One benefit is that when we want to talk about such arguments metalinguistically, e.g. in logician's English, we can just adopt the policy of re-using the expressions from the object language, with the metalinguistic uses counting as names for their object-language counterparts, and yet we still get a tidy metatheory. The upshot is that, if Shoenfield were to use e.g. '$P \vee \neg P$' in what Kleene would call the observer's language to metalinguistically denote an expression, it would be denoting the following object-language expression: $P \vee \neg P$. Whereas for Kleene, that same metalinguistic expression might denote 'Jack loves Jill or it isn't the case that Jack loves Jill' or it might denote '$3 < 5 \vee 3 \not< 5$', or whatever, depending on context.

So, Shoenfield quietly takes it that a mathematician's sentences and arguments have *already* been interpretatively pre-processed and regimented into a canonical first-order language, *before* the business of logical assessment starts. And then, when we logically step back from these recast mathematical arguments to comment metalogically, there's no work to be done at least in forming expressions to refer to object-language expressions in a way that makes theorizing about them easy – we just reuse the expressions as metalinguistic names for themselves. For Kleene, however, the vernacular arguments, if not taken exactly raw, are rather more modestly tidied up. It is when we step back to the observer's stance that we do the next stage of regimentation and get to refer to an object-language sentence as $P \vee \neg P$ or as $\forall x(Fx \supset Gx)$, say, thereby using a perspicuous representation which shows the relevant structure of the object-language sentence.

Shoenfield's and Kleene's stories about just how metalogical apparatus gets to apply to informal reasoning are therefore somewhat different. But on the most basic thing they are in agreement. For like Kleene, Shoenfield gives definitions of validity and consequence which apply directly to object-language sentences. The account *uses* metalinguistic schemata of course in giving the definitions: but what the account tells us what it is for sentences in a first-order object-language to be valid without *mentioning* schemata.

Likewise, a formal proof is a sequence of non-schematic object-language sentences. Like Kleene, Shoenfield is rather quick when it comes to telling us what an object-language proof-system is *for*: again, the game seems to be one of axiomatizing a pre-existing practice. But why choose this kind of regimentation rather than a natural deduction system? Shoenfield's given proof-system for first-order logic with identity is in fact decidedly idiosyncratic, with just three families of axioms (given by schemata) and five inference rules; and he doesn't pause for a moment to defend its merits as against more familiar systems. Still, that's beside the present point: just as with the notion of a valid consequence, the notion of a deductive proof is defined for object-language arguments in first-order languages without going via schemata.

With Shoenfield, then, what you see is what you get. Writing down a series of logical expressions isn't (as with Kleene) something done at one remove from some object-language derivation: it will be the real deal. And validity etc. is defined directly for this real arguments. His approach – in some respects the most natural of our three authors so far – again doesn't accord with the 'contemporary conception' which would have it that logical properties are to be defined in terms of properties of schemata.

5 From the 1970s: Enderton, Bell & Machover, Manin

(a) One clue, then, to the overall approach of various logic texts is to see which way they gloss the claim (made in our observer's language, as Kleene would put it) that $P \supset P$ is a tautology.

1. It says that '$P \supset P$' (a *meta*language schema) defines a truth-function which always takes the value true.

2. It says that 'If Jack loves Jill then Jack loves Jill' is true whatever the truth-value of 'Jack loves Jill'. [That's in a context where 'P' represents the object-language sentence 'Jack loves Jill'. Mutatis mutandis for other contexts. It *could* be, in a very special context, that the object-language sentence being represented itself belongs to a formalized language and looks like '$P \supset P$'.]

3. It says that '$P \supset P$' (an *object*-language sentence from a formalized language, where P expresses some proposition) is true whatever the truth-value of 'P'.

As we've seen, Mendelson adopts the first reading, Kleene goes context-dependent in the second way, while Shoenfield with his emphasis on already-formalized languages as the object of logical enquiry (with the same sentences being used metalinguistically to refer their object-language counterparts) could naturally take the third as the primary usage.

So what happens in other texts? Let's next take a much snappier look at three famous books from the 1970s, with a nice geographical spread of authors, concentrating (in this section) on how they think of metalinguistic symbols and – much more importantly – on their approach to defining notions like validity.

(b) Herbert B. Enderton's *A Mathematical Introduction to Logic* (1972) is an undisputed classic of exposition – still occasionally hard going, but much appreciated by generations of students as rather more friendly than Mendelson.

The approach here is slightly different from both Kleene's and Shoenfield's, but in the same general ballpark (and very different from Mendelson's approach with his emphasis on schemata as the primary bearers of logical properties). Let's go straight to predicate logic. Then Enderton assumes like Shoenfield that the object languages the logician is dealing with are already formalized first-order languages (perhaps the language of arithmetic or the language of set theory, or perhaps what he nicely calls an 'ad hoc' language (p. 71) that we use use to translate more mundane propositions like 'some apples are bad'). But unlike Shoenfield, Enderton doesn't assume that the symbols we use in our logic text, when talking about the arguments in these object languages, are the same as in the object language itself. Thus 'The conditional symbol may or may not have the geometric property of being shaped like an arrow, though its name [what we use in our logician's metalanguage] '$\to$' does' (p. 18). So here Enderton is taking a line about the metalogical notation more like Kleene's.

But the point of agreement between Enderton, Kleene and Shoenfield is the crucial thing. Enderton too defines notions like validity directly for his object languages (*using* schemata, of course, for generalizing purposes, but the bearers of the logical properties are the contentful sentences of the object language in question).

(c) Just a bit later, in the UK, J.L. Bell and M. Machover are giving lectures in London that get written up as *A Course in Mathematical Logic* (1977). They take a similar line to Enderton. What is under investigation are arguments already in formalized languages. But again, we are supposed not to care very much about displaying the languages in their native form:

> Thus, when we say, e.g. "$\mathscr{L}$ has the implication symbol $\to$", the arrow-shaped sign used here is to be regarded as a syntactic constant denoting a certain symbol (also called "implication symbol") of the object language $\mathscr{L}$. What the latter symbol actually looks like is of no importance; and the reader may give free rein to his imagination. (p. 8)

Bell and Machover again give direct accounts of validity and logical consequence for sentences in a first-order object-language $\mathscr{L}$ (whatever that looks like) – direct in the sense that they don't go via schemata. In fact they never talk at all about schemata or statement forms. (Their only talk about 'form' is in anodyne contexts as when we say, e.g., that a wff has the form $\forall x\varphi$.)

(d) The same year, Yuri Manin published the first edition of *A Course in Mathematical Logic for Mathematicians*. This is interestingly different in overall shape from more routine textbooks, and is rather attractive for that reason. Again an admired book. But as far as the basics are concerned, Manin is in Shoenfield's camp. The object languages are formalized first order languages; but this time we are given many examples of sentences from such languages in 'raw' form (not, as in Enderton or Bell and Machover, via a metalogical representation which may or may not look like the original). Notions like validity are once more defined directly for these first-order languages. So again, the story does not go via schemata (in fact the only time when schemata are mentioned in the book is when talking about axiom schemata as a way of presenting many object-language axioms at once).

6 Dirk van Dalen, *Logic and Structure* (1980)

(a) Gentzen's conception of a natural deduction system for logic only very slowly became fully appreciated, at least by mainstream mathematical logicians. For example, Kleene's masterful *Introduction to Metamathematics* (1952) has a final chapter on Gentzen, but this concentrates on the sequent calculus; and there is something about natural rules of inference – but (mis)conceived as derived rules of an axiomatic system – in Kleene's *Mathematical Logic* (1967). But even that much acknowledgement is unusual in texts by mathematicians of the time. And after Dag Prawitz's little monograph *Natural Deduction* (1965) and the publication of an English translation of Gentzen's papers (1969), it still seems to take a while for the word to really get around.

Thus Enderton's (1972) proof system for first-order logic is baldly axiomatic (with modus ponens as the sole rule of inference). He says of formal proofs in his system 'These will mirror (in our model of deductive thought) the proofs made by the working mathematician to convince his colleagues'. But *that*'s not a very plausible claim! If you want that kind of mirroring without too much distortion, you surely really will need to deploy a natural deduction system. Bell and Machover (1977) introduce both 'the method of first-order tableaux' and an axiomatic proof-system. The first is regarded as a strategy for trying to show that a given finite set of formulas is not satisfiable (so the tableaux regiment semantic reasoning in the metalanguage), while the system given for regimenting object-language deductions is axiomatic again. They only mention natural deduction in a fleeting historical note which runs together Gentzen's approach and tableaux (as if one is just the other turned upside down). Manin (1977) again gives us an axiomatic system.

Neil Tennant deserves the credit, as far as I can recall, for writing the first logic text to focus on Gentzen-style natural deduction (*Natural Logic*, 1978). But this was aimed primarily at philosophers. The first mathematical logic text book where natural deduction is put centre stage followed a couple of years later, Dirk van Dalen's *Logic and Structure* (1980, now in its 5th edition).

(b) You might expect that a text that aims to regiment the modes of deductive inference real mathematicians use would take the ingredients of the regimented arguments also to be real contentful propositions.

But actually things are somewhat murkier in van Dalen's text. In fact, he can be surprisingly slapdash. Here's one just example: In the treatment of first-order logic, we get introduced to 'the' language of a certain similarity type or signature (§2.3 of the 4th edition). So we'd expect there to be just one language whose sole non-logical expression is a dyadic predicate (for example). But we are soon given two different languages with this signature, neither using the predicate supplied by 'the' language for the signature (§2.7). So what exactly is going on here? We might momentarily suspect something like Kleene's approach, so that §2.3 is giving us a uniform way of talking, metalogically, about the various specific object languages we meet in §2.7. But that doesn't actually seem to be van Dalen's line.

Earlier we are introduced to 'the language of propositional logic' whose wffs are built up from the proposition symbols '$p_1, p_2, p_3, \ldots$' in the predictable way. But how are we to regard the proposition symbols p_i? Initially we are told they 'stand for the indecomposable propositions, which we call atoms or atomic propositions' (p. 7 in the 4th edition). But also the propositions of propositional logic 'are built up from rough blocks by adding connectives. The simplest parts (atoms) are of the form "grass is green", "Mary likes Goethe", "$6 - 3 = 2$', which are simply true or false' (p 15). The first formulation again makes it sounds as if the p_i *stand for* atomic propositions like *grass is green* (as in Kleene again, perhaps: it sounds for a moment that we are at the meta-level); but the second formulation makes it sound as if the p_i *are*, or are equivalent to, such atomic propositions. I think van Dalen's intended view is probably that his formal languages of propositional or first-order logic are formalized object-languages (which get applied by interpreting their constituents in an ad hoc way, differently in different uses).

Anyway, he characterizes a tautology as a proposition of the language of propositional logic (a proposition of the object language, as we are taking it) which is always true, i.e. true on all valuations (p. 18). Van Dalen does wobble again by a couple of pages later referring to '$\varphi \vee \psi \leftrightarrow \psi \vee \varphi$ as a tautology (when he's made it very clear that the likes of φ don't belong to his propositional language but are helpful additions to our informal metalanguage, i.e. English): but the charitable reader will discern careless talk once more. He isn't the clearest about such matters, but I suggest that for van Dalen too, logical properties like being a tautology belong, in the first instance, to particular object-language propositions, not to meta-linguistic schemata.

7 And so it goes . . .

Where next? Looking along my shelves at well-known, well-regarded books published in the last twenty years, I find René Cori and Daniel Lascar, *Mathematical Logic* (original French publication 1993); H-D Ebbinghaus, J. Flum and W. Thomas, *Mathematical Logic* (1994); Christopher C. Leary, *A Friendly Introduction to Mathematical Logic* (2000); Peter G. Hinman, *Fundamentals of Mathematical Logic* (2005); Wolfgang Rautenberg, *A Concise Introduction to Mathematical Logic* (2006); Ian Chiswell and Wilfrid Hodges, *Mathematical Logic* (2007). Good books all. But this is getting a little wearisome, isn't it, and I don't want to test your patience any more. I just report that, in their basic approach to the handling on symbolism and the definitions of logical notions, some are more like Shoenfield, some more like Enderton. None are like Mendelson. In fact, Mendelson's is the *only* classic mathematical logic text I've found so far that does clearly and unambiguously take the sort of line that we found Goldfarb and Bostock saying is characteristic of modern logic.

Which isn't to deny that there are important differences between Frege's approach to logic and Russell's approach, on the one hand, and most contemporary logicians on the other. But it does suggest that whatever is to be put on our side of the contrast, it isn't a matter of us moderns typically giving schemata a special role in our very definitions of key logical properties like validity.