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New Essays on Belnap-Dunn Logic

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Prof. J. Michael Dunn and Prof. Nuel D. Belnap
Pittsburgh, April 2018

(Photo: courtesy of Prof. Anil Gupta)

Contents

An invitation to *New Essays on Belnap-Dunn logic*

Hitoshi Omori and Heinrich Wansing

Abstract In this introductory note, we place the new essays on Belnap-Dunn logic, **FDE**, of the present volume against the background of the development of **FDE**. This note is an invitation to study the volume. It presents a chronological perspective on Belnap-Dunn logic and a slightly idiosyncratic list of further research topics.

Keywords Belnap-Dunn logic • First degree entailment • Tautological entailment • Dunn's semantics • Relevance logic • Routley star • American plan • Australian plan • Paraconsistent logic • Exactly true logic • Non-falsity logic • Bilattices • Trilattices • Constructible falsity • Catuṣkoṭi • Negation as a modal operator • Connexive logic • Dialetheism

1 Introduction

Among the continuum many systems of nonclassical logic, Belnap-Dunn logic seems to enjoy a very special status for a number of different reasons. With Belnap-Dunn logic at hand, we are able to offer different topics of interest to philosophers, computer scientists, and mathematicians, such as relevance of entailment, aboutness, negation, paraconsistency, inconsistency-tolerant information processing, definitional equivalence, etc. Moreover, at class rooms, we can show some of the basic ideas behind different techniques that are standard in philosophical logic by only focusing on Belnap-Dunn logic, including many-valued semantics, two-valued relational semantics, possible worlds semantics, algebraic semantics, and so on. When it comes to the latter pedagogical aspect, Belnap-Dunn logic seems to score better than the more popular classical logic.

The present edited volume is in fact the second volume by the same editors dedicated to Belnap-Dunn logic. The first volume, which appeared as a special issue of *Studia Logica*,[1] focused on the more technical developments related to Belnap-Dunn logic. This volume, in contrast, aims at including also contributions that touch some more philosophical issues related to Belnap-Dunn logic. Moreover, we are very happy and proud to be able to reprint the three seminal papers by Nuel D. Belnap and J. Michael Dunn [4,5,10], publish the famous manuscript "Natural Language versus Formal Language" by J. Michael Dunn for the first time, and include an interview with Nuel D. Belnap as well as a new essay by J. Michael Dunn. These form the first part of the volume, titled **Essays by the Founders**.

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[1] Special issue "40-years of FDE," *Studia Logica* 105(6), 2017.

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As one can see by reading them, these essays themselves deliver a lot of information regarding the early exciting developments of Belnap-Dunn logic. It even seems to us that they are already serving as an excellent introduction to the whole literature on Belnap-Dunn logic, and we therefore felt justified to decide to keep our introduction short and not to keep our readers wait too long before jumping into the whole volume.

Still, we believe that it is one of the editors' responsibilities to guide our readers into the new essays on Belnap-Dunn logic. We will, however, refrain from reviewing the basic technicalities of Belnap-Dunn logic since our introduction [25] to the first volume should serve well for that purpose. Therefore, for the rest of this introduction, we first offer a brief guide to the new essays, included in the second part of the volume under the title **New Essays**, by reviewing some major developments related to Belnap-Dunn logic, and putting each essay in context (but without giving a summarizing overview). We then turn to pointing out a few topics that are related to Belnap-Dunn logic and seem to be quite interesting from the editors' perspective.

2 Major developments and contributions to this volume

1959: Belnap on the first degree entailment of relevance logic E

What we refer to as Belnap-Dunn logic is also known as first degree entailment, or first-degree entailment logic, **FDE**. This is so because of the origin of the logic under discussion. More specifically, first degree entailments are formulas of the form $A \rightarrow B$, where the formulas A and B contain at most conjunction, disjunction, and negation. Belnap, in his unpublished doctoral dissertation from 1959, presented an axiom system that captures the first degree entailment fragment of the Anderson-Belnap system **E** of relevant entailment. Moreover, Belnap, in an abstract published also in 1959, reported on a characterization of the provable first-degree entailments in an intuitive way as the tautological entailments.[2] These results can be found in [2]. Semantically, the valid first degree entailments of **E** were characterized by an eight-valued matrix. This was later improved by Timothy Smiley, who pointed out that a four-valued matrix will suffice for the characterization, but the four-values did not have their intuitive readings yet.

As one can see from the interview with Belnap, included in this volume, Belnap is still in favor of viewing Belnap-Dunn logic as first degree entailment in the language with implication rather than in the language without implication, implication being replaced by a (semantic or proof-theoretic) consequence relation. The latter seems to be the more popular and prevailing presentation nowadays, but it is also important to keep in mind the origin of Belnap-Dunn logic.

A note on the papers in this volume (we will use this way of highlighting contributions to the present volume with a bar on the left). Allen Hazen and Jeff Pelletier also take the more recent presentation of Belnap-Dunn logic, and in view of the result that we cannot define reasonable implication connectives in the language with negation, conjunction, and disjunction, their contribution discusses, among other things, some expansions of

[2] $A \rightarrow B$ is a *tautological entailment* iff it can be put into a provably equivalent normal form $A_1 \vee \ldots \vee A_m \rightarrow B_1 \wedge \ldots \wedge B_m$ and for every $A_j \rightarrow B_k$, the conjunction A_j and the disjunction B_k share a propositional variable (so that $A_j \rightarrow B_k$ is tautologically valid in this sense).

FDE, as well as Kleene's strong three-valued logic, **K3**, and the logic of paradox, **LP**, by different implications.

1966: Dunn's intuitive semantics

So, it should be now clear why Belnap-Dunn logic is also known as **FDE**. We next turn to a contribution by Dunn, which substantially improved the semantic understanding of **FDE**. In brief, Dunn offered a semantics for **FDE** in terms of *two* truth values. This was made possible by using a non right unique valuation relation instead of a total valuation function. Roughly speaking, the truth and the falsity of a formula now come apart, and as a result, the given connectives will receive not only truth conditions but also falsity conditions in the semantics. In particular, the negation of A is true iff A is false, and the negation of A is false iff A is true.

Even though this new semantics was published only in 1976, in a paper which is reprinted in this volume, the results were already in Dunn's dissertation from 1966, and thus we decided to place Dunn's contribution here, not later.

A note on the papers in this volume. Jc Beall's contribution addresses the question whether Belnap-Dunn logic may serve as "The One True Logic." As Beall acknowledges explicitly at the beginning of his essay, this was not a question addressed by Belnap or Dunn, but Beall aims at giving some arguments in favor of the claim that **FDE** is The One True Logic.

1972: Routleys' star semantics

Yet another *two*-valued semantics for **FDE** was presented by Richard Routley (later Sylvan) and Valerie Routley (later Plumwood). In contrast to Dunn's semantics, this was achieved by using a possible worlds semantics and, especially, by making use of the so-called star operation on worlds, which is an involutive operation. In particular, the negation of A is true at a world w iff A is not true at the star world w^* of w.

A note on the papers in this volume. Adam Přenosil considers two expanded languages of **FDE**, one obtained by adding two propositional constants, and the other obtained by adding intuitionistic implication besides the two constants. For the latter language, Přenosil makes an interesting use of star semantics to formulate a system that can be seen as a combination of classical logic and intuitionistic logic.

So, we now have two different but equivalent semantics for **FDE** with two truth values, namely Dunn's semantics and Routleys' star semantics. Even though these semantics are equivalent for the language of **FDE**, they turned out to be quite different when it comes to devising semantics for the full language of relevance logics. Two different approaches to the semantics of relevance logics are sometimes called in the literature *the American plan*, that does without the star operation, and *the Australian plan*, that makes use of the Routley-star.

A note on the papers in this volume. Takuro Onishi's contribution is concerned with the relation between the American plan and the Australian plan. Building on a paper by Richard Routley, [31], Onishi aims at showing that the Australian plan is obtained by developing the American plan.

1977: Belnap's seminal papers

Belnap's four-valued semantics as presented in the two seminal papers [4,5] can be seen as obtained by a combination of Belnap's ideas together with Smiley's observation that for characterizing **FDE** it is sufficient to have four values, instead of eight values, and Dunn's semantics. More specifically, the four values can be represented as the elements of the powerset **4** of the set of classical truth values **2** = $\{1, 0\}$. Thus, we have **t** = $\{1\}$, **f** = $\{0\}$, **b** = $\{1, 0\}$, and **n** = $\{\}$. These four-values were motivated and read by considering "How a computer should think" in terms of information passed to a question-answering computer confronted possibly with contradictory information or nor information at all concerning some given atomic formulas. If the value **t** is assigned to an atomic formula p, then p is understood as *told only true* by at least one information source. For the remaining values, we obtain that the value **f** is read as *told only false*, **b** as *both told true and told false*, and **n** as *neither told true nor told false*. Moreover, the representation of the four values as the subsets of **2** led Belnap to distinguishing two partial orders on **4**, an "approximation" ordering with **b** as the top element and an ordering referred to as a truth ordering, with **t** as the top element. Belnap called the resulting lattices an approximation lattice and a logical lattice, respectively, and then introduced **FDE** as the logic of the four-element logical lattice, **L4**, with conjunction interpreted as lattice meet and disjunction as lattice join. Moreover, negation is uniquely determined by the requirement that the function interpreting it in addition to mapping **t** to **f** and **f** to **t**, is also monotonic, i.e., respecting the ordering $\leq$ of **L4**. Finally, Belnap defined semantic consequence as order preservation; B follows from A just in case for every four-valued valuation function v, $v(A) \leq v(B)$.

A note on the papers in this volume. Katalin Bimbó's contribution aims at exploring non-monotonic consequence relations based on Belnap-Dunn logic, instead of taking classical logic. More specifically, Bimbó defines several default rules, and considers some applications of them. As non-monotonic logic forms an important area in computer science, Bimbó's contribution can be seen as echoing the original intuition of Belnap that was brought in from computer-related issues.

By seeing how Dunn's idea nicely motivates a reading of the four values, it is also quite natural to define the consequence relation as is done in many-valued logic. In particular, if we assume the standard truth preservation account of the semantic consequence relation based on Dunn's semantics, then the equivalent way is to preserve the values that contain 1, namely **t** and **b**.

Belnap's two papers and the presentation of **FDE** as a *useful four-valued logic* for how a computer should process information meant a kind of break-through for Belnap-Dunn logic. Given a many-valued semantics, there are, however, also other choices for the set of designated values from **4**. Pursuing the idea that in some sense positive values are preserved in semantic consequence, the following choices are perhaps suggestive, namely to take **t** only, and all values except **f**. The resulting logics are known as **ETL** (exactly true logic) and **NFL** (non-falsity logic), respectively, and discussions on these logics can be found in [20,28] and [32,36], respectively.

> *A note on the papers in this volume.* Yaroslav Shramko offers various proof systems that are equivalent to the original formulation given by Belnap, but more flexible in considering various extensions of Belnap-Dunn logic. One of the implications of Shramko's results include sound and complete proof systems for **ETL** and **NFL**.

Note also that there is some room for discussing how we give intuitive readings to the four values. See, for example, the paper [9] by Didier Dubois and a reply [40] to it by Wansing and Belnap concerning whether the four values are to be understood in epistemic or rather in informational terms.

> *A note on the papers in this volume.* Andreas Kapsner focuses on the issue of possible readings of the four values with some constructive flavors. The term "constructive" is here understood along the line of thought given by Michael Dummett, who is also one of the main figures discussed earlier by Kapsner in [18].

The algebraic structure enjoyed by the four values in Belnap's semantics was later generalized in terms of the notion of a *bilattice*, introduced by Matthew Ginsberg in [12]. Briefly speaking, and as already pointed out, the four values form a lattice structure with respect to two different orderings, one measuring the amount of truth (in a sense), the other measuring the amount of information. Further interesting developments can be found in, e.g. [3,11].

> *A note on the papers in this volume.* Francesco Paoli's contribution is concerned with an expansion of bilattices by what is called *demi-negation*, introduced by Lyold Humberstone in [14]. In brief, demi-negation is a unary operation such that it behaves as a single classical negation (or possibly other negations one prefers) once they are iterated twice. Paoli explores both semantics and axiomatizations from the perspective of abstract algebraic logic.

Note also that the developments of bilattice logics were followed by the introduction of the notion of a *trilattice*, by adding an ordering to measure constructivity, a suggestion made by Yaroslav Shramko, J. Michael Dunn, and Tatsutoshi Takenaka in [33], or to measure falsity, a suggestion made by Yaroslav Shramko and Heinrich Wansing in [34]. For the latter approach, see also [35].

1984: Almukdad and Nelson's constructible falsity

Quite independently of the developments we have seen so far, there is also another related development by David Nelson. Nelson's first work on the related topic of strong negation was already published in 1949, namely [21], but that was, seen from the current perspective, resulting in a paracomplete logic, but not in a logic that is both paracomplete and paraconsistent. It was in [1] that Nelson together with Ahmad Almukdad came up with the system nowadays called **N4**, which can be seen as an expansion of Belnap-Dunn logic by intuitionistic implication. The logic **N4** enjoys constructible falsity as a constructive feature in addition to the constructive features of intuitionistic logic, namely that $\sim(A \wedge B)$ is provable iff it holds that $\sim A$ is provable or $\sim B$ is provable. However, Almukdad and

Nelson did not refer to the literature on Belnap-Dunn logic, and it was only much later that the relationship between **FDE** and **N4** has been pointed out *explicitly*, for example in [16]. A discussion of **N4** in connection with paraconsistency, relevance logic, and information processing can already be found in the dissertation [38].

Note that there exists some intensive and systematic research on Nelson's logics. This includes investigations on algebraic semantics given by Sergei Odintsov in [22], as well as on proof systems studied by Norihiro Kamide and Heinrich Wansing in [16,17]. One of the more recent developments related to Nelson's logic includes the systematic development of modal logics based on the classical extension of Nelson's logic since [23] (cf. [24] for an overview).

> *A note on the papers in this volume.* The joint paper by Igor Sedlár and Ondrej Majer takes the modal logic developed by Odintsov and Wansing in [23], and develops a framework which not only allows to deal with inconsistent bodies of information (this was already possible within the framework presented in [19]), but also to keep track of the sources of inconsistency. Sedlár and Majer also expand the semantic framework by adding a compatibility relation to allow more expressivity on the relation between sources, and presents some basic results.

2010: Priest's Logic of Catuṣkoṭi

The application of Belnap-Dunn logic to philosophy is not restricted to the Western tradition, but also stretches out to the Eastern tradition. The idea of applying paraconsistent logic to buddhism has been in the air among dialetheists since the 1980s, but one of the first clear applications of paraconsistent logic was given by Graham Priest in [29].

> *A note on the papers in this volume.* Jay Garfield's contribution offers a comparison between Belnap and Nāgārjuna by pointing to a few similarities and differences, with a special emphasis on the notion of truth. This is also related to the reading of the four truth values of Belnap-Dunn logic, already mentioned earlier.
>
> Graham Priest's contribution is concerned with developing systems of natural deduction for what Priest calls the **FDE** family. This family obviously includes **FDE** itself, but also comprises **FDEe**, which is the system introduced and discussed in [29]. It is worth highlighting that the construction Priest uses to obtain **FDEe** from **FDE** is an application of a very general construction called *plurivalent semantics* which produces many-valued semantics out of another many-valued semantics with less truth values. The details of the construction is given in [30], and a comparison of Priest's technique with Dunn's semantics, with a focus on truth-value gaps, is briefly discussed in [37].

3 Some further topics: a small and idiosyncratic list

There are some other topics related to Belnap-Dunn logic, but not discussed as central topics in the contributed essays. We will pick three topics that are of special interest to the editors, and briefly discuss why we believe these topics are interesting.

3.1 Negation

As we saw earlier, Belnap-Dunn logic enjoys at least two kinds of two-valued semantics, namely Dunn's relational semantics and the Routleys' star semantics. This has an implication on two different semantics for negation.[3] There are some recent discussions comparing negation as a modal operator with negation as a contradictory-forming operator (in the sense mentioned above that (i) the negation $\sim A$ of a sentence A is true iff A is false and (ii) $\sim A$ is false iff A is true), see [6,7,8]. In the end, the discussion may boil down to a matter of taste. Still, it remains to be seen what we can learn about negation from the two different semantics. In particular, it seems to be worthwhile to set up some criteria and compare the two accounts of negation in more detail.

3.2 Connexive logic

One of the charms of Belnap-Dunn logic, when formulated in terms of Dunn's semantics, is the possibility to formulate highly non-classical principles, such as Aristotle's theses and Boethius' theses that characterize connexive logic.[4] In fact, if we set the formulation of connexive principles as one of the criteria to compare two accounts of negation, then Dunn semantics seems to score far better than Routleys' semantics. Indeed, we only need to modify the falsity condition for the conditional in a very simple manner in the former, whereas it is extremely complicated to achieve that in the latter. In fact, it seems that we can reach a much broader family of contra-classical logics (cf. [15,26]) in a relatively simple manner. It again remains to be seen to which extent this is the case, and what is the exact picture we obtain of contra-classical logics.

3.3 Inconsistency and dialetheism

Finally, there is the issue of interpreting the *both* value in Belnap's four-valued semantics, or allowing propositions to be related to *both* true and false in Dunn's relational semantics. As one can see from their writings, both Belnap and Dunn are strictly resisting the dialetheic reading, defended by Routley and Priest, among others. Still, there seems to be a reason why we still need to keep discussing this issue. Here is why. As a byproduct of the approach to connexive logics, mentioned above, the propositional logic will have a formula and its negation as both valid for certain formulas. Of course, one might see this as a bad result, and apply a *reductio* to conclude that the very approach to connexive logic should be rejected.[5] But if, for whatever reasons, may they be more technical or philosophical, the approach to connexive logic turns out to be favored, then we need to make sense of the inconsistency. How exactly that should be done, again, remains to be seen.

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[3] Needless to say, these two semantics do not exhaust the options. For an up-to-date overview, see [13].

[4] For an overview of connexive logics, see [39], and for some current trends, see [27].

[5] Note that some connexive logics are indeed consistent.

4 Concluding remarks

Already 60 years have passed since the publication of Belnap's abstract on first degree entailment in 1959, and 50 years since the talk "Natural Language versus Formal Language" delivered by Dunn in 1969. Half a century is quite a long time, and we have already seen a lot of exciting developments related to **FDE**. Still, as the essays in this volume show, there are many directions for further explorations, and we hope that some readers will be motivated to join the continuing investigation and development of Belnap-Dunn logic.

Acknowledgment First of all, we would like to thank Nuel D. Belnap and J. Michael Dunn for agreeing to be part of this project and offering us numerous support. We would also like to thank the authors of the new essays for accepting our invitation and contributing excellent essays that shed light on a number of different formal as well as philosophical aspects of Belnap-Dunn logic. Moreover, we would like to thank Otávio Bueno for his enthusiastic support for our volume as the editor–in–chief of the Synthese Library series, Anil Gupta and Ties Nijssen for their kind help, which was necessary and substantial to deal with some of the challenges to reprint the seminal papers, and Tobias Koch for assisting us in the typesetting of the manuscript.

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Part I
Essays by the Founders

Natural Language versus Formal Language*

J. Michael Dunn

The comparison of natural languages and formal languages has become quite popular of late. The topic was on the program of the last International Congress for Logic, Methodology and Philosophy of Science in Amsterdam, and also on the program of the 1968 New York University Institute of Philosophy. I have read the published results of both meetings [1], and I must say that I am not quite sure what all the fuss is about.

On both occasions it was pointed out that a natural language typically differs from a formal one in that a natural language is ambiguous, vague, context dependent, and generally untidy. I agree that one can typically point to these differences, but frankly my reaction is, so what? We all know that untidiness has both its good points and its bad.

The very title of this symposium, "Natural Language *versus* Formal Language", suggests a certain opposition that I think is inappropriate. It sounds rather like the opposition, "Ford cars versus General Motor cars", but it seems to me that the opposition is more like that of "Ford cars versus John Deere tractors". It could be that both are useful, for different purposes.

* *For presentation at the joint APA-ASL symposium, New York, Dec. 27, 1969.* I presented "Natural Language versus Formal Language" as an invited speaker (together with Frederic Fitch, Bas van Fraassen, and Richard Montague) in the joint symposium by that title of the Association for Symbolic Logic and the American Philosophical Association at their joint meeting in New York, December, 1969. While it covers a number of topics related to that symposium, it was also the first public presentation I gave of the 4-valued semantics for my Ph.D. supervisor Nuel Belnap's system FDE of First-Degree Entailments.
I prepared a typed manuscript just prior to that talk. Heinrich Wansing and Hitoshi Omori, working from a computer scan of a poor photo scan that I provided, prepared a much more readable transcript for this volume. My computer scan is at http://www.philosophy.indiana.edu/people/papers/natvsformal.pdf. Unfortunately the copy that I had did not contain the references, though it did contain their citations in the text. I was easily and unambiguously able to reconstruct all but one of the intended references. The only one I could not find is [4] on p. 1 when it mentions "the program of the 1968 New York University Institute of Philosophy," whatever that is. The manuscript builds on material from my dissertation *The Algebra of Intensional Logics*, Univ. of Pittsburgh, 1966 (Director: Nuel D. Belnap), which also does "An Intuitive Semantics for First Degree Relevant Implications," contributed paper, meeting of the Association for Symbolic Logic, Chicago, May, 1967, (Abstract) "An Intuitive Semantics for First Degree Relevant Implications," *The Journal of Symbolic Logic*, 36, 1971, pp. 362 363. And it is a precursor to my "Intuitive Semantics for First Degree Entailments and Coupled Trees," *Philosophical Studies*, 29, pp. 149 168.
For more information about the relationships of the items mentioned above, and to work by others (particularly, R. and V. Routley and N. Belnap) see my paper "Partiality and its Dual," *Partiality and Modality*, eds. E. Thijsse, F. Lepage & H. Wansing, special issue of *Studia Logica,* Vol. 66, 2000, pp. 5-40. Another place to look is my "Relevance Logic and Entailment," in *Handbook of Philosophical Logic*, vol. 3, eds. D. Gabbay and F. Guenthner, D. Reidel, Dordrecht, Holland, 1985, pp. 117-224, or the newer version "Relevance Logic" (with G. Restall), *Handbook of Philosophical Logic*, 2nd edition, vol. 6, eds. D. Gabbay and F. Guenthner, Kluwer Academic Publishers, pp. 1-128.

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Thus if one is writing poetry, it seems desirable to have a language that is ambiguous; but not necessarily if one is writing mathematics. And if one is talking to one's wife, vagueness might be a convenience; but maybe not if one is programming a computer.

Now surely the purpose which is of most interest to the people here today is that of "doing philosophy", as we say. With this purpose in mind, one might think that there is a definite choice, at least in principle, between natural languages and formal languages. But I doubt that there would be any universal agreement on this choice, simply because I am sure that there would be no universal agreement about what doing philosophy involves. Indeed, if doing philosophy is an activity having something to do with the gaining of insights, these insights might just as well be produced by a Zen master's stick as by the use of either natural or formal language.

But if one believes that part of doing philosophy involves attempts at constructing valid arguments, then it seems to me that one should be concerned with making explicit why they are valid, at least in problematic cases, and the best way of doing that is by means of a formal language. I am not suggesting that arguments should actually be written completely in a formal language, nor that the steps in arguments be numbered and labeled according to, say, Copi's rule. Mathematicians do not do this either, but the formal structure of their arguments is usually (though not always) clear enough that it could be reconstructed in some appropriate formal language if one so desired.

When the classical logic of *Principia Mathematica* was the only brand of formal language on the market, it was understandable for some philosopher to feel that his argument lost something in translation (often its validity) when it was formalized. But nowadays, what with modal logics, free logics, tense logics, deontic logics, epistemic logics, entailment logics, *et al*, this feeling deserves less sympathy. Of course it is always possible that a philosopher with such a feeling has some genuine insight about what follows from what, and that this insight is not captured by any extant formal logic. But insights being rather rare, and logical errors being all too common, a little shopping around among at least the more well known formal logics would not hurt. And if none of these fit, one is always free to knit one's own.

I have pointed out that logicians seem to be getting away from the bugaboo, to paraphrase Ramsey out of context, "What we can't say in *PM* we can't say, and we can't whistle it either". Formal languages are becoming increasingly natural, and I believe that this undercuts part of the supposition behind the topic of this symposium.

There is a converse development which also tends to undercut the distinction between natural and formal languages. Roughly put, natural languages are lately appearing to be more formal. What with the work that Chomsky and others have done on generative grammar, it is no longer clear that the so-called natural languages such as English are not formal languages after all. It is now frequently conjectured that the grammatical sentences of, say, English are recursive. It is true that the transformational rules most often suggested for generating them are context-dependent, whereas the rules for generating the well-formed sentences of a formal logic are typically context-independent. This, however, seems to be but a mere difference in detail (which is not to say that detail cannot be extremely interesting).

Now it might be thought that the difference between a natural language and a formal language arises not at the level of syntax, but instead at the level of semantics. The most extreme view that might be taken here is that of the formalist: a formal language,by definition, is regarded as uninterpreted. I learned very early to avoid quarreling with definitions. But I cannot help pointing out that many of the languages created by logicians are regarded as interpreted. Indeed, Gödel's famous completeness and incompleteness theorems take full advantage of such interpretations.

Now it is quite true that the semantical theories proposed by logicians for their formal languages have typically differed from the semantical theories of natural languages proposed by linguists. Thus the semantics of formal languages has typically centered around a Tarski-type recursive definition of truth. This definition can be quite complex in the case of some of the more sophisticated formal languages whose semantics is some variant of the Kripke semantics for modal logic. (Professor Montague has a general semantical theory for such languages, calling them all *pragmatic languages*.) But still the basic objective is to define under what conditions a sentence is true (relativized to a model, a world, a history, a speaker, or what have you).

On the other hand, the semantical theories of natural languages proposed by linguists have typically avoided the notion of truth altogether. Instead they have tried to provide "readings" of sentences by some ideal representation of their semantic structures. Simple semantic components, for example Katz's semantic markers which are supposed to stand for simple ideas, are strung together in such a way so as to provide an unambiguous reading. These semantic theories have concentrated on the ways that readings are generated from sentences, and of central interest here has been the disambiguation of sentences into their different readings.

At the risk of oversimplifying, the quickest way to characterize the difference between the logicians' and the linguists' semantic theories is to mobilize Quine's division of semantics into the theory of reference and the theory of meaning. Logicians have talked as if they have been concerned with the former, and linguists as if they have been concerned with the latter.

Lately, however, there have been developments on the side of the logicians which challenge this easy dualism. Thus it has been argued that an account of meaning for both formal and natural languages alike should proceed via a Tarski-type truth definition. In defense of this, Donald Davidson says [2]:

> There is no need to suppress, of course, the obvious connection between a definition of truth of the kind that Tarski has shown how to construct, and the concept of meaning. It is this: the definition works by giving necessary and sufficient conditions for the truth of every sentence, and to give truth conditions is a way of giving the meaning of a sentence. To know the semantic concept of truth for a language is to know what it is for a sentence – any sentence – to be true, and this amounts, in one good sense that we can give to the phrase, to understanding the language.

Professor Montague has also argued on several occasions for handling the semantics of natural languages in the same general manner as the semantics of formal languages, and his English as a Formal Language I is an ingenious and penetrating application of this point of view.

Now let us leave aside the obvious problems caused for this approach by the fact that there are many sentences in natural languages that appear to be neither true nor false (questions, imperatives, etc.). These cases need not necessarily vitiate the Montague-Davidson approach. First, because there may be ways of handling the troublesome cases by some extension of the concept of truth (perhaps along the lines suggested in Michael Dummett's paper "Truth", and as actually carried out in the Belnap approach to the logic of questions and the Casteñada approach to the logic of imperatives). Second, because even though the Montague-Davidson approach might not be appropriate to all of English, it might still be applicable to that large fragment of English consisting of ordinary declarative sentences.

Indeed, both Montague (in [1], p. 276) and Davidson (in [2]) have claimed that the Tarski truth definition can be straightforwardly applied so as to provide a satisfactory semantics for that fragment of English that consists of the literal translations of the formulas of the classical predicate calculus.

Should we identify the meaning of a sentence with its truth conditions? I do not ask this as a metaphysical question like the question are numbers *really* classes of equinumerous classes? If classes of equinumerous classes behave enough like numbers, then at least we have some sort of isomorphism, and that is all we need for certain purposes dear to logicians. But the present question is whether truth conditions do behave enough like meanings. Clearly, meanings determine truth conditions, but I do not find the converse so obvious.

Now there is an obvious truism that we can take to justify the claim that truth conditions determine meaning. Thus consider the sentence 'snow is white'. A truth condition for this sentence is: 'Snow is white' is true iff snow is white. I readily agree that if one knew and understood this truth condition, then one would know the meaning of the sentence 'snow is white' (for the unexciting reason that one must understand this sentence in order to understand the truth condition, which is formulated by using this very sentence itself). If truisms such as this were the only things in the wind, I would not bother to turn my head.

But typically when people claim that truth conditions determine meaning, they go on to say some profound but ultimately silly things, such as that any two logically equivalent sentences have the same meaning since they have the same truth conditions. This leads quickly to the view that any two logically false sentences (or any two logically true sentences) are synonymous. We get the most striking application of this line of thought in Wittgenstein's *Tractatus*, where he says [8, 4.461]:

> Propositions show what they say: tautologies and contradictions show that they say nothing.
>
> A tautology has no truth conditions, since it is unconditionally true: and a contradiction is true on no condition,
>
> Tautologies and contradictions lack sense.

I simply do not believe that the sentence

$$\exists x \forall y (y \in x \equiv y \notin y)$$

has the same meaning as

$$1 \neq 1$$

nor do I believe that they are both meaningless, even though I grant that they are both logically false.

This Tractarian view survives today in the best logic texts. Jeffrey, in his *Formal Logic* [5] says a little more than most authors to justify that the truth table rules of valuation give meaning to the connectives. Thus he says (p. 15):

> The rules of valuation make no mention of the meanings of sentences; they are couched entirely in terms of truth-values. Nevertheless, the rules of valuation determine the meanings of compound sentences in terms of their ingredient sentence letters, for we know the meaning of a sentence (we know what statement the sentence makes) if we know what facts would make it true and what facts would make it false. Now if we have this information about the letters that occur in a sentence, the truth conditions supply the corresponding information about the whole sentence.

A little later (pp. 30-31) in discussing contradictions, Jeffrey says:

> The sentence
>
> It is and is not raining
>
> is only apparently about the weather, just as the sentence

$$2 + 2 = 4 \text{ and } 2 + 2 \neq 4$$

is only apparently about numbers. In fact the two sentences have exactly the same truth conditions: in all possible cases, both are false.

I think we can avoid the necessity of Jeffrey's conclusion while yet agreeing, in a trivial sense, that the meaning of a sentence is determined by its truth conditions. Thus, let p be the sentence 'it is raining' and let q be the sentence '$2 + 2 = 4$'. By standard truth table considerations it follows that $p \wedge \neg p$ is true iff p is true and $\neg p$ is true, that is, iff p is true and p is false. Similarly $q \wedge \neg q$ iff q is true and q is false. The question bluntly then is whether the condition that p is true and p is false is the same condition as that q is true and q is false. I think it is not.

Notice that it is no argument against me to reply that the first is a contradiction meaning p is true and p is not true, while the second is also a contradiction meaning q is true and q is not true, and that of course any two contradictions have the same meaning. This only pushes the question with which we began up into the metalanguage.

Intuitively, $p \wedge \neg p$ and $q \wedge \neg q$ describe different situations, granted that neither situation is realizable. What we need is a semantics that is sensible to this intuition.

I may as well let any who do not know me in on a little secret at this point. I was a student of Belnap and Anderson's at the university of Pittsburgh, and I am one of those crazy people who think that there is something in their system E of entailment (and in the other similar relevant logics that have been developed). I believe that there is a sense of 'entails' (or 'implies') in which it simply is not true that a contradiction entails or implies any old sentence whatsoever. It thus becomes extremely critical that not just any two contradictions are synonymous. For if $p \wedge \neg p$ were synonymous with $q \wedge \neg q$, then since it is true that $q \wedge \neg q$ entails q, then by substitution of synonyms *salva veritatae*, it would be true that $p \wedge \neg p$ entails q. Having made a clean breast of my motivation, I hope that they will not be held against me as I continue.

I mention at this point that both Professor van Fraassen and myself have developed semantical ways of ruling out $p \wedge \neg p$'s entailing q. I, in my dissertation [3], in terms of q's possibly being about some topic that $p \wedge \neg p$ is not about, van Fraassen [7] in terms of some fact forcing $p \wedge \neg p$ which does not force q. Both of these semantics lead to completeness proofs for a very narrow fragment of the system E, namely those sentences of the form A entails B, where A and B are purely truth functional (the so-called *first degree* entailments), and it is very difficult to see how these semantics might be generalized so as to take care of all of E (with entailments entailing entailments, etc.). Furthermore, both these semantics suffer from the defect that they are formulated in terms of concepts that are out of fashion in logic (*topics* that sentences are about, *facts* that force sentences to be true).

Let us then recall which concepts are in fashion, and I shall try my best to talk that language in trying to communicate the notion that not just any two contradictions are synonymous. The standard realization of a proposition as found in Montague, Kaplan, Scott and others is a mapping from possible worlds (or reference points, situations, call them what you will) to truth values. That corresponds to the principle that different meaning can be distinguished by different situations with different truth values, i.e., by different truth conditions. But it has the untoward consequence that (relative to a given set of situations) there is only one contradictory proposition, simply because there is only one constant false mapping.

However, we need modify this picture only slightly to provide a kind of extensional apparatus that allows us to distinguish contradictory propositions from one another. Starting from the intuition expressed above that a contradiction can be true in some situations (of course, unrealizable) in which some other contradiction is not true, we can identify a proposition with a relation from a set of situation into the set $\{T, F\}$, where every situation is related to at least one of T and F. A contradictory proposition is then such a relation where F is in the image of every situation. There can then be many different contradictory propositions. These can be distinguished by a situation such that one of the propositions has T in its image while the other does not.

What this means as far as the modeling of truth functional logic is concerned is that a valuation is a relation from sentences into the set $\{T, F\}$, rather than a mapping, and of course it is required that every sentence be related to at least one of T and F (we shall eventually speculate upon what happens if we drop this last requirement). This relation is determined inductively in just the classical truth table way. Thus

i) $\neg A$ is T iff A is F,
$\neg A$ is F iff A is T;
ii) $A \wedge B$ is T iff A is T and B is T,
$A \wedge B$ is F iff A is F or B is F;
iii) $A \vee B$ is T iff A is T or B is T,
$A \vee B$ is F iff A is F and B is F.

Note that in each of i) – iii), we need two clauses, one giving truth conditions and the other giving falsity conditions. We cannot rely upon the standard intuition that a sentence which has been given the value T is not F.

We can already give a semantical explication of one of the principal features of entailment, namely, that $p \wedge \neg p$ need not entail q. For there is a valuation in which $p \wedge \neg p$ receives the value T and yet q does not. This is a valuation in which p receives both the values T and F, while q receives the single value F.

We can also give a semantical explication of perhaps the most controversial feature of entailment, namely, that $\neg p \wedge (p \vee q)$ need not entail q (the failure of the so-called rule of disjunctive syllogism). Let me give this explication in the context of examining the supposed proof of Lewis's that a contradiction entails everything.

The proof starts out by supposing that $p \wedge \neg p$ is true. We then detach p by the rule of simplification, and from p we obtain $p \vee q$ by the rule of addition. Next we obtain $\neg p$ from our supposition of $p \wedge \neg p$ by another use of the rule of simplification. So far, O.K. But finally we claim that q follows from $\neg p$ and $p \vee q$ by disjunctive syllogism. In producing this proof for a class, it used to be my habit to motivate this last step by telling the following story. "So on our assumption that $p \wedge \neg p$ is true, we have obtained that one of p or q is true. But we have also obtained $\neg p$, which says that p is not the true one." When I was once telling this story, some wise guy yelled out, "But p was the true one – look again at your assumption."

That wise guy was right. If we assume that $p \wedge \neg p$ is true, we are thereby assuming that p is both true and false, and hence it should not be surprising that $p \wedge (\neg p \vee q)$ comes out true under that assumption, while q might still be false.

Do not get me wrong; I am not claiming that there are sentences which are in fact both true and false. I am merely pointing out that there are plenty of situations where we suppose, assert, believe, etc., contradictory sentences to be true, and we therefore need a semantics which expresses the truth conditions of contradictions in terms of the truth values that the ingredient sentences would have to take for the contradictions to be true.

I must unfortunately remark that these particular insights have not as yet given the degree of illumination regarding entailment that might be expected. In particular, they have not led to completeness proofs for the system E of Entailment. But I have obtained reasonably intuitive completeness results based upon this framework for an extension of E called R-mingle. These I now announce for the first time, and they should not be confused with earlier purely algebraic completeness results obtained by both my colleague Robert K. Meyer [6] and myself [4]. The intuitive results are rather like those of Kripke for intuitionist logic, and even more like those of Thomason for the system of Professor Fitch's *Symbolic Logic*, though with assignments allowed to give both the values T and F. Furthermore, the extension to R-mingle with quantifiers seems not a bother. The basic idea is that instead of doing the classical thing of interpreting n n-ary predicate as a propositional *function* (a mapping from the n-tuples of objects in the domain into $\{T, F\}$), we rather interpret the predicate as a propositional *relation* (a relation from the n-tuples of objects of the domain into $\{T, F\}$, with the requirement that every n-tuple be related to at least one of T and F).

The reason why we do not obtain a semantics for the system E in this framework is that it is difficult to rule out $p \wedge \neg p$'s entailing $q \vee \neg q$, since $p \wedge \neg p$ is always false and $q \vee \neg q$ is always true. Thus whenever the antecedent is true, the consequent is true (it being always true); and whenever the consequent is false, the antecedent is false (it being always false). Thus we are stuck no matter how we try to falsify the entailment, and yet the entailment is not a theorem of the system E. One way out that suggests itself is to let $q \vee \neg q$ have no truth value, and we could naturally arrange this by allowing an assignment in which q was related to neither T nor F. Then $p \wedge \neg p$ could be given the value T (by giving p both the values T and F), while $q \vee \neg q$ is not given the value T (by giving q no value whatsoever). This works for first degree entailments, but there are vast problems both of an intuitive and a technical sort in generalizing this approach to entailments nested in entailments.

In closing, let me urge that even it the particular approach I have suggested for distinguishing contradictions semantically is not your liking, still something should be done in this area. It may be good logic to say that any two contradictory sentences are logically equivalent, but I would think that it would be bad linguistics to say that any two contradictory sentences have the same meaning.

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Intuitive Semantics for First-Degree Entailment and 'Coupled Trees'

J. Michael Dunn

1 Introduction[1]

Classically, an argument *A* therefore *B* is 'valid' (or *A* is said to 'entail' *B*) if and only if (iff) each situation (model) is such that either *A* is false or *B* is true. This fits well with so-called 'tableau' methods for showing that *A* entails *B* by working out the mutual inconsistency of *A* and ∼*B*. But both the classical notion of validity and the corresponding tableau methods allow that *A* may entail *B* because of some feature of *A* alone, irrespective of *B*, and vice versa. Thus if *A* is a contradiction, then each situation is such that *A* is false, and so *a fortiori* is such that *A* is false or *B* is true. And if *A* is a contradiction, then a tableau construction will show that *A* is inconsistent, and so *a fortiori* that *A* and ∼*B* are inconsistent. Of course, the same points can be made dually when *B* is a logical truth.

A competing theory of 'entailment' developed by Anderson and Belnap requires that for *A* to entail *B* there must be some relation of real relevance between *A* and *B*, e.g., they share some sentence letter. In this paper I shall develop a notion of 'relevant validity' and a corresponding tableau method that tie in with the Anderson-Belnap theory.

Jeffrey (1967) introduced 'coupled trees' as a modified tableau method for testing an argument for validity. In Section 2 I shall describe the formalism of the coupled tree method and explain how by pruning it of complications (needed by Jeffrey to get precisely the classically valid arguments) we get a well-motivated syntactical characterization of when an entailment holds relevantly between truth-functional sentences. In Section 3 an 'intuitive' semantical characterization is presented and motivated using inconsistent and incomplete 'situations'. In Section 4 these two characterizations are connected by completeness and soundness results. In Section 5 the semantical characterization is connected similarly with a well-known syntactical characterization ('tautological entailment') due to Anderson and Belnap (1962), and connected by them to the provable 'first-degree entailments' (formulas of the form $A \to B$, where *A* and *B* contain no occurrences of →) of their system **E**. In Section 6 another semantical characterization using 'topics' and having an information-theoretic flavor is related to the semantics of Section 3. So we have the happy circumstance that all these characterizations coincide. In Section 7, I ruminate.

I must mention that there are in the literature by now at least two other semantical modelings of the first-degree entailments, one due to van Fraassen (1969) and the second

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[1] This work was supported in part by NSF Grant GS-33708. I suppose the 'coupled trees' provide the excuse for the present publication, for the rest has seen 'semi-publication' (abstracts, mimeo, talks) some years previously, as specific references in the sequel will indicate. Basically it all stems from my dissertation (Dunn, 1966) (cf. Section 6 of the present paper) and so I must once more express indebtedness to all who helped there. I recall as being particularly 'relevant' here my teachers N. D. Belnap, Jr. (the director) and the late A. R. Anderson, and my then fellow students R. K. Meyer, B. van Fraassen, and P. Woodruff.

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due to Routley and Routley (1972). Both of these have certain similarities to my own and to each other.[2] It would be too lengthy an excursion to compare them all, so I will content myself with just a quick flight over.

The major advantage I see of my semantics over van Fraassen's is that it is less complicated and more familiar both philosophically and mathematically. Van Fraassen's philosophical motivation rests on certain somewhat quaint ontological intuitions concerning complex facts. Mathematically, it is just plain hard to keep straight the various steps and typelevels of his clever set-theoretical reification of these intuitions. There need ultimately be nothing wrong in any of this, but since I have a fairly narrow partisan interest in boosting the Anderson-Belnap relevance program I would like to here present a semantics that is more accessible to the evergrowing number of people who have cut their semantical eye-teeth on the 'possible world' semantics for modal logic.

Now the Routleys' approach does relate directly to 'possible world' semantics. Their 'set ups' are advertised as liberalizations of the notion of 'possible world' so as to include impossible and incomplete 'worlds'. My semantics has the same end – it is the means over which we differ. The Routleys perform some magic with a 'star operation' in giving the truth condition for negation. By a feat of prestidigitation one 'set up' H is switched with another set up H^*. Thus $\sim A$ is true in H iff A is not true in H^* (instead of the usual plain H). But just what is this 'star operation' and why does it stick its nose into the truth condition for negation?[3] This seems to me to remain an ultimate mystery in the Routleys' semantics, and I count it as a philosophical virtue of my semantics that it does without the 'star operation'.

2 Relevantly Coupled Trees

Jeffrey's logic text (1967) provides an excellent introductory treatment of the method of 'analytic tableaux' of Smullyan (1968). Jeffrey (p. 92) compares the method of 'truth trees' (his suggestive name for analytic tableaux) with indirect proof, the essential point being that in order to test an argument, say, A therefore B (in symbols $A \vdash B$), for validity one uses the method of truth trees to test for the mutual inconsistency of A and $\sim B$. The idea of a truth tree is that it diagrams in a branching treelike fashion (so as to keep track of various alternatives) all of the truth conditions of a set of sentences. Each path represents a way in which the given sentences might become true, and when testing for inconsistency we search to see whether all of these paths are 'closed' by virtue of containing both a sentence and its denial.

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[2] The Routleys' and my modelings are basically isomorphic, in a mathematically precise sense. The only reason for the qualification 'basically' is that the Routleys take their 'set ups' to be certain sets of sentences, and so (assuming a denumerable language) crude considerations of cardinality prevent my larger models from having Routley correlates. However, taking 'set ups' more abstractly (as is in fact done in Routley and Meyer, 1973) so as to provide collections of 'set ups' of all cardinalities, each Routley model can be regarded as an assignment to each truth-functional sentence of an element in a 'quasi-field' of sets (this notion from Bialynicki-Birula and Rasiowa, 1957). In Dunn (1966) (cf. also Dunn, 1967,1971) my models (directly in their Section 6 version here) are similarly connected to de Morgan lattices of 'proposition surrogates' or the somewhat dual '2-products' of a field of sets, and these related by isomorphisms to quasi-fields of sets. It would be nice to have some similar connections to van Fraassen's modeling.

[3] Almost any inference could be 'invalidated' by some analogous device. Thus, e.g., one could invalidate the inference from $A \wedge B$ to A by changing the truth condition for conjunction so that $A \wedge B$ is true in H iff both A and B are true in H^*.

Jeffrey (p. 93) provides a modification of the basic method of truth trees, which modification he calls the method of ‘coupled trees’ and compares to direct proof. The basic idea is that in order to test the validity of an argument, $A \vdash B$, one constructs two truth trees, one for A (coming down) and one for B below (going up). Since each path in the tree for A represents an alternative set of truth conditions for A, and similarly for B, it is natural to require that every path in the upper tree ‘cover’ some path in the lower tree in the sense that every sentence letter or denial of a sentence letter (the term *atom* will henceforth embrace both these) that appears in the covered path appears also in the covering path. Thus every way in which A is true is also a way in which B is true.

However, there are two technical complications that Jeffrey needs in order to get precisely the classically valid arguments. I shall explain these complications after I describe with more precision the formalism of the coupled tree method.

Jeffrey’s formalism includes sentence letters (we shall suppose they are p, q, r, etc.), and connectives for negation, conjunction and disjunction (we suppose these are $\sim$, $\wedge$ and $\vee$) as well as for the truth-functional conditional and biconditional. We shall ignore these last two since they are not primitive in the standard formulations of the system **E** (though they can of course be introduced as abbreviatory devices *via* their ordinary contextual definitions). There are then schematically the following five rules:[4]

$$(\sim\sim)\ \frac{\sim\sim A}{A}$$

$$(\wedge)\ \frac{A \wedge B}{\begin{array}{c} A \\ B \end{array}} \qquad\qquad (\sim\wedge)\ \frac{\sim(A \wedge B)}{\sim A \quad \sim B}$$

$$(\vee)\ \frac{A \vee B}{A \quad B} \qquad\qquad (\sim\vee)\ \frac{\sim(A \vee B)}{\begin{array}{c} \sim A \\ \sim B \end{array}}$$

The rules are reasonably self-explanatory. Note that $(\sim\wedge)$ and $(\vee)$ have branching conclusions representing alternatives and are the source of the ‘tree’ structure.

The basic idea of Jeffrey’s coupled tree method is illustrated by the argument $p \wedge q \vdash q \vee r$, for which we can construct the following coupled trees (the arrow indicates covering):

$$\begin{array}{c} p \wedge q \\ p \\ q \\ \swarrow \\ q \qquad r \\ q \vee r \end{array}$$

[4] Jeffrey actually avoids formal recognition of the first rule by a practice of erasing pairs of juxtaposed negation signs, but the rule is formally in Smullyan (1968).

The corresponding entailment $(p \wedge q) \rightarrow (q \vee r)$ is not only a theorem of **E** but it is basic to the motivation of **E** presented in Anderson and Belnap (1962), being paradigmatic of what they there call a *primitive entailment*.

The first of the two technical complications needed by Jeffrey is nicely illustrated by the argument $p \wedge {\sim}p \vdash q$. The coupled tree, if any, for this argument would be the following:

$$\begin{array}{c} p \wedge {\sim}p \\ p \\ {\sim}p \\ q \end{array}$$

But there is a conspicuous absence of covering. This fits nicely with the Anderson-Belnap intuition that there is no relevance between premiss and conclusion. Jeffrey though is concerned to capture this classically valid inference. Thus he complicates the basic idea that every path above must cover some path below by excepting the closed paths above.[5] Thus trivially the above diagram (with a cross written under ${\sim}p$ to indicate that the path is closed) counts as a coupled tree.

The second complication is nicely illustrated by the dual of the last argument, namely, $p \vdash q \vee {\sim}q$. It would seem that the following would represent a failed attempt at constructing an appropriate coupled tree:

$$\begin{array}{c} p \\ q \qquad {\sim}q \\ q \vee {\sim}q \end{array}$$

Again the lack of covering can be taken to be in accord with Anderson-Belnap intuitions about irrelevance.

Jeffrey's device to wash this one through is to permit in the construction of the tree coming down from the premiss a simultaneous branching with a sentence and its negation. Thus the following counts as a coupled tree:

$$\begin{array}{c} p \\ q \qquad {\sim}q \\ \downarrow \qquad \downarrow \\ q \qquad {\sim}q \\ q \vee {\sim}q \end{array}$$

This amounts to tacitly adding as a rule (in constructing upper trees only) the following, which we shall call 'punt':

$$\frac{A}{B \qquad {\sim}B}$$

Let us close this section with the definition it has been motivating: An argument $A \vdash B$ *passes the relevantly coupled tree test* iff in constructing truth trees for A and B, every path in the tree for A (including the closed paths) covers some path in the tree for B (not allowing use of 'punt').

[5] Of course this appears as no complication at all in the (classical) context Jeffrey has working for him, where 'closed' paths are discountenanced in just the way their name suggests.

3 Intuitive Semantics[6]

Wittgenstein says in the *Tractatus* (Wittgenstein, 1961, translation):

> 4.461 Propositions show what they say: tautologies and contradictions show that they say nothing.
>
> A tautology has no truth-conditions, since it is unconditionally true: and a contradiction is true on no condition.
>
> Tautologies and contradictions lack sense.

This Tractarian view survives today in some of the best logic texts. Jeffrey (1967) says a little more than most authors to justify that the truth table rules of valuation give meaning to the connectives. Thus he says (p. 15):

> The rules of valuation make no mention of the meanings of sentences; they are couched entirely in terms of truth-values. Nevertheless, the rules of valuation determine the meanings of compound sentences in terms of their ingredient sentence letters, for we know the meaning of a sentence (we know what statement the sentence makes) if we know what facts would make it true and what facts would make it false. Now if we have this information about the letters that occur in a sentence, the truth conditions supply the corresponding information about the whole sentence.

A little later (pp. 30-31) in discussing contradictions, Jeffrey says:

> The sentence
>
> *It is and is not raining*
>
> is only apparently about the weather, just as the sentence
>
> $2 + 2 = 4$ and $2 + 2 \neq 4$
>
> is only apparently about numbers. In fact the two sentences have exactly the same truth conditions: in all possible cases, both are false.

I think that we can avoid the necessity of Jeffrey's conclusion while yet agreeing, in a trivial sense, that the meaning of a sentence is determined by its truth conditions. Thus let p be the sentence 'It is raining' and let q be the sentence '$2 + 2 = 4$'. By standard truth table considerations it follows that $p \wedge \sim p$ is true iff p is true and $\sim p$ is true, that is, iff p is true and p is false. Similarly, $q \wedge \sim q$ is true iff q is true and q is false. The question bluntly then is whether the condition that p is true and p is false is the same condition as that q is true and q is false. I think it is not.

Notice that it is no argument against me to reply that the first is a contradiction meaning p is true and p is not true, while the second is also a contradiction meaning q is true and q is not true, and that of course any two contradictions have the same meaning. This only pushes the question with which we began up into the metalanguage.

Intuitively, $p \wedge \sim p$ and $q \wedge \sim q$ describe different situations, granted that neither situation is realizable. What we need is a semantics that is sensitive to this intuition.

The by now orthodox realization of a proposition is a function from possible worlds (or indices, reference points, situations, whatever) to truth values (cf. for explicitness Montague, 1972, who credits the idea to Kripke – cf. also the articles by Lewis and Stalnaker in the same volume as the Montague paper). This corresponds to the principle that different meanings can be distinguished by different situations with different truth values, i.e., by different truth conditions. It too has the untoward consequence that (relative to a given

[6] The bulk of this section is taken verbatim from the middle of Dunn (1969).