SORITES

SORITES

ISSN 1135-1349

Issue #07. November 1996

TABLE OF CONTENTS

Editorial Note

Two years ago, on December 16, 1994, the decision was taken to launch *SORITES*, a refereed all-English international electronic quarterly of analytical philosophy. Under whose auspices? A non-profit organization was formed, the «colectivo **SORITES**», incorporated under Spanish law and proprietor of the *SORITES* journal. The *colectivo* then appointed the editorial cabinet, comprising the Editor and the Associate Editors.

On December 20 1996 the **SORITES** team met in Madrid. After canvassing an array of available alternative policies aimed at improving and rationalizing our editorial activity, the Cabinet resigned, while the Editor's appointment was renewed. For efficiency sake ownership of the electronic journal *SORITES* was conveyed to him personally.

Having now designated a new Editorial Cabinet, the Editor hereby thanks the two outgoing members of the previous Cabinet — namely Prof. Jesús Padilla-Gálvez and Prof. Manuel Liz. The Editor gratefully appreciates their excellent work and generous dedication to *SORITES* over the past two years, as well as the high academic standards they have helped to set.

The **SORITES** team has now become genuinely international. Besides Mr. Francisco J.D. Ausín (Centre for Logic and Juridical Analysis, Spain), who remains an Associate Editor, the new Editorial Cabinet comprises two new Associate Editors (Prof. Guillermo Hurtado [Universidad Nacional Autónoma de México] and Dr. Peter J. King [University of North London]), as well as an **Editorial Assitant**, namely: Prof. Raymundo Morado (Universidad Nacional Autónoma de México). The Editor warmly thanks them for their kind willingness to serve on the Cabinet.

As a matter of editorial policy, *SORITES* welcomes contributed papers by all analytical philosophers — whether or not they are members of our board of Editorial Advisors. All papers will be refereed, of course. As a rule, though, members of the Editorial Cabinet will publish no papers in our journal except reviews or critical notices. By so doing we are hopefully serving the interests of good analytical philosophy the world over.

Madrid, 31-12-1996

L.P.

ABSTRACTS OF THE PAPERS

THE VALIDITY OF INDEXICAL ARGUMENTS

S.H. Elkatip

The paper is concerned with the validity of the first version of indexical arguments as put forth in «'He': A Study in the Logic of Self-Consciousness» in 1966 and is in defence of the view that logical structure of statements containing personal pronouns alone does not account for personal identity. Castañeda's 1966 analysis does not establish that the S-use characterises some usages of the personal pronoun better than the F-use or the E-use. While the major problem with F-use, which involves *de re* belief, is its conflict with the doctrine of propositions as transmitted from Frege, Castañeda's rejection of body or E-use is based on common sense. But, the argument against E-use has no persuasive force against physicalism. It is, also, absurd to maintain that persons could speak an actual language or produce actual sentences the logic of which Castañeda claims to study objectively without bodies.

♣ ♣ ♣ ♣ ♣ ♣ ♣

VAN INWAGEN AND GUNK: A RESPONSE TO SIDER

Kelly J. Salsbery

In a recent article, Theodore Sider raises an interesting objection to some of the ontological views of Peter van Inwagen. In van Inwagen's view, all material things are either mereological atoms or living things composed of

such mereological atoms. Sider claims that it is possible for there to be worlds at which matter consists of atomless gunk. He argues that the possible existence of atomless gunk undermines van Inwagen's claims (along with any sort of atomism). I argue that the possible existence of atomless gunk does not undermine van Inwagen's position, and that Sider's claims concerning gunk are unwarranted.

♣ ♣ ♣ ♣ ♣ ♣ ♣

Graham Priest's "Dialectheism" — Is It Althogether True?

Lorenzo Peña

Graham Priest's book *In Contradiction* is a bold defense of the existence of true contradictions. Although Priest's case is impressive, and many of his arguments are correct, his approach is not the only one allowing for true contradictions. As against Priest's, there is at least one contradictorialist approach which establishes a link between true contradictions and degrees of truth. All in all, such an alternative is more conservative, closer to mainstream analytical philosophy. The two approaches differ as regards the floodgate problem. Priest espouses a confinement policy banning contradictions except in a few special domains, particularly those of pure semantics and set-theory (and perhaps arithmetics), whereas the alternative approach admits two negations — natural or weak negation and strong negation, the latter being classical; accordingly, the alternative approach prohibits any contradiction involving strong negation, thus providing a syntactic test of what contradictions have to be rejected.

THE VALIDITY OF INDEXICAL ARGUMENTS

S.H. Elkatip

My concern is mainly the validity of Castañeda's exposition of indexical arguments and particularly its first version as put forth in «'He': A Study in the Logic of Self-Consciousness» in 1966.[1] The following two or three years witnessed other papers by Castañeda on the issue. In 1977 he returned to the topic again with «Perception, Belief, and the Structure of Physical Objects and Consciousness», a paper that «extends Guise Theory to perception»[2]

The difference between the earliest 1966 paper and the later 1977 one is that in the latter there are references to Kant and the transcendental deduction while in the former there virtually are none, or, if there is a reference, its tone is very dissimilar[3] In the 1966 paper «'He'» (for short) there is a single reference to Kant and that associates him with Hume: «philosophers (especially Hume and Kant) have known all along, that there is no object of experience that one could perceive as the self that is doing the perceiving»[4] In comparison, «Physical Objects and Consciousness» (for short) ends with an ovation to Kant and «the so-called egocentric predicament, which, to put it in a Kantianesque way, consists in that all judgments and all perceptual fields lie within an implicit *I think*»[5]

---

[1]   Some of the numerous drafts of this paper profited from the opinions of Professors Adam Morton and C.J.F. Williams, University of Bristol, the editors of the *Canadian Journal of Philosophy* and suggestions by an anonymous referee of *Sorites*. It was revised thanks to discussions on indexical arguments at ECAP II in Leeds in relation to the draft of another paper on indexicality presented there.

[2]   According to the description given in James E. Tomberlin (ed.) [1986], p. 414.

[3]   pp. 308, 345.

[4]   . 142.

[5]   . 347.

In 1977, Castañeda seems to suggest that Kantian philosophy, disagreeing with Humean philosophy which leaves valid knowledge of things such as the self to the imagination, guarantees «the awareness of experiences as part of one consciousness and as having an object, although neither the owner nor the object of those experiences can be found in the experiences as such»[6] There were intimations of the course of development of Castañeda's 1966 indexical argument already in 1967 in «Indicators and Quasi-Indicators» where he begins talking about «Descartes' *Cogito* and Kant's theses on the transcendental self» and «the fundamental fact underlying the idea of the transcendental self» in the first section[7]

Doubts

Castañeda acknowledges distinctions among «external», «internal» and «transcendental» sorts of knowledge in a reply, in the Tomberlin volume of 1986; to Boër and Lycan's criticism he retorts in the replies section:

> At that level, as Descartes, Leibniz, and Kant knew very well, the sceptic is right: all the claims to know are false, except for the *cogito* claims. We must distinguish a transcendent species of knowledge, from an internal one; the transcendent species of knowledge is a lofty one where nothing more than logical deduction and, perhaps, semantic know-how and perceptual powers are presupposed.[8]

Looking back at his work, Castañeda might have viewed it all as Kantian. His preoccupation with «physical space-time» and «visual space-time» as he essays to formulate the «most general properties of visual space» in «Physical Objects and Consciousness» in 1977 may be interpreted as his search for synthetic a priori knowledge. The 1966 indexical argument, however, as I noted above, does not even pay lip service to Kant: its only reference to Kant takes him to endorse Hume on the self, and if knowledge of selves is neither an external sort of knowledge nor transcendent -, then it remains that it is an internal sort of knowledge.

On the one hand, the argument of 1966 for S-used 'he' (or 'he*') seems to boil down to a projection from a first person point of view to a third person one. While in the first person, it is noted that all references to me are interpreted by me in terms of the first person even if heard or read as third person references. From this observation it is then inferred that all third person references must be interpreted as «referring» to some 'I' in the first person.

---

[6]   p. 29, 30, D.W. Hamlyn [1967].

[7]   . 87.

[8]   p. 353.

On the other hand, the argument of 1966 appears to be a purely logical argument. Firstly (i) there is the group of sentences borrowed from Geach:

«a=b and b believes that b is F.» entails «a believes that b is F.» «a=b and b believes that b is F.» does not entail «a believes that a is F.» «a=b and b believes that he himself is F.» entails «a believes that he himself is F.» Castañeda credits Geach with the discovery of the logic of the S-use of the pronoun 'he'.

Substitutivity in belief contexts provides another group of sentences (ii) concerning the meaning of personal pronouns. Given that Smith is the editor of *Soul*, and Smith believes that he is a millionaire, substituting 'Smith' or 'the editor of *Soul*' for 'he' will obtain for the whole statement different truth values on different interpretations produced by different scenarios about Smith's beliefs about himself: Does he believe that he is Smith? Does he believe that he is the editor of *Soul*? What is up for grabs here is the doctrine of propositions as held by Frege[9] The applicability of the sense and reference distinction to personal pronouns also comes into question.

And, thirdly (iii) there are the Hintikka sentences: «The man who is in fact a knows that he is a.» «There is a person known to a such that a knows that such a person is a.» By removing Hintikka's restriction that the universe of discourse be limited to persons known to a, Castañeda claims to have improved Hintikka's calculus. He quantifies directly over persons in the universe of discourse, whether they are known to a or not. Having removed the restriction, Castañeda's calculus enables him to write:

$$(\exists x_a)(Ka(x = a) \ \& \ \neg Ka(x = \text{himself}),$$

where 'himself' has a as its logical antecedent.

The domain is now populated with human persons from the outset. Quantification is not over entities known to a, as for Hintikka, and the values of the variables are not metaphysically neutral entities for Castañeda: computers, monkeys, people, angels which interest us only because of the logical properties of their linguistic productions[10]

Castañeda was after a logical restriction on the set of sentences he wished to consider. In his 1966 paper he appeals to the logical grammar of sentences with occurrences of the singular first and third person pronouns and formulates four laws, claiming they provide «an exhaustive discussion of, and a rigorous treatment of the logic of, the S-used third-person

---

[9]   see pp. 5, 9, John Perry [1979].

[10]   p. 136.

pronoun»[11] The argument relies on a fork for the meaning of personal pronouns. Its first prong is the replaceability of a personal pronoun by a name or description[12] The second is its replaceability by a demonstrative. Castañeda discusses the fork in detail as he goes through the seven usages of the personal pronouns enumerated, in Castañeda style, from «A» to «G». In conclusion 'he*' is distinguished from A to G uses in being baptised as the use of self-consciousness or the S-use.

Castañeda could not choose the sentences to be studied for their logical properties by assessing their meanings. If he did that, then have «I am a composite of a self and a body: I am a human person!», while there is considerable philosophical opposition insisting «No, you are just a very complex body, for all that is known.» Castañeda had to leave out the act of self reference accomplished in a simple sentence like «I am Napoleon.» or «I am a monkey.» These sentences also contain pronouns; why exclude them? Apparently because, granted that Castañeda was arguing for the thesis that all self-reference is reference to a self, one may still claim that one is not a self or a person and contradict his thesis by saying «I am not a person.»

In «On the Phenomeno-Logic of the I» and in «Indicators and Quasi-indicators», Castañeda argues that «I exist.» is necessarily true[13] But «I am a person.» is not discussed. It is excluded from the 1966 discussion not by means of its necessary truth but by two devices which limit the argument to *oratio obliqua* statements. Castañeda's devices look deceptively like purely syntactical constraints.

His first device is to take an equalitarian approach to «cognitive» verbs and prefixes. The terminology of Latin grammar is implemented by new Castañeda definitions for cognitive verbs and cognitive prefixes.[14] The first allows 'to say' to be a cognitive verb. It defines assertive and quasi-assertive verbs to be cognitive along with verbs which express cognitive acts, attitudes or dispositions. The second allows assertive or quasi-assertive verbs to form cognitive prefixes and such cognitive prefixes to function in the main clause for a sentence in *oratio obliqua*.

Castañeda introduces in addition «an epistemological priority» thesis in [1966]. His cognitive verb and prefix definitions permit treating a speech act as a mental act. The device of the cognitive prefix restriction, strengthened by the epistemological priority device limits the set of

---

[11]   p. 131.

[12]   p. 139.

[13]   p. 166 and p. 87 respectively.

[14]   p. 131, 148 in [1966].

sentences Castañeda is prepared to consider to sentences all of which are prefixed with some token of the cognitive prefix «to say». All such *oratio obliqua* sentences in the first person imply no more than that there is a «sayer» or a «speaker».

The *oratio obliqua* restriction, armed with the cognitive prefix definition and the epistemological priority thesis, successfully circumvents the trouble of addressing sentences like «I am not a person.» and «I am an evolved animal.» which are not evidently contradictory. Allowing such sentences to stay within the set to be explored would be an invitation to an empirical investigation, not a logical discussion, and limiting the set of sentences to be studied thus by the grammatical category of *oratio obliqua* with the two devices neatly encompasses the Geach group of sentences (i) and two other groups of sentences bearing personal pronouns, (ii) and (iii).

The preliminary doubts which lead to the consideration that the 1966 argument was probably invalid are further entrenched by the persistent confusion of psychological matters with matters of logical analysability. On the one hand, the argument sets out to be a logical study of statements containing tokens of personal pronouns, but, on the other hand, it is a study of knowledge of selves or persons. The logical aspect is made more prominent:

> My major contentions are: (a) that the S-uses of 'he' are quite different from the other uses of the third-person pronoun; (b) that the S-uses of 'he' constitute the employment of a *unique logical category*, which is not analyzable in terms of any other type of referring mechanism»[15]

On the one hand it argues that there is a use of personal pronouns which is irreplaceable by demonstratives or definite or indefinite descriptions, but, on the other hand, it interjects that it is replaceable by the I». Psychological matters play an important role in the claim that S-use has «purely demonstrative reference»:

> Thus, we conclude that *the pronoun 'he*' is never replaceable by a name or a description not containing tokens of 'he*'. This suggests that 'he*' is a purely referential word.*[16]

> In sum, the S-uses of 'he' or 'he himself', that is, *the uses of the pronoun 'he*' can not be analyzed in terms of the demonstrative reference of the strictly third-person pronoun 'he'*. The only demonstrative reference of 'he*' is bound up with that pertaining to the first-person pronoun 'I'.[17]

Castañeda, rejecting on the one hand replaceability by names, demonstratives and descriptions, nevertheless is advocating that 'he himself'

---

[15]   p. 131.

[16]   [16]   p. 139, italics his.

[17]   p. 144, italics his.

must have a demonstrative reference «bound up» with the first person 'I', a «pure reference».

Even the illustrations given in the first introductory paragraph of the 1966 article are suggestive of the ongoing confusion: «We say, e.g., 'He believes (knows, says, argues, claims) that he (himself) is healthy (rich, tall, heavy, Napoleon, a victim)'». If a mentally unhealthy man announces that he himself is Napoleon or that he is a victim, is he to be taken seriously as Napoleon or as a victim? Castañeda continues: «This use of 'he' (to be called the *S-use* of 'he') as a pointer to the object of someone's self-knowledge [,] self-belief, self-conjecture, is the main topic of this study».

Inconsistencies

Castañeda was not just using the predicate calculus of first order logic to re-write sentences containing personal pronouns. He is after a pronoun which is «ineliminable» for a person:

> Privatus cannot remember, or merely consider later on, that he* is , unless he remembers, or merely considers, what he would formulate by saying «I am» or «Privatus is and I am Privatus.» At least the statements of identity «I am Privatus» or «I am the one who» must include an ineliminable use of 'I' for Privatus.[18]

«Ineliminability for the person» is just the thesis of «epistemological priority» renamed. A most objectionable notion in this argument is «eliminability». Eliminability *simpliciter* is the notion that an S-use of 'he' is bound up with an S-use of 'I': that they are correlatives which are eliminable into one another in terms of their meanings. The notion appears again later in John Perry's 1979 paper[19]

Eliminability is roughly implication, entailment, analysis or translation[20] and, for Castañeda, the following obtain: Strict eliminability implies eliminability *simpliciter*, but its converse does not hold[21] I call these «* theses» or just «*». They are crucial for Castañeda's discussion of 'he*'.

Thus, Castañeda has divided the notion of eliminability into two sorts. Strict eliminability consists in being eliminable by something other than a token of an S-use of 'he', or, when appropriate, other than a use of e I». Eliminability *simpliciter* of a token of a personal pronoun is its analysis into

---

[18]   p. 145.

[19]   p. 16.

[20]   cf. p. 88 in [1967]: «*The references made by an indicator other than 'I' are ephemeral, and necessarily eliminable for those who make them*' Elimination is here a process of preserving information, not a process of analysis or of literal translation». (emphasis his)

[21]   p. 148.

its correlate[22] It is Castañeda's view that the S use of 'he' is the correlate of a use of 'I'. He claims that «the only demonstrative reference» of an S-use of 'he' is «bound up with that pertaining to the first-person pronoun 'I'»[23]

Paraphrasing the four laws of S-use in [1966], they read as:

(a) If 'I' is ineliminable for the person it refers to in *oratio obliqua,* then it can be correlated with another person's ineliminable use of an S-use of 'he'.

(b) Every sentence which contains a token of S-use of 'he' and which is not in *oratio obliqua* is an incomplete sentence or clause.

(c) From a statement of the form «X knows that p» we can not infer the corresponding statement that p, if p is expressible in sentences containing an S-use of 'he'.

(d) A token of an S-use of 'he' is not strictly ineliminable in any case other than its subordination to a proximate or approximate antecedent of the token occurring in the main clause of a sentence in *oratio obliqua.*

I call (a) and (d) «the laws of eliminability *simpliciter*» or «the access laws» (following John Perry's use of «access» in his 1979 paper). (a) says that somehow, i.e., in terms of pure demonstrative reference, 'I' and 'he' may concur. (d) says that if 'he' occurs in the subordinate clause (with one or more prefixes all having the same antecedent) of an *oratio obliqua* sentence, then it is strictly ineliminable. But note that by * strict ineliminability may involve eliminability *simpliciter*. The implication is not annulled in that direction. There are, not counting «ineliminability for the person» because that turns out to be the epistemological priority thesis, (at least) four items involving eliminability: eliminability *simpliciter*, ineliminability *simpliciter*, strict eliminability and strict ineliminability.

(b) says, if an S-use of 'he' occurs in the subordinate clause of a sentence (with one or more prefixes all having the same antecedent), then it is not eliminable even *simpliciter* because it is incomplete in a very significant way. It is ineliminable into an S-use of 'I'. (b) and (c) together stress that an S-use of the third person singular pronoun in a subordinate clause can not be disentangled from its antecedent. (b) and (c) constitute the laws of ineliminability, since, by *, ineliminability *simpliciter* implies strict ineliminability. Strict eliminability and eliminability *simpliciter* do not constitute disjoint sets.

Castañeda's third law (c) captures the importance of the cognitive verb «to know» and his preoccupation with knowing the nature of the self in psychological terms; this is not merely a logical inquiry. It now emerges that «to know» is not on a par with «to say», in spite of the cognitive verb

---

[22]   p. 148.

[23]   p. 144.

and cognitive prefix stipulations which seem to allow replacing all cognitive verbs with the weakest one of saying.

There are two explanations for (c). On the one hand, psychologically speaking, it is not communally known how each person knows himself, or, on the other hand, the personal pronoun in a dependent clause of a propositional attitude statement expressing self-knowledge can not be replaced by an expression belonging to any other category because it is impossible that there is such a replacement exclusively for logical reasons. However, the former should not be the reason for the latter.

In each case, supplying an appropriate correlate as in the first instance, the sentences below, or sentences resulting from substitutions, all contain personal pronouns:

(1) I ___.

(1') He ___.

(2) I say that I ___.

(3) I think that I ___.

(4) I believe/perceive/know/feel/entertain/cognise that I ___.

(5) I dream that I am back in Spain.

(6) I know that I am in pain.

(7) I know that I am back in Spain.

(b) and (c) prohibit passing from (6') or (7') to (1'). (a) permits passing from (4) to (4'). (d) permits passing from (4') to (4). Sentences in the form of (6') or (7') are entailed by and entail sentences in the form of (6) and (7). The restriction Castañeda falls back on is denying that a sentence in the form of (6') or (7') implies a sentence in the form of (1') but, he is not clear on the entailment of (1) from (6) or (7).

(4), (6) and (7) are similar to (2) in form and could be traded for it in accordance with the cognitive verbs and prefixes stipulations. And, furthermore, some instances of (2) probably do translate into instances of (1), in cases of naming, promising, marrying for instance, although (1) does not always entail (2). If (6) can be exchanged with its counterpart in the form of (2) and (2) with (1), then (1) entails (1') and the result clashes with laws (b) and (c) of S-use.

Although, by the epistemological priority thesis, Privatus is permitted to get (1) from (1'), Castañeda tries to bar, say, Gaskon from getting (1') from (1). In [1966] Castañeda writes, «But the epistemological priority of the demonstrative 'I' is only partial. Everybody else must replace a person's references to himself in terms of 'I' (me, my, mine, myself) by references in terms of some description or name of the person in question». But, why?

If from (1) there is an inference to (1'), there is a formal contradiction in S-logic and the inference clashes with laws (b) and (c).

Doctrinal changes

In the shortest possible *oratio obliqua* sentence the possible alternatives for 'I' and 'he' would be as follows:

p1. I ___ that I ___.

p2. He ___ that I ___.

p3. I ___ that he ___.

p4. He ___ that he ___.

According to Castañeda in [1966], in p2 there is not any S-use and in p3 'he' is not in S-use, but 'I' is. So is the first token of 'I' in p1. Not all uses of 'I' in *oratio obliqua* are S-uses, but, presumably, all uses in *oratio recta* would be.

However, there is a troublesome point, that is, why the token of 'I' in p2 is not in S-use. In [1966] Castañeda announces that «The whole thing is simply that the only ineliminable tokens of 'I' for the user of 'I' are (I) those occurring in *oratio recta* and (2) those in *oratio obliqua* subordinated only to prefixes of the form 'I E that»[24] There would be no pride or embarrassment involved in a sentence in the form of «He thinks that I ___ !» unless what was at stake was more than one of the unessential ways of talking about the referent of 'I' available to the referent of 'he'[25] It could be like finding out that sugar was spilling from your shopping cart, to use Perry's example. The sugar spilling test (or the burning trousers test) qualifies p2 just as well and raises the trustworthiness of asking «What would you do if it were you?» as a test for indexicality.

Secondly, in [1966] Castañeda claims that the pronoun in «Privatus is and I am Privatus.» is an «implicit 'I'»:

> In order to analyze in detail the connection between a use of 'he*' and an implicit use of 'I', we need some grasp of the logic of 'I'. This does not mean, of course, that whenever, e.g., Privatus hears «Privatus is …» he is to perform a physically, or psychologically, distinguishable act of translation: «That is, I am…» The point is a logical one: If he only entertains or thinks the statements, without actually making any assertion, we shall speak of his making an implicit use of 'I'[26]

Privatus knows an implicit logical language. The task is logical; neither Cartesian nor Kantian.

---

[24] pp. 150, 151.

[25] cf. pp. 146, 147 [1966].

[26] pp. 144, 145.

There are doctrinal shifts on the logic of S-use from 1966 to 1969 both about the nature of p2 above[27] and about the modalities of statements and propositions with S-used pronouns. In the context of modalities it is relevant to bring up Castañeda's evaluation of a view defended by Carl Ginet. Castañeda's response is to reject it and to criticise it because «this analysis of 'he*' in terms of 'I' is at bottom circular»[28] Castañeda complains that Ginet does not make clear the proposition a person believes when he refers to himself; sentences in the form of (1) do not express propositions. Is the answer that the S-use of a pronoun is always subordinate in accord with laws (b) and (c)? Yes, this seems to be Castañeda's answer, but it does not resolve the present inconsistency with the epistemological priority thesis according to which Castañeda depicts Privatus doing a logical translation. If Privatus' implicit logical statement in the form of (1) was explicitly stated, then it was not implicit. But, if it was implicit, then Castañeda, and only Castañeda, knows both the form of the sentence into which Privatus translates things about himself, and that Privatus' translation expresses a complete proposition. Therefore, Castañeda's account is also somewhat circular[29]

Castañeda must opt for the first horn of the dilemma, avoid the second one, and go against his own remarks against Ginet's analysis:

> There is also the fact that «X believes that He* is H» does not entail that there are any sentences or that «I am H» is a sentence in some language, or that 'I' is a word. But Ginet's analysans does require that «I am H» be a sentence and 'I' be a word in some language[30]

Castañeda's progress on this problem becomes explicit in the 1969 paper in which he argues that «The fact that the first-person pronoun has always largest scope has as its immediate consequence that certain propositions cannot be asserted by anyone[31], in comparison to the 1967

---

[27]   In [1967] Castañeda considers an interpretation that takes 'I' in p2 to be indexical; see pp. 89-93.

[28]   pp. 143, 144 [1966].

[29]   It is also possible that Privatus does not make any sounds or written marks since in his apparently exhaustive exploration from (A) to (G) of candidates for replaceability Castañeda convincingly argues that an S-use is not to be replaced by expressions denoting bodies. It is possible that he just can not make explicit statements.

[30]   pp. 143, 144 [1966].

[31]   p. 166.

paper in which he declares that «a person's statements of the form 'I am not-a-self' are contingent»[32]

Later evaluations

C.J.F. Williams follows Castañeda in inferring (1) from (2): «The use of 'I' indicates that the person who utters it is the very same person as the subject of what is said»[33] He stresses that this inference is not the converse one of deriving (2) from (1): «When Rosie says 'I am so-and-so' she does not say or imply that she says of herself that she is so-and so. She says nothing at all about saying[34]; but, one of deriving a meaningful use of 'I' from an act of saying: 'A person's meaningful use of 'I' is a sufficient condition, but not a necessary condition, for her having said something about herself»[35]

Williams argues that «the concept of personal identity is not wholly an empirical concept[36], but he disagrees with Castañeda, in «Myself,» about the entailment relation of sentences falling into group (ii):

(3') The editor of *Soul* thinks that he is a rich man.

(3²) The editor of *Soul* thinks that the editor of *Soul* is a rich man.

By the inferential relations claimed by Castañeda and discussed above, (3') is not eliminable for the person of the editor of *Soul* and it is eliminable *simpliciter* into 'I'. In short, (3') entails its substitution instance of (3). For Castañeda, again by the same inferential relations, (3²) does not entail (3). Williams holds that (3²) does not entail (3'). He rewrites (3') as «There is just one man who is editor of *Soul* and the editor of *Soul* thinks that that man is rich.» and argues that (3') as such now implies (3²).

Whether (3') entails (3²) or not is a scope distinction between primary occurrence where the entailment holds and secondary occurrence where it does not. Secondly, Williams denies that (3') entails its substitution instance of (3). Consequently, he claims that logically (3') is prior to (3) and, since (3') rewritten as primary occurrence entails (3²), (3') is prior to (3) and (3²). Strawson's epistemological asymmetry in respect to (3') and (3), he maintains, is matched by a syntactical asymmetry explaining why both forms of statements are related to a no place predicate form, possibly like «Rich!», similar to «It's raining!» or «It hurts!»

---

[32] p. 87.

[33] p. 78 [1991a].

[34] p. 77 [1991a].

[35] p. 79 [1991a].

[36] p. 195 [1991b].

Morton's example in «Why there is no Concept of a Person» can be added to the list of cognitive verbs in (4'):

(4) He wants that he ___.

His characters Hyperrabbit and Fred are alike because they can say sentences in the form of (4') but they cannot make an inference from (4') to (4) or vice versa. The Hyperrabit, in addition, can not infer (1) from (1') or vice versa. If Fred occasionally infers (1) and (1') from each other, his Korsakoff's psychosis prevents him from making historical inferences about himself. If Fred was more intelligent, imagines Adam Morton, then he, call him «Hyperfred», would learn to infer from (4') to (4). However, Hyperfred can not derive (1) from (4). Note that Castañeda does this through his cognitive verb and cognitive prefix definitions, i.e., via (2). Hyperrabit and Hyperfred are thought experiments demonstrating that it is possible to imagine states of affairs in which inferences with personal pronouns break down. Fred's case is medical and thus real. Adam Morton points out that Fred is similar to the rabbit in his minimalised ability to conceptualise the meaning of an S-used pronoun, but yet dissimilar to her, for he is not just a thought experiment.

Conclusion

One of the possible choices for the meaning of the third person singular pronoun is that it is a proxy for a demonstrative. It could also behave like a relative pronoun or a variable of quantified logic and Castañeda takes that into account in uses D, C and G. For the demonstrative use there are three cases: A-use for «that man», «that woman» and so forth; B-use for 'this' or 'that'; and, E or body use for 'this body'. A-use and B-use are discarded quickly by Castañeda on the grounds that pronouns can refer even when it is possible that the referent is not ostensively within the experience of the speaker or of the hearer.

F-use explains self-reference to take place through a concept, which is known to the referring person, but unknown to all the others. Castañeda and later Perry find this account troublesome, since the presence of a concept correlated with the sense of a pronoun or referring expression in the that-clause is shown to be impossible by (i) and (ii). The logical relations ascertained by Geach would then break down. It will not be entailed that, given a=b and a knows that he himself is F, b knows that he himself is F.

F-use according to Castañeda is *de re* belief for Perry: Perry thinks that *de re* belief may be «interesting»[37] and Castañeda finds them «intriguing»[38]. The major problem with F-use is its conflict with the doctrine

---

[37]   p. 11, John Perry [1979].

[38]   p. 150 [1966].

of propositions. By (ii) clearly an S-used pronoun can not be a place-holder for a description because descriptions involve concepts.

Both the E-use and the F-use are rivals for the S-use. In [1966] Castañeda's rejection of E-use is based on common sense: «It is extremely doubtful that the Editor of *Soul* or Privatus or Gaskon think of a mere body as a millionaire. But even if they all did, we may suppose that in this case they all think of persons. Thus, Privatus' use of 'he*' is not a proxy for 'this (that) body'»[39] According to Castañeda one knows about one's body through «kinaesthetic sensations, pains, itches, etc., in that body» and there could be moments in which all bodily sensations cease[40] In this respect, the E-use seems similar to the uses in A and B and therefore may be eliminable for the person.

The rejection of E-use has no persuasive force against a proponent of physicalism and it is inconsistent to maintain that persons could speak an actual language or produce the actual sentences the logic of which we are studying, with Castañeda, without having their bodies. Unless thoughts are expressed, the two laws of S-use (b) and (c) can not be applied, since they pronounce a verdict on what counts as a complete thought and thus a proposition and what as an incomplete thought.

Castañeda promises a series of logical studies on the logic of S-use in [1966], but the analysis does not establish that the S-use characterises some usages of the personal pronoun better than the F-use or the E-use. It begs the question by harbouring presuppositions about the nature of a human person. And, it is unclear, because it delineates the set of sentences to be studied for their logical properties by restrictions which are not purely logical.

Suppose as a thought experiment that «a» and «b» are the names of two different halves of brains of two different persons living in the same body after their transplantation for some medical and ethical reasons; then the first and third Geach entailment would fail to hold as well, like the second one, for situations of perceiving, believing, knowing, etc. in which a and b, halves of different brains, do not establish communication. If a is transplanted in c's body and so is b, there will be a person a+b+c and that is numerically identical with itself. But, there will be a sense in which a+c will know things which b+c will not know[41] In Geach's sentences the

---

[39]   p. 134 [1966]; the 1966 example of weight (p.152) finds its way into the 1969 paper (p. 164).

[40]   Hinshelwood discusses analogous cases to this thought experiment, which are «social» relocations of personal identity, in [1995].

referents of 'a' and 'b' are curious things: they have numerical identity and can entertain cognitive structures. If brain halves a and b co-operate to some degree, they would count as one person. We could call this person «a's derivative» or equally «b's derivative» and they would now be the same human person after the transplant operation, but what they know, experience, desire, believe, remember, etc., are, at least for a while, quite different. Logical structure of statements containing personal pronouns does not account for personal identity.

References

Braude, Stephen E.[1995]: «Commentary on 'The Social Relocation of Personal Identity'», *Philosophy, Psychiatry, & Psychology*, 2, **3**, 1995, 205-209.

Cassam, Quassim (ed.) [1994]: *Self-Knowledge*, Oxford University Press.

Castañeda, H.-N. [1966]: «'He': A Study in the Logic of Self-Consciousness», *Ratio*, **VIII**, 130-157.

Castañeda, H.-N. [1967a]: «Indicators and Quasi-Indicators», *American Philosophical Quarterly*, **4**, 85-100.

Castañeda, H.-N. [1968]: «On the Logic of Attributions of Self-Knowledge to Others», *Journal of Philosophy*, **65**, 1968, 439-456; I wonder if it is significant that the title of this paper appears as slightly changed to be about attributions of «Self-reference» in Landini's bibliography in the Tomberlin work [1986].

Castañeda, H.-N. [1967b]: «On the Logic of Self-Knowledge», *Nous*, **1**, 1967b, 19-21.

Castañeda, H.-N. [1968]: «On the Phenomeno-Logic of the I» in Quassim Cassam (ed.) [1994]; according to a footnote this paper was first published in 1969, but according to Gregory Landini's philosophical bibliography in Tomberlin's [1986] it was published in 1968.

Hamlyn, D.W. [1967]: «Epistemology, History of», *Encyclopedia of Philosophy*, vol. 3, 8-38.

Hinshelwood, R.D. [1995]: «The Social Relocation of Personal Identity as Shown by Psychoanalytic Observations of Splitting, Projection and Introjection», *Philosophy, Psychiatry, & Psychology*, 2, **3**, 1995, 185-205.

Lewis, D. [1979]: «Attitudes *De Dicto* and *De Se*», *Philosophical Review*, **88**, 513-43.

Mellor, D.H. [1991]: *Matters of Metaphysics*, Cambridge University Press.

Morton, Adam [1990]: «Why There is no Concept of a Person» in *The Person and the Human Mind*, C. Gill (ed.), Oxford University Press.

Perry, John [1979]: «The Essential Indexical», *Nous*, **13**, 3-21.

Rorty, Amelie Oksenberg (ed.) [1976]: *The Identities of Persons*, Berkeley, University of California Press.

Strawson, P. F. [1959]: *Individuals*, London, Methuen.

Tomberlin, James E. (ed.) [1986]: *Hector-Neri Castañeda,* Dordrecht, D. Reidel.

Wiggins, D. [1976]: «Locke, Butler and the stream of consciousness: and men as a natural kind» in Amelie Oksenberg Rorty (ed.) [1976].

Wiggins, D. [1980]: *Sameness and Substance*, Oxford, Blackwell.

Williams, C.J.F. [1991a]: «Myself», *Ratio*, New Series **4**, 76-89.

Williams, C.J.F. [1991b]: «On Sameness and Selfhood» in *Peter Geach: Philosophical Encounters*, Harry A. Lewis (ed.), Dordrecht, Kluwer Academic.

S.H. Elkatip

University of Bristol

<S.H.Elkatip@bristol.ac.uk>

VAN INWAGEN AND GUNK: A RESPONSE TO SIDER

Kelly J. Salsbery

In a recent article (Sider [1993]), Theodore Sider raises an interesting objection to the ontological views of Peter van Inwagen (van Inwagen [1990]). He attributes to van Inwagen the following two theses:

> (1) For any material objects X, the Xs compose something iff the activity of the Xs constitutes a life, or there is only one of the Xs.

> (2) Every material object is either a mereological atom or a living thing.

> Sider notes that here 'mereological atom' means «an object lacking proper parts» (p. 285). He also notes that (2) seems to follow from (1).

> Sider's objection to van Inwagen's approach is based on the claim that, «there are (or rather might have been) situations in which 'objects' like tables and chairs are not composed of fundamental particles» (p. 286). He suggests that an alternative would be to posit material objects composed of 'atomless gunk'. Sider borrows this term from David Lewis (See Lewis [1991], p. 20). He notes that such atomless gunk would have no mereological atoms (or simples) as parts and would be infinitely divisible (p. 286).

> Sider asks us to imagine possible worlds at which there is only gunk. What he calls a «gunk world» would be a possible world at which there exist no mereological atoms (and for the sake of simplicity, no living things). If (2) is true, however, then the only things which exist are mereological atoms or living things. Thus, no material objects would exist at gunk worlds.

> Sider finds such a claim implausible. Concerning this he writes:

>> Surely there is a gunk world in which some gunk is shaped into a giant sphere, and another where some gunk has the shape of a cube. Surely, there are gunk worlds that most of us would describe as containing objects much like objects from our world: tables and chairs, mountains and molehills, etc. (p. 286).

Sider thinks that plausibility of the existence of gunk worlds undermines van Inwagen's position because he believes that van Inwagen holds (2) to be necessarily true (p. 287). Sider's argument goes as follows:

(p1) If (2) is necessarily true, then gunk worlds at which material objects exist are impossible.

(p2) Gunk worlds at which material objects exist are not impossible.

Thus, it is not the case that (2) is necessarily true.

Sider further claims that such a conclusion undermines van Inwagen's approach because this approach fails to account for the existence of gunk worlds.

Sider's argument is clearly valid, but it is not so clear that it is sound. Premise p1 seems obviously true. If it is necessarily true that every material object is either a mereological atom or a living thing, then there cannot be world at which a material object is composed of gunk.

Premise p2, however, is somewhat problematic. Sider supports p2 mainly on the basis of imaginability or conceivability (as noted above). That is, Sider thinks that the fact that we seem to be able to imagine a world at which there exist objects composed of gunk supports the claim that such a possible world exists. Consequently, he thinks it is implausible to deny the existence of material objects at such worlds.

Such a response, however, very nearly begs the question against van Inwagen's approach. One might equally well claim that one can imagine lifeless possible worlds at which there seem to be objects such as rocks, mountains, etc. composed of mereological atoms. On van Inwagen's view, this would be a mistake. Surely there seem to be rocks and so forth at such a world, but really there are only mereological atoms arranged in certain ways. Van Inwagen describes how mereological atoms may come to compose a living thing, but he denies that there are any non-living composite objects. This is the crux of the controversy surrounding his view. Claiming that the picture it presents seems implausible is not enough.

Even if we admit the existence of gunk worlds, it does not follow that we must admit the existence of material objects at these worlds. There may be worlds at which there is gunk, but we need not admit that this gunk composes anything. It seems to compose some objects, but we are simply mistaken if we think this is the case. Further, since the gunk is infinitely divisible, there are no mereological atoms at such worlds. Thus, no material objects exist at such worlds. We need more than Sider's insistence here in order to claim that they do.

We can raise some other questions for Sider. For instance, what exactly is it for an object to be composed of gunk? I cannot reproduce it here, but van Inwagen gives us a rather detailed account of what it is for

mereological atoms to compose living things (and why it is that composition fails for other putative objects). Roughly, it involves objects acting in such a way that they constitute the «life» of some living thing (van Inwagen [1990], pp. 81-82). Sider does not give such an account for gunk. We are left to imagine that gunk comes to compose an object in a way similar to mereological atoms. But why should we suppose that this is the case?

On the contrary, we seem to have good reasons for doubting that this is the case. As van Inwagen notes:

> I assume that every material thing is composed of things that have no proper parts: «elementary particles» or «mereological atoms» or «metaphysical simples.» I suppose that questions about whether two objects are composed of or constituted by the same «quantity» or «parcel» of matter — or «the same matter» *tout court* — make sense only in the case of composite objects, and that in that case these questions must be understood as asking whether the composite objects are composed of the same ultimate parts. Thus, in my view, there is no notion of sameness of *matter* that is prior to or independent of the notion of sameness of *objects* (van Inwagen [1990], p. 5).

If van Inwagen is right about this, it is not clear how Sider can address the notion of sameness of matter in the case of gunk. Moreover, even if we grant that gunk could come to compose objects in a way similar to that of mereological atoms, this would not yield the result that tables and chairs exist at some gunk world. If matter in the form of mereological atoms cannot come to compose any non-living thing (as claimed by van Inwagen) why should we suppose that gunk can? Again, Sider needs to give an account of gunk that answers what van Inwagen calls the «Special Composition Question» (see especially van Inwagen [1990], pp. 20-22 and pp. 30-31). That is (roughly), what conditions must an object or objects satisfy in order to compose something? Sider does nothing either directly to undermine van Inwagen's treatment of composition or to give an alternative account of his own.

Sider finds it particularly problematic that van Inwagen's approach provides for so-called «virtual objects» at worlds where there are mereological atoms, but not at gunk worlds. That is, instead of claiming that the sentence:

(s1) There is a table here,

is false, van Inwagen comes up with a paraphrase that preserves the truth of the sentence. He would paraphrase the sentence s1 by a sentence such as:

(s2) There are Xs arranged tablewise here.

Strictly speaking then, there are no tables. In van Inwagen's terms, the furniture of the world has simply been rearranged without any addition (van Inwagen [1990], p. 124). Sider claims that van Inwagen's approach is unable to supply a paraphrase and hence, is unable to supply a virtual object in the case of objects at a gunk world. Thus, he concludes that there is no

way to eliminate commitment to composite objects at gunk worlds because van Inwagen's paraphrase in terms of simples fails at such worlds (p. 287).

It is not entirely clear, however, what exactly Sider finds so problematic about this feature of van Inwagen's approach. Perhaps it is because sentences such as s1 would turn out to be false (while at a world where some atoms are arranged tablewise here, the sentence would be true). Thus, at some gunk world, all our utterances concerning objects such as tables, chairs, rocks and so forth would be false. Surely this is an unintuitive consequence of van Inwagen's approach, but we need not view it as undermining the approach.

Sider also claims that van Inwagen must accept (2) as a necessary truth because «his arguments seem to be based on non-contingent considerations» (p. 287). First, it is not so clear that his arguments are based on purely non-contingent considerations. Van Inwagen does cite current physical theory (see van Inwagen [1990], p. 99 for one example). Second, even if his arguments were based on non-contingent considerations, it is not clear why it should follow that he must accept (2) as a necessary truth.

On the other hand, it seems that van Inwagen's account might well be modified in order to address the case of gunk. Let us tentatively admit that there could exist gunk worlds where there seem to be material objects such as tables and chairs. Even if we admit that such worlds are possible, we might still be able to apply an analysis which eliminates putative commitment to objects such as tables and chairs. As I note above, Sider claims that we can imagine gunk being shaped into geometrical forms or even into tables and chairs. It does not follow that we must accept the existence of tables and chairs at such worlds. Suppose we take the sentence:

s3) There is a table at R,

(where R is a spacial region) and paraphrase it (in a way parallel to van Inwagen's approach) by the sentence:

s4a) Gunk is shaped tablewise at R,

or by the sentence:

s4b) Some gunk is shaped tablewise at R.

Such sentences do not either implicitly or explicitly quantify over tables. That is, it does not follow from either s4a or s4b that:

s5) There exists an x such that x is a table and x is made of gunk.

It may be that sentences such as s4a or s4b do quantify over masses of gunk, but we need not accept that such masses compose anything. Thus, we might well have a world filled with gunk, but this gunk need not be said to compose objects such as tables, chairs, or rocks.

Perhaps we would be committed to the existence of one thing at gunk worlds; namely, the scattered object composed of all the gunk at a given world. In *Word and Object*, Quine suggests that we might allow a term such as 'water' to denote the aggregate of all the scattered bits of water in the world, «a single scattered object» (Quine [1960], pp. 98-99. See also pp. 120-121). We need not go so far as to characterize the gunk itself (at a world) as constituting some sort of blob or «blobject» to which we can refer. (Horgan [1991] addresses the notion of a blobject in the context of a Parmenidean materialism.)

Doing so, however, raises an intriguing alternative. At one point van Inwagen claims that Aristotle's view of organisms being «entirely composed of absolutely continuous stuffs» entails «that living organisms are simples» (van Inwagen [1990], p. 98). This suggests that the blobject might itself might be treated as a simple. Given this, we would be committed to the existence of at most one thing at any gunk world (the blobject) since we would have a thing composed of continuous stuff and having no proper parts. Thus, we would be committed to the existence of the blobject qua metaphysical simple, but we would not be committed to things like tables, chairs, or rocks.

Need we be committed to the existence of the blobject? Here it is crucial to recognize a deep connection between the notions of countability and quantification. This point is nicely addressed by Jose Benardete when he discusses the difficulties involved in the application of Quine's criterion to mass terms (Benardete [1989], p. 35). For instance, in Thales' view, all is water. Benardete notes that Nicholas White has suggested that Thales' claim be symbolized as:

(x) (x is water),

but since water is not a count noun, it would be difficult for Thales to talk about how many things there are in the world. That is, it would not make sense for him to say how many water(s) there are. Benardete concludes that this seems to be a case where ordinary quantification is inapplicable and Quine's criterion simply does not apply (Ibid.). This is perhaps the cause of Quine's frustration with mass terms in *Word and Object* when he disparagingly refers to the category of mass terms as «that archaic survival of the first phase of language learning» (Quine [1960], p. 121.).[1]

We can, however, characterize Thales' claim in a way more favorable to a Quinean approach. We might take Thales' claim to mean something like:

(x) (x is entirely made of water).

---

[1]   I am grateful to José Benardete for this point.

Such a thesis would be true of (say) the visible, non-biological parts of an Eskimo's environment.[2]

Benardete, however, explicitly denies that either of these alternatives adequately captures Thales' position. He claims that Thales might respond to such suggestions in the following way:

> But there is no number of things, absolutely speaking, though pragmatic considerations allow us to parcel out the world's water as it may suit our convenience, as we notice that here it is more condensed, there the more rarified. If on my theory there is only water and nothing else, that is not to be understood as entailing that there is either only one thing in the universe, which 'the world's water' might be supposed to denote, or that there are two or more things each of which consists solely of water (Benardete [1989], p. 35).

That is, the notion of quantification over a certain class of objects is connected with the notion that those objects are in some sense countable. Given Benardete's view, this would not be the case for something like gunk.

Sider's argument against van Inwagen's approach raises some important questions for any such approach. His argument is clearly valid, but the second premise is problematic. It is difficult for Sider to support the claim that gunk worlds containing material objects exist without begging the question against van Inwagen. Further, Sider's claims against van Inwagen's approach do not hold up under careful scrutiny.

First, Sider fails to give us an account of how gunk could come to compose an object, nor does he directly dispute van Inwagen's approach to composition. Second, the existence of gunk worlds does not cause the sort of trouble for van Inwagen that Sider claims. Van Inwagen need not accept the existence of objects such as tables, chairs, and rocks at gunk worlds. Moreover, van Inwagen's approach to paraphrase can easily be modified to handle the case of putative objects composed of gunk.[3]

Bibliography

Benardete, José [1989]: *Metaphysics: The Logical Approach*, Oxford University Press.

Horgan, Terence [1991]: «Metaphysical Realism and Psychologistic Semantics», *Erkenntis*, 34, pp. 297-322.

Lewis, David [1991]: *Parts of Classes*, Basil Blackwell.

Quine, W. V. O. [1960]: *Word and Object*, The MIT Press.

---

[2]   I am grateful to Peter van Inwagen for this point.

[3]   I would like to thank Thomas McKay for comments on a previous draft of this paper.

Sider, Theodore [1993]: «Van Inwagen and the Possibility of Gunk», *Analysis*, 53, 4, October, pp. 285-289.

van Inwagen, Peter [1990]: *Material Beings*, Cornell University Press.

Kelly J. Salsbery

Dept. of Philosophy

Syracuse University

Syracuse, New York, USA

<salsbery@mailbox.syr.edu>

GRAHAM PRIEST'S "DIALECTHEISM" —

IS IT ALTHOGETHER TRUE?

Lorenzo Peña

§1.— True Contradictions

Graham Priest's book *In Contradiction* (Dordrecht: Martinus Nijhoff, 1987) is a bold and well argued-for defense of the existence of true contradictions. Priest's case for true contradictions — or "dialetheias", as he calls them — is by no means the only one in contemporary analytical philosophy, let alone in philosophy *tout court*. In some sense, other defenses of the existence of true contradictions are less philosophically "heterodox" than his is, since, unlike Priest's orientation, other approaches are closer to prevailing ideas in mainstream analytical philosophy, whereas Priest's leanings are strongly anti-realist, and not distant from the logical empiricism of the thirties.

However, such issues seem to me almost immaterial for the chief arguments in Priest's book. Much of what he says can be accepted from a wide variety of philosophical outlooks. And most of it seems to me right and important. Others among his arguments are less cogent, but can be modified and thus rendered far more convincing. Even when that is not so, weaker — less sweeping, but more plausible — arguments can be put in their place. And, what is more, such a watering-down does not affect the main conclusion of the book, the existence of true contradictions.

Whoever is prepared to accept thesis **ETC** — namely, **that there exist true contradictions** — will meet strong opposition. A number of people will be flabbergasted. «But it is contradictory to say that there are true contradictions!». And? Well, as for arguments there is a deplorable shortage of them against **ETC**. Just claiming that **ETC** is contradictory can hardly be regarded as an argument. Priest has in other places tackled some of such arguments as there exist. Let me begin this critical notice with a short discussion of a couple of those preliminary objections against **ETC**.

The main argument is of course Aristotle's, which has always been taken to be an argument for the principle of noncontradiction, but which can

be read as an objection against **ETC**. (Whether an argument against ⌜p⌝ is the same as one for ⌜not-p⌝ is a central issue, which will in due course be dealt with in this paper.) Aristotle's objection amounts to this, according to my exegetical lights (although of course there is an astonishingly wild variety of interpretations): if you are prepared to accept just one true contradiction, you are bound to accept any and every contradiction — and hence every statement —, since the only or at least the strongest reason against accepting a contradiction is just the fact that it is one; if that fact does not debar you from accepting it, you lose any reason for rejecting any other contradictory claim. Not that no other reason can be found, but none will be as strong as the mere fact that the belief — or the statement — is contradictory. If the strongest reason against a claim is not strong enough, neither are weaker reasons.

A possible reply is that for a belief to be contradictory is not bad, and so it is no reason against that belief; hence, it is downright false that contradictoriness is the strongest reason against a belief.

Such a reply does not seem to me to be open to G. Priest's peculiar brand of "dialetheism" — adherence to **ETC**. For he seems to share the view that true contradictions are bitter pills to swallow, which must be done only exceptionally in a few fields. Other things being equal we ought to refrain from accepting contradictions. So contradictoriness is after all a reason against a belief. Yet Priest clearly denies that it is the strongest reason. But no clear indication of what reasons against a belief are good or convincing is provided in Priest's book. (I'll dwell on this issue later on.)

A different objection against **ETC** is that whatever reasons there are for ⌜p⌝ are reasons against ⌜not-p⌝ and the other way round. Since reasons for ⌜p and not-p⌝ are only all reasons for ⌜p⌝ which are also reasons for ⌜not-p⌝, it emerges that all reasons for ⌜p and not-p⌝ are also reasons against ⌜p and not-p⌝. Since there are other reasons against ⌜p and not-p⌝ (namely reasons for the universal truth of the principle of noncontradiction), the balance is definitely against ⌜p and not-p⌝.

Priest rejects the equation between **reasons-for-not-p** and **reasons against-p**. I think he is right, but unfortunately his dialetheism seems to me incapable of explaining what a reason against a belief may be. The notions of **rejection** and **being-a-reason-against** have to remain primitive (or explained one through the other), with no sentence being even available in the language by asserting which rejection could be expressed. That is unfortunate. What about the etymology of '**contradiction**' as **saying-against**?

Let me at this stage anticipate the core of my coming criticism. Priest does not accept degrees of "against-ness", degrees of negation, degrees of anything connected with the notions he explores in his book. Which blocks one of the most reasonable and straightforward roads to an accommodation with what the objection we are studying is up to. Should there be degrees of truth and falsity, we could say that a reason [strongly] against ⌜p⌝ is a

reason not for ⌜not-p⌝ but for some stronger negation of ⌜p⌝ — namely ⌜not p at all⌝. Thus the objection would be rebutted on count of its failing to distinguish natural or weak from **strong negation** or **overnegation**. No such move is available to G. Priest.

Last, there is the hackneyed and by now discredited claim that from ⌜p and not-p⌝ everything follows. Again, yet, the claim depends on a number of steps, the only one which seems reasonably rejectable being **DS** (**Disjunctive Syllogism**). But rejection of **DS** is not easy. There are clear uses of **DS** which are right. Priest allows **DS** with a contextual proviso — namely that contradictions in the field within which **DS** is being applied do not arise, or are likely not to arise. I'll discuss his position on that issue a little later.

Alternatively, if we had both weak and strong negation we could say that the only true contradictions which exist involve weak negation, whereas **DS** is valid only for strong negation. Which of course is no reply available to Priest.

Thus I conclude that the preliminary objections against **ETC** fail, and that there are not unreasonable ways of parrying them — even if the ones I prefer are not those which are in agreement with Priest's particular sort of dialetheism.

If the preliminary objections against **ETC** are far from being the knock-down arguments Aristotelians were fond of thinking they were, there are several good arguments for **ETC**. Priest's book displays several of them. Not that they are knock-down either — although Priest seems to me to think they are final. Acceptance of true contradictions is not going to offer a miraculous whole-sale solution to all problems in philosophy, but is going to make things much easier in all fields. After all, many convincing reasonings end in contradictory conclusions some of which can be maintained upon consideration, with a lessening of the strains and the constraints under which we used to find ourselves. At the very least, in many cases an alternative is now open to weighing, which was formerly unthinkable, due to acceptance of Aristotelian logic and its offspring.

§2.— Are true contradictions in between complete truth and complete falsity?

I find it odd that G. Priest, in his mustering of reasons for **ETC**, does not consider the notion of degrees of truth. Now, that notion is connected with the blossoming of fuzzy logics and set-theories — about which not a word is said in Priest's book — which have been extensively argued for in a variety of papers, and shown to have important applications both in theory

and in practice (engineering). Priest cannot be unaware of those developments. He has clearly chosen to ignore them as pointless for his enterprise.[1]

I feel confident, though, that many readers will share my impression that the only non-classical truth value Priest accepts is precisely in between the extremes of pure truth and pure falsity; and so an intermediary value, which is less true than the wholly true.

Let me explain. Priest posits three truth-values: {T}, {F} and {T,F}. In order to give a uniform account we can identity T with {T} and {F} with F. Then we can say that the truth values are

(1) T, F

(2) for any two values, X, Z, X∪Z

There is no fourth truth value, since the union of any of the three with either of the other two is one of the three values.

Now, it seems obvious that what we have is a trichotomy of pure truth, pure falsity, and a mixture of them. What has a mixture of a property ϕ and its opposite is less ϕ than what only has the property ϕ and completely lacks its opposite. So the third truth-value is intermediary, a middle-course in between the extremes — in the same way as mixing white and black yields grey, which is less black than the black and less white than the white, but blacker than the white and whiter than the black. Mixing dryness with moistness gives something which is in between, humid.

Now, what is the reason for stopping at that stage rather than proceeding to introduce further intermediary truth values? Well, yes, according to the procedure, any new mixture is going to be just one of the three values — namely the mixed value {T,F}. But why not change the procedure slightly?

For instance, we can think in terms of multi-sets rather than sets — a multi-set being characterized by the fact that an entity may belong to it several times. Or we can directly think in terms of fuzzy sets — but, not to beg the question, I will not avail myself of them at this stage. Or we can take as the operation which generates new values, not union, but the pair-forming operation, { , }. So, in addition to T — which we no longer need to identify with {T} — and F — no longer to be equated with {F} — we have as additional truth values: {T,F}; {T,{T,F}}; {F,{T,F}}; {F,{T,{T,F}}}, etc.

It is not easy to understand what the last one is. We can impose a constraint which makes things clearer, by ruling out such combinations as {X,{Y,Z}} if X≠Y and X≠Z. Then we have infinitely many intermediary

---

[1]   The notion of fuzzy set was first endowed with a formal treatment by Lofti Zadeh in 1965. A huge literatures has at this stage been published developing its theory and applications in an impressive variety of fields.

values which are clearly degrees of truth and falsity — except the original ones, T or total truth, and F or total falsity. {T,F} or ½ is equidistant between: T and F. {T,{T,F}} is equidistant between T and ½. {{T,F},{T,{T,F}}} is equidistant between ½ and the value which is equidistant between ½ and T. And so on.

Priest offers no reason against adopting a procedure like that. He does not consider doing so. He probably thinks that doing with just T, F and {T,F} is enough, that nothing new is either needed or desirable or even perhaps possible, since we already have Truth, Falsity and the Mixture of them. Yet we also have Wine, Water and … "the" mixture of them. What mixture? A 50-50 one? Is it the same as the 99-1 mixture? Are we to count all such mixtures as the same, because we do not care how much water there is, a small drop carrying the same weight as a million drops?

In most cases, if two qualities — or masses, or whatever — can be mixed, they can be mixed in many degrees of either. Certainty and doubt, love and dislike (or even hate), joyfulness and sadness, sweetness and bitterness, etc. Each mixture has a dose of either ingredient. Now, perhaps Truth and Falsity can never get mixed, as the classical logician contends. If they can consort with one another, why in such a way that degrees cannot be taken into account?

Since, according to Priest, true contradictions have both Truth and Falsity, they are true and false (although a problem arises here with what Priest calls the principle of exclusion, to which I'll come back later on). But, if such is the case, why cannot that mixture admit of degrees of each of the mixed properties — with the obvious constraint that, the more of the one, the less of the other?

By refusing such a gradualistic approach, Priest adopts a stand only slightly less rigid than the Aristotelian, for whom there are exactly two situations as regards truth: either it is [completely] present or else it is [completely] absent. Priest allows a 3d case where it is both or perhaps where both truth and falsity are present — but agrees with the Aristotelian (or classicist) in rejecting any further complication or any degrees of presence. Is our world-view much improved by accepting just a 3d, internally uniform, degree-less, mixture of heaven and hell, along with the originally given extremes, rather than a full scale of infinitely many gradations?

Against the foregoing considerations it can be argued that, truth and falsity not being mass terms, it does not make sense to speak of mixing, still less to talk of "more truth" as we talk of "more water".

Is that so? Well, I suppose my considerations bear a distinctive Platonistic ring: in some — perhaps non-literal — sense, when a thing is hotter than another one, there is more heat in the former — or maybe there is a greater presence of heat. If a proposition is truer than another one, there is more truth in the former than in the latter.

Well, yes, I know, not everybody is prepared to accept that a proposition can be truer than another: either it is true, *tout court*, or else it isn't. (But, please, notice that I of course accept the principle of excluded middle, and that not only nothing of what I've hitherto said runs against excluded middle, but in fact linking truth-graduality with true contradictions assumes excluded middle — else we could avoid the contradictions by contending that sentences with intermediary truth values can be neither asserted nor denied.)

It seems to me, though, that such a line is not open to G. Priest. He has clearly taken the true and the false to be mixable, his third truth value being an alloy of both, and being meant to be both. He even uses such expressions as "purely true" or the like for characterizing sentences with value {T}, as against sentences which are true but not purely true, namely those with value {T,F}.

Since unalloyed truth, pure truth, completely rules out falsity, whereas the mingle of truth and falsity which is {T,F} does not, we seem allowed to gather that sentences with value {T,F} are less true than sentences with value {T}, the latter alone lacking falseness altogether. Since being fully true is the same as lacking falsity altogether, only sentences with value {T} are entirely or wholly true — and only sentences with value {F} are utterly false.

Such considerations can be countered by insisting that a sentence with value {T,F} may be completely true — and completely false, too? Well, if it comes to a matter of definition, it is hard to find an argument on those matters. Yet, I feel that Priest cannot deny that the most natural reaction to his proposal that some sentences have as their truth value {T,F} is to view such a situation as a case of those sentences being neither completely (or purely) true nor entirely (purely) false, but in between, having both truth (to some extent) and also falsity (up to a point).

Natural reactions may be quite mistaken. Perhaps it is natural to extrapolate our visual field and conclude that the Earth is flat. But then there are arguments to the effect that the Earth is not flat (at all). Are there arguments which show that {T,F} is not a mixture of truth and falsity?

A different objection against my gradualistic construal of value {T,F} as a blend of truth and falsity is that there is no way to make sense of degrees of presence or any such Platonistic talk, which as such is merely metaphoric. But that is wrong. The set-theoretical approach I have sketched is a way of cashing the metaphor. (Moreover, a Platonist needn't even fall back on such a re-wording.)

Therefore, pending arguments against the gradualistic interpretation, it seems to me we can be confident that what in effect Priest is putting forward is the existence of cases of truth which are not cases of complete truth; cases of something being partly true only. Is it reasonable to assume

that all beliefs which are true but not purely true are equally true, none of then being truer than another?

Let us come to specifics. One of the main intended applications of Priest's defense of **ETC** is semantics. He thinks — rightly, to myself — that the sentence 'This sentence is false' is both true and false. If we have an implication, '→', read as 'to the extent that…, ---', or the like, then we can construe the (simple) liar in this way.

Graham Priest emphasizes that in any language rich enough to contain Peano arithmetics and endowed with a truth-predicate 'T', we obtain a contradiction (p. 99). For any sentence of the language, ⌜p⌝, let '#p' be the numeral of the Gödel number assigned to sentence ⌜p⌝ (under some definite way of codifying expressions into numbers, be it Gödel's original one or any other). Let us use the diagonalization technique in order to construct an open sentence ⌜φ(x)⌝ which is true **to the extent**, and to the extent only, that (loosely speaking) its diagonalization is not true; less inaccurately, let δ map the Gödel number of an open sentence with one free variable, 'v', into that of this diagonalization, i.e. of the result of substituting, in the given sentence, the numeral of its Gödel number for its only free variable. In virtue of the diagonal lemma (a particular case of the fixed-point theorem), if ⌜α⌝ is any formula with one free variable 'v', there is a sentence ⌜β⌝ such that ⊢β↔α(v/#β). Let us take the open sentence ⌜~Tx⌝, where we have laid down that ⊢T#p↔p as an axiomatic schema. It follows that there is a sentence ⌜p⌝ such that: ⊢~T(x/#p)↔p, i.e. ⊢~T#p↔p. In virtue of the axiomatic schema ⊢T(#p)↔p, we'll have:

(1) ⊢T#p↔~T#p (for some «p»)

What does (1) mean? If we cleave to our proposed reading of '→' (and hence to that of '↔' as 'to the extent, and to the extent only, that'), it means that the sentence ⌜p⌝ under consideration is true to the extent that it is not true, and conversely; hence as true as not true, neither more nor less. Let us grant all that. But, if there is such a sentence, equally true and false, why not a sentence slightly more true than false, and one in between being slightly more true than false and being wholly true, and so on?

What is unique to the liar is that it says of itself that it is not true. Hence, in virtue of ⊢p↔p, and substitutions, we have that, to the extent it is true, it isn't, and conversely. Nothing like that is available for sentences which are more true than false, or more false than true. Still, if the liar exists, why not those others?

The issue is not whether we can prove merely by such means that such intermediary cases exist. After all, proving the liar depends on a number of very debatable assumptions — although I am confident Priest has shown that the usual ways-out are not as good as their adherents are keen on thinking. I do not deny the naturalness of proving the truth-and-falsity of the liar. The point is that, once its existence has been granted, the plausibility of further intermediary cases is much enhanced.

§3.— **Avoiding ineffability and the need for strong negation**

One of the most forceful and recurring arguments throughout Priest's book is that the usual ways-out lead to ineffability. I am not going to repeat Priests' detailed and convincing arguments. In fact such a conclusion should be obvious. If there is no language which is the metalanguage of all languages, in what language can such a non-existence be said? It must be a language wherein we can speak of all languages and their semantic qualities. The "hierarhist" approaches — as G. Priest calls them (p. 24) — cannot even say of themselves that they are true. They have to fall back on unquantified schemata, through "systematically ambiguous" expressions. The problem is similar to that encountered by type distinctions, but not quite the same.

I find Priest's arguments congenial and very plausible. Yet his way of putting all that in terms of all or nothing seems to me unfortunate. You get the impression that either a sweeping, naive, thoroughgoing approach to semantics is accepted, and then the paradoxes ensue and are also espoused — which of course calls for a paraconsistent logic — or else we have to cling to some variety of the hierarchy, be it Tarski's or Kripke's or whatever. But surely there are hierarchies and hierarchies. Not all of them are as harsh and unpalatable as Tarski's. In fact Tarski's was a crude, extreme reaction, whereas later approaches are milder, more refined, less destructive. Priest himself acknowledges that some of them admit of truth-value gluts rather than gaps (see p. 26, n. 20 on Woodruff's treatment). And of course new approaches can be devised, by refining, or qualifying, those which are available.

Against adherents of truth-value gaps, Priest convincingly argues that they cannot express that a sentence is not true (p. 20). Unfortunately though, he encounters exactly the same situation. For he needs to differentiate between that which is only false and that which is both true and false. He does so by using expressions such as 'only', 'purely', 'plain[ly]' (p. 239) and the like. Now, what is a plainly false sentence? One which is true and which is not false? If we could say that, and by saying so enough information were to be provided, it would be fine. Can we? Not if we accept the **exclusion principle**, namely that, to the extent that a sentence is false, it is not true. The immediate effect of the exclusion principle is that the contradictions spread to the "metalanguage" — speaking in the customary jargon. For let ⌜p⌝ be a sentence both true and false, i.e. with value {T,F}. Then the sentence «This is true: ⌜p⌝» will also be true and false. By saying that ⌜p⌝ is true and not false we say nothing incompatible with ⌜p⌝ having as its truth-value {T,F}, and with so doing T#p.

Dropping the exclusion principle avoids such spreading of contradictions into the meta-language at a high price. Rather than having the T schema for mutual (contraposible) coimplication, '↔', we are supposed

to do with some makeshift, to which contraposition does not apply (see pp. 88-91 and 99-100). Priest says (p. 100).

> There seems to be no reason why, *in general*, if α is a dialetheia, Tα is too. If α is a dialetheia, Tα is certainly true, but it might be simply true, and not also false. The truth predicate is therefore a *partial* consistensizer.

Priest contends that the exclusion principle spreads contradictions beyond necessity (p. 90). 'On the basis of this I tentatively reject the exclusion principle' (ibid.). It never emerges whether in the end Priest comes to accept the principle.

It is not just a matter of definition. If the exclusion principle fails, many arguments for true contradictions which hinge on the T schema also fail and have to be reformulated. (The reformulation involves more debatable principles (pp. 162-3), so those who oppose Priest's dialetheistic solutions are provided with a number of possible and plausible retreats.) Moreover, the fundamental idea that the truth of ⌜p⌝ is the fact that p is no longer correct.

Furthermore, Priest's account of the T predicate without the exclusion principle really fails to thwart the spreading of contradictions into the "metalanguage". It introduces two separate predicates, truth, T, and falseness, F: a formula is false iff its negation is true — 'iff' being my own reading of Priest's non-contraposible biconditional '⇔'. What emerges (p. 176, bottom) is that ⊢p⇔T#p and ⊢~p⇔F#p. (Priest dodges the exclusion principle, ⌜F#p⇒~T#p⌝, by dropping contraposition for '⇒'.) In the end this account does not succeed to stop the spreading of contradictions, since it espouses the conclusion that, for a certain ⌜p⌝, we have both ⌜T#p⌝ and ⌜~T#p⌝.[2]

Hence, I find waiving the exclusion principle unattractive. Nonetheless, suppose we in fact jettison the exclusion principle and, by doing so, manage to keep contradictions away from the "metalinguistic" level — i.e. that we thus successfully ban contradictions involving the predicate 'T', contradictions of the form ⌜Tx∧~Tx⌝. Now, with or without the exclusion principle we need to differentiate between situations which are

---

[2]   The proof that the thus weakened truth theory still contains contradictions (in virtue or the previously attained result that, for some ⌜p⌝, ⊢~T#p⇔p) involves abduction rules for the noncontraposible conditional '⇒', namely p⇒~p ⊢ ~p and ~p⇒p ⊢ p — although the point is not made quite clear in the text. The whole treatment is somehow marred by the fact that '⇒' is not provided with an English reading; and, once contraposition has been junked for it, I surmise that not everybody will accept the abduction rules. If you relinquish the naive simplicity underlying the original T schema — namely that ⌜p⌝'s truth is just the fact that p (with the **strongest** biimplication thus linking the fact that p with ⌜p⌝ being true)—, I guess some contradiction-averting manoeuvres become less implausible: e.g. refusing to accept either of the abduction rules for conditional '⇒'.

only false and such as are both true and false. Suppose that ⌜p⌝ is both, i.e. that it has truth value {T,F}, while — thanks to our having scrapped the exclusion principle — both T#p and F#p are only or plainly true, with no admixture of falseness in them (!). Let ⌜q⌝ be such that its truth-value is {F}. How can we differentiate the values of ⌜p⌝ and ⌜q⌝? Not through F, since both F#q and F#p, but through T. But then we shall have a way of expressing a strong negation. Let us define 'H' in this way: ⌜Hp⌝ abbr ⌜T#p∧~T#(~p)⌝. Notice that 'H' is an operator, not a predicate. Yet, within the whole arithmetic-cum-semantic theory Priest provides, it is definable. 'H' is strong assertion. The rule Hp, ~p ⊢ q is truth-preserving — the premises cannot be both true. Strong negation is definable: ⌜¬p⌝ abbr ⌜H~p⌝. The Cornubia rule for strong negation (viz. p, ¬p ⊢ q) is also truth-preserving (for the same reason, of course): p , ¬p ⊢ q. Those rules can of course be avoided by imposing conditions on the turnstile over and above mere truth-preservation. But I do not see what further requirements Priest imposes.

Now, let us considerer the sentence:

(L) ¬L

(L) says of itself that it is completely non-true, i.e. that it is not at all true that it holds. If all the semantic machinery Priest has developed is still available at this stage (and how could it have broken down by now?), an **overcontradiction** can be proved, namely: L∧¬L. (An overcontradiction is simply a contradiction involving strong negation.) Hence q (every ⌜q⌝). Unless, … Unless we impose further conditions on the turnstile (but is that really a solution or a mere stipulation?) or revert to the initial T schema, with ⌜T#p⌝ having the same truth value as ⌜p⌝. Which would mean that we accept the exclusion principle after all.

With the exclusion principle, however, we can no longer — within Priest's account — differentiate truth *tout court* from plain truth. It does not help to say that ⌜p⌝ is true and not false; that will be the case, too, if its truth value is {T,F}: it will both have and fail to have truth; the latter is true, for ⌜~T#p⌝ is — in accordance with the exclusion principle — implied by ⌜T#~p⌝, which is true if the value for ⌜p⌝ is {T,F}.

A strong negation is needed, one '¬' such that ⌜p⌝ completely rules out ⌜¬p⌝ and the other way round. With strong negation we can explain the difference between being true and being plainly (i.e. completely) true; and between being false and being downright false. Then many things come into place. We'll have a criterion on when **DS** can be relied upon (pp. 137ff): whenever the negation involved is strong, or can be taken to be strong. We know when there is an argument against a claim: whenever there is one for the strong negation of the claim. We know why rejection and acceptance are fully incompatible (pp. 128-32): rejecting ⌜p⌝ means or entails accepting ⌜not p at all⌝, i.e. ⌜¬p⌝. We know why Priest's principle R (see p. 141) holds, namely 'If a disjunction is rationally acceptable and one of the disjuncts is

rationally rejectable, then the other is rationally acceptable'; the reason it holds is that **DS** is valid for strong negation.

More than that is gained with adding strong negation. **CL** (**classical logic**), all of it — including *Modus Ponens* for '⊃', provided ⌜p⊃q⌝ abbreviates ⌜¬p∨q⌝ — is now incorporated into the paraconsistent system. **CL** is shown to be, not wrong but poor, insufficient. The classicist can be placated; he can be asked only to refrain from reading '¬' as 'not'. The resulting system is more ecumenical.

Yet, of course, there is a price, a high price. We can no longer accept the T predicate (within a language sufficiently powerful) or the comprehension axiom in set theory (on which, see hereinbelow), or Priest's elegant and simple treatment of the Gödel sentence B, namely «This very same sentence cannot be proved». For in each of those cases, putting strong negation in the place of natural negation brings about an overcontradiction — unless the principles in question are weakened or somehow qualified.

Priest seems to me implicitly committed to having strong negation. On p. 146 he introduces a propositional constant 'F' such that for all sentences ⌜p⌝ ⊢F→p. He stresses that if the language contains its own truth predicate the constant 'F' can be defined as '∀xTx'; the characteristic principle is then proved. Such being the case, Priest's system — once it encompasses arithmetics and the truth predicate — does in fact comprise strong negation; for let ⌜¬p⌝ abbr ⌜p→F⌝ . Priest is right when he thinks that rejection of contradictions in general is expressed by the schema ⌜p∧~p→F⌝ (which amounts to ⌜¬(p∧~p)⌝; i.e. every contradiction is completely false). Replacing '~' with '¬' yields a formula which expresses rejection of over-contradictions, namely ⌜¬(p∧¬p)⌝ (overnegation of any contradiction involving overnegation). Although Priest's own system shuns ⌜¬(p∧¬p)⌝ as a theorem — by dint of avoiding conjunctive assertion (see below, §5) — it is committed to something close, namely ⌜¬p→.p→F⌝ .

Thus Priest faces a dilemma. If he accepts constant 'F' and so strong negation (or if it is true that 'F', with its characteristic principle, is already present in his whole system), then something not far distant from **CL** is included, the naive and simple arguments for **ETC** in semantics and set theory are no longer available, and anyway semantics and set theory need some further remedies over and above the mere acceptance of contradictions (contradictions involving '~' but not '¬', i.e. not **over**contradictions). If he keeps clear of strong negation (and of constant 'F', which I doubt he can do, and so does he), then ineffability results ensue, which lessens the effect of his criticisms of any hierarchist way-out of the semantical paradoxes; moreover, all the whole issue of **arguing-against** (p. 141), the rationale for principle R, rejection, etc., becomes misty if not enigmatic.

§4.— **Set Theory**

Although semantics is the main reason for Priest's dialetheism, set theoretic paradoxes also feature among the grounds for his approach. And rightly so. He discusses the abstraction principle under the form $\ulcorner\exists y\forall x(x\epsilon y\leftrightarrow\beta)\urcorner$ and extensionality. He launches (pp. 37ff) an onslaught on the cumulative conception of sets implemented in ZF. His criticism is, to myself, quite cogent. Then he espouses (pp. 178ff) the abstraction principle and hopes that his formal system Δ can admit it without **deliquescence** (i.e. Post-inconsistency, or triviality, as he says following the current fashion). His reason to hope is that Δ is very close to a relevant logic, DK, which has been shown by Brady to be compatible with the abstraction schema.

Priest's objections against the cumulative hierarchy seem to me so obvious that I find it amazing, not that a number of mathematicisms **use** ZF, but that some philosophers take it as what it was never meant to be (not by Zermelo anyway), namely an "intuitive" conception of what sets are. Priest's objections can be strengthened. If something like the temporal metaphor he rightly denounces (p. 39 bottom) is to be taken ever so little seriously, then the idea is a constructivistic one. But then quantifiers cannot be allowed to range over **all** sets, but only over sets which "already" exist (i.e. a predicative set theory is required, which ZF and the like are not).

A different objection against ZF is that it lacks not only a universal set but complements (except relative complements). Not only it cannot be the set theory used in its own metatheory, but it cannot provide a satisfactory semantics for internal negation — there being no set which comprises only all entities that not p, for any $\ulcorner p\urcorner$.

However, Priest fails to discuss Quine's systems NF and ML for the reason that they are 'widely regarded as little more than… curiosit[ies]' (p. 38). Yet, if **CL** is true, and if some sort of Tarskian semantics is to be possible, the hierarchical cumulative approach is bound to be wrong, and something like NF or ML right. Philosophically those systems are infinitely more appealing than ZF, and deserve to be discussed. That NF entails some oddities concerning the ordinals, is of little or no concern, since the whole subject is anyway riddled with surprising results.

Priest does not discuss non-well-founded set theory or Fitch's combinatory logic, either. His case would be much stronger if he took such alternatives into account. Neither does he discuss other paraconsistent approaches, which accept the abstraction principle with some restrictions. Yet I guess what leads him to ignore such approaches.

His line is straightforward and clear. Take the abstraction principle in its pristine and unpolluted purity; a contradiction ensues. Tampering with Abstraction, as in ZF, causes a lot of trouble and extremely undesirable results. Hence.

But suppose that we have both natural negation '∼' and strong negation '¬', as we have seen in the previous section. Then Abstraction has to go. Yet we could have something weaker.

Let us discuss, not abstraction, but Comprehension. Let {…:---} be a vbto (variable-binding term-forming operator), which we take as primitive (we could use λ notation). We need a set theory with these principles, or qualified versions thereof:

(Existence) $\exists z(z=\{x:p\})$

(Comprehension) $\{x:p\}x \leftrightarrow p$

(with 'z' not free in ⌜p⌝ in the former principle), and membership being expressed as 'zx' rather than 'x∈z').

With strong negation, catastrophe!

$\{x:\neg(xx)\}\{x:\neg(xx)\} \wedge \neg\{x:\neg(xx)\}\{x:\neg(xx)\}$

Hence ⌜q⌝. Suppose that we qualify Comprehension by requiring that ⌜p⌝ contains neither '→' nor strong assertion 'H' (and hence not '¬' either). But we can add half of it without qualification, namely:⌜p→{x:p}x⌝.

The system — which can be implemented on the basis of some strengthening of relevant logic E — is reasonably strong. We can prove that the set of such sets as do not comprise themselves both comprises itself and does not. We can also prove that the set of such sets as do not comprise themselves at all comprises itself (although of course we cannot prove that it does not comprise itself).

Let us call 'crowds' such sets as our theory, thus conceived, would account for. Crowds can be thronged or [over]crowded. They can let in entities which utterly fail to comply with the entry-condition — with the characterizing matrix or formula. Such cases can be exceptional. We can add a number of particular cases of (full) Comprehension for which we satisfy ourselves that no overcontradiction is going to arise.

Priest does not discuss any such proposal but it is clear why he finds them distasteful. Gone with them is the straightforward argument from Abstraction to dialetheism. The classicist can retort that if so-called strong negation is debarred from featuring in the two-ways Comprehension principle, why not so-called natural negation, too? Are they not on a par? If from applying comprehension to {x:¬(xx)} everything follows, why not from applying it to {x:∼(xx)} — '∼' having the property of negation in relevant logic **E**? Are those not exactly the same pattern of inference?

No, they are not. An inference pattern is syntactically characterized, as is every proof-theoretic notion. And the difference between the two negations can render one of the patterns correct while the other is wrong.

— «But you do not intuitively see that one is right and the other wrong. You show such conclusion from the fact that within such a system — a

strengthening of E —, enriched with strong negation as you call it, from {p, ¬p} everything follows, whereas nothing of the sort happens with {p, ~p}. That is no longer a matter of intuitive principles, but an artificial contrivance. And then why bother us with nonclassical negation at all? Don't you have the worst of both worlds?»)

Yet Priest's approach, thus mollified (or watered-down), could become more attractive. After all, his current approach has it that R (i.e. {x:~(xx)}) both comprises itself and does not. As much is true — according to him. Necessarily true (I gather). Better to recognize a necessary truth than to say that it is so false that everything follows from it — which is what the Aristotelian does, when he mistakes his '¬' for 'not'. A theory which countenances RR∧~(RR) is better than one which enforces RR∧~(RR) ⊢ q. So the considered theory of crowds is not worse off than classical set theories (with the possible exception of Quine's — about which I shall say nothing here, since Priest does not consider them).

Besides, crowd-theory would accept a universal set. We would be rid of the troubles which arise from ZF's necessarily rejecting the existence of a universal set. Gone is the need for the hierarchy. So there are not one but several respects in which crowd theory would be better than standard set theory based on **CL**.

On the other hand, since we'd have strong negation, such difficulties surrounding Priest's approach as have emerged in the previous section would be overcome.

We could conceive of a similar treatment of semantics. Rather than the wholesale unqualified schema T, restricting it to sentences with no occurrence of either '→' or strong negation (or strong affirmation), while keeping one half for all sentences:

p→T#p

Then the simple liar would be as true as false (and hence both) but the strong liar would be only true. (The exclusion principle could — but needn't — be dropped, as Priest advises us to do.) Of course, such a retreat leaves us without the sweeping and direct reasoning from semantics to the existence of true contradictions Priest is keen on. We now need more sophisticated arguments to the effect that natural negation is all right for the T schema, but strong negation can be admitted only into half of it. (Well, a reason is that overcontradictions ensue from admitting strong negation, not natural negation, and that for someone to have a brother who causes trouble and hence is not admitted into the best circles is no sufficient ground for her to be also ostrasized.) We could reinforce predicate 'T' with a number of approximations to the original T schema, short of countenancing it in its pristine generality, — which would thus become a regulative ideal to which we would tend asymptotically.

Furthermore, the resulting approach would keep a lot of what Priest has put forward. It is not as if such an approach had nothing to do with his philosophical enterprise. While obviously closer to canons dear to the **CL** enthusiast, the approach would also accept qualified versions of Priest's principles and most of his conclusions. Most of the contradictions he is keen on proving in his book would remain welcome. Only, things would be a bit less simple and straightforward. And his *a priori* view of logical and mathematical truth would be jeopardized, which of course he does not want. (For you can hardly say that your "intuition" tells you, analytically, that the T schema is right under exactly such qualifications and no others.)

§5.— **Conjunctive Assertion**

Although in his chapter on set theory (pp. 178ff) Priest focuses on how the mere acceptance of contradictions — and hence the rejection of the Cornubia rule — makes it possible to have a full-fledged, unqualified comprehension axiom, things are not that simple. As Priest himself has made abundantly clear in a previous chapter (pp. 103ff) the Curry paradox compels any set theory with an unqualified comprehension axiom (and extensionality — or even weakened versions of extensionality) to have as its underlying sentential calculus one without the **contraction** schema $\ulcorner p{\to}(p{\to}q){\to}.p{\to}q\urcorner$. Priest gives us 'the strongest form of it' (p. 103): **conjunctive assertion — CA** for short —, namely $\ulcorner p{\to}q{\wedge}p{\to}q\urcorner$.

Although **CA** entails **contraction** only once other principles have been countenanced which are not unanimously agreed upon — e.g. any result of adding reduced factor ($\ulcorner p{\to}q{\to}.p{\to}.p{\wedge}q\urcorner$) to the set of axioms and inference rules of **E** minus **contraction** —, yet, since I accept the implication of **contraction** by **CA**, I agree with Priest that the latter is — for our current purposes anyway — what is really at issue. Moreover, in a rule form we can deduce **contraction** from **CA** under very weak entailment principles (see e.g. Richard Routley, ***Exploring Meinong's Jungle***, p. 917, bottom); i.e. from $\ulcorner p{\to}.p{\to}q\urcorner$ we infer $\ulcorner p{\to}q\urcorner$ with the aid of **CA**, even if we do not have as a theorem schema $\ulcorner p{\to}(p{\to}q){\to}.p{\to}q\urcorner$.

Giving up **CA** seems to me too high a price for anything you can win by such a move. Iff $\ulcorner p\urcorner$ can be **strongly inferred** from set A of premises (i.e. inferred in such a way that the degree of falsity of the conclusion is at most as high as the one of the falsest premise) is it the case that, if $\ulcorner q\urcorner$ is the conjunction of all formulae in A, $\vdash q{\to}p$. Since *Modus Ponens* for implication is a strong inference rule in that sense, **CA** is bound to obtain. (My talk of degrees may sound distasteful to relevantists — although Priest is not one of them — but something similar, if worded in somehow different terms, is the idea underlying Anderson & Belnap's famous Entailment thesis.) A proof theory for a logic without **CA** is then bound to be weak or awkward. And, most of all, the plausibility of **CA** much exceeds that of any set-theoretical principle. For, suppose that an instance of **CA** fails (completely — i.e. its strong negation is true). Then although $\ulcorner p{\to}q\urcorner$ and $\ulcorner p\urcorner$ are both

true (since ⌜p→q∧p⌝ is true), ⌜q⌝ is either not true at all or else: less true than ⌜p⌝ and less true than ⌜p→q⌝. But if ⌜q⌝ is less true than ⌜p⌝, ⌜p→q⌝ is utterly false. (Again my talk in terms of degrees is not that essential; Anderson & Belnap in their construction of system **E** would put it in different words, with substantially the same conclusion.)

And, if **CA** fails, so does importation, of course (⌜p→(q→r)→.p∧q→r⌝) or else self-implication (⌜p→p⌝). (Perhaps importation is the strongest form of the principle we are now discussing; it is no coincidence that e.g. Łukasiewicz's logics contain neither importation nor **CA** nor **contraction**.)

Thus the price of having an unqualified comprehension principle is much higher than Sect. 10.1 of Priest's book («Naive Set Theory», pp. 178-80) suggests.

Much the same can be said of semantical paradoxes. There is a semantic duplicate of the Curry paradox (initially I think, put forward by P. Geach in *Logic Matters*, pp. 209-11). Priest gives it in these terms (pp. 103-4): given any arbitrary sentence, β, by diagonalization, self reference or a similar device, we can find a sentence, δ, of the form T#δ→β. The T schema for this sentence yields: T#δ↔.Tδ→β. With contraction we prove ⌜β⌝ (the details are left as an exercise to the reader). Thus the system becomes deliquescent. Everything can be proved.

Thus we ought to choose. Either **CA** or unqualified comprehension and unqualified T schema, not both (not **at all** both). I really feel at a loss as to how to conceive implication without **CA**. Of course, Priest is not the only logician who rejects **CA**. Most characteristically the principle has been fought by Richard Sylvan in his endeavour to develop **deep relevantism**. In *Exploring Meinong's Jungle* (authored with his then name, 'Richard Routley'), p. 919, Sylvan argues in this way against **CA**:

> [**CA**] would exclude situations of the type which occur with semantical paradoxes, where A→B and A both hold but B fails to hold, that is, where an implication which holds is also counterexampled.

Let us critically examine the argument. What does 'fail' mean here? If strong negation is let in — which of course runs against relevantism in general and more so against deep relevantism —, then that B fails probably means that ¬B holds — where '¬' is strong negation, 'not… at all'. But then of course, if B fails to hold, it cannot be true **at all** that both A and A→B hold. On the other hand, if 'fails' is taken here in a weak sense, i.e. if for B to fail is just that ~B is true — '~' being natural negation, the mere 'not' —, then what B's failing entails is that either A→B or A fail **in the weak sense**, i.e. that either ~(A→B) or ~A is true. But that one of them is true does not [completely] rule out A and A→B being both true, too.

A further broadside against **CA** is displayed in *Relevant Logics and their Rivals* (by R. Routley, V. Plumwood, R.K. Meyer & R.T. Brady, Atascadero: Ridgewiew, 1982, pp. 278ff). As the argument makes it clear,

in order for one to junk **CA** it suffices, within a modal modelization, to re-linquish reflexivity of the accessibility relation. (And in fact in Priest's own modal semantics for his paraconsistent system Δ — propounded in his book, pp. 106-8 — reflexivity of the accessibility relation R does not hold, even though our world accesses all worlds, and so also accesses itself.) Under relevant modellings — I quote ***RLR***, p. 279, top — the three-place relation has to be reflexive for **CA** to hold generally, i.e. Raaa has to obtain for every [world or setup] a. The argument goes on:

> Such reflexivity requirements in fact impose serious restrictions on the class of situations admitted in semantical evaluation. They have the effect of ruling out various paradoxical situations. The simple relation Raa includes [excludes?] various paradoxical worlds in rather the way that the simple equation a=*a excludes inconsistent worlds.

What is thus claimed is not that **CA** is incompatible in general with negational inconsistency — 'in rather the way that the simple equation a=*a excludes [upon Routley-Meyer semantics with usual constraints] inconsistent worlds'. The context makes it abundantly clear that what is at issue is Curry's paradox. What emerges is that **CA** rules out situations which would render our theories deliquescent should we have **both** unqualified comprehension or the T schema **and CA**. But it is not enough to devise some modelling according to which **CA** fails, since — as ***RLR*** itself puts it a couple of pages later (p. 281, paragraph 2):

> such an argument has its dangerous aspects, especially as we can now countermodel virtually any logical principle. Accordingly a general objection to semantics which falsify entrenched principles like [**CA**] takes the following turn (…)

What follows is a quotation of a previous paper by G. Priest («Sense, Entailment, and *Modus Ponens*», ***Journal of Philosophical Logic*** 9 (1980), pp. 415-35). The gist of the argument is that saying 'We are more sure of the Truth of [**CA**] than of any theoretical account of logical truth' is, in the context of the present discussion, to beg the question.

Is it? Suppose — as Priest claims — that the two incompatible claims — **CA** on the one hand and the T schema and Comprehension on the other — are, as he puts it, obvious. The argument in support of **CA** points to the fact that it is **more** obvious. Again, I feel that, when degrees are ignored or overlooked, things go awry.

In the end, ***RLR*** rounds out the discussion of **CA** with a simple remark, 'observing the damage [**CA**] wreaks in paradoxical situations is enough to shake confidence in it and to begin to shift the onus of argument'. And the authors add that *Modus Ponens* 'no more supports its "normalization" [**CA**] than Material Detachment licenses Disjunctive Syllogism'. Quoting Russell they show that, unlike the rule of **MP**, **CA** merely requires the **hypothesis** that A is true; 'in short, it applies in situations beyond the actual one'.

The idea is that in any framework the world or setup, $\mathbb{w}$, which there plays the role of "the actual world", has to be closed for **MP** (i.e. if both ⌜p→q⌝ and ⌜p⌝ hold at $\mathbb{w}$, so does ⌜q⌝), but other worlds may fail to enjoy such closure. Is that idea plausible?

There are several usually accepted principles which taken together rule out the idea:

(**T1**) If an absurdity follows from a hypothesis, the hypothesis cannot be true.

(**T2**) What cannot be true is impossible (i.e. is not possibly true).

(**T3**) When a conclusion follows from a hypothesis, the possibility of the conclusion follows from the hypothesis being possibly true.

(**T4**) ⌜q⌝ follows from ⌜p→q∧p⌝.

(**T5**) It is absurd that ⌜p⌝ holds and that ⌜p⌝ [utterly] fails to hold.

(**T6**) If from a hypothesis several consequences follow, then from the hypothesis it also follows that the conjunction of those consequences Follows.

(**T7**) The possibility of an absurdity is absurd.

Suppose now (*Hyp*):

(*Hyp*) ⌜p→q∧p⌝ holds while ⌜q⌝ utterly fails to hold.

Obviously from (*Hyp*) it follows that ⌜p→q∧p⌝ holds; whence (by (**T4**)) it follows that ⌜q⌝ holds. Now from (*Hyp*) it follows, too, that ⌜q⌝ utterly fails to hold. The set of both consequences is an absurdity (by (**T5**)), which follows from (*Hyp*) (in virtue of (**T6**)). But then, in virtue of (**T3**) and (**T7**), the same absurdity follows from ◊(*Hyp*). Hence ◊(*Hyp*) cannot be true (by (**T1**)). Therefore (by (**T2**)) it is impossible that (*Hyp*) be possibly true.

It seems to me that among such assumptions what is rejected by deep relevantists is either (**T3**) or (**T7**). According to them it would indeed be absurd that ⌜p→q⌝ and ⌜p⌝ hold while ⌜q⌝ didn't [at all], but not that a situation should be **possible** wherein ⌜p⌝ and ⌜p→q⌝ hold but ⌜q⌝ doesn't [at all].

My reply is that, if without (**T3**) no clear sense of '**possible**' remains, and that without (**T7**) the very notion of absurdity vanishes.

What is right, though, is that the rule of **MP**, p→q, p ⊢q, allows us to draw ⌜q⌝ from the truthful assertability of ⌜p→q⌝ and ⌜p⌝, whereas **CA** does more than that. Thus, if we have a connective 'B' meaning 'It is truthfully assertable that' (with the rule p ⊢ Bp, even though we do not have ⊢ p→Bp), **MP** only requires a qualified version of **CA**, namely (**T8**):

(**T8**) ⌜Bp∧B(p→q)→Bq⌝

(**T8**) is equivalent to ⌜B(p→q∧p)→Bq⌝ ; whereas [unqualified] **CA** requires (**T9**), namely:

(**T9**) ⌜B(p→q∧p→q)⌝

(**T8**) is weaker. But as for rendering deliquescent any system with a full-fledged T schema or an unqualified Comprehension principle, (**T8**) would be enough. Thus weakening **CA** in such a way would be of no avail.

The comparison with **DS** seems to me wrong. No relevant similarity. If there are true contradictions — and, as Priest and I contend, some of them exist necessarily — then **DS** (or material detachment) is not a correct deductive inference rule, whereas **MP** (for implication '→') is.

Finally, against both Priest's argument and that of the authors of **RLR**, it must be emphasized that **CA** is more basic, more general. It is a principle of sentential logic. Admittedly, sentential logic is also liable to feedback from its applications; so there are reasons to qualify some principles of the sentential calculus in virtue of considerations pertaining to its applications in a number of fields. But there must be very very strong reasons for that. And whenever possible a distinction ought to be offered by which the junked principles can be retained under some reading (e.g. when we reject principles involving negation, a distinction between weak and strong negation allows us to keep all classical principles for strong negation). Sacrificing **CA** just for the sake of coping with Curry's paradox seems to me as *ad hoc* as anything can be in logic.

§6.— **Motion**

Traditionally, Zeno's paradox of the arrow has been associated with truth at intervals, not instants. Spinoza put it like that: there is no fixed unique position that the travelling body occupies at an interval, for the interval is made up of infinitely many subintervals, and the body is not at the same position at them all.

However, Priest argues for the contradictoriness of motion on the basis of instant-semantics (pp. 221ff). His version of the arrow argument hinges on the **spread hypothesis**, **SH**, (ibid.), namely:

SH A body cannot be localised to a point it is occupying at an instant of time, but only to those points it occupies in a small neighbourhood of that time.

Let us modify the wording by replacing 'point' and 'points' with 'place', or 'region', or the like (a body cannot be contained in a point).

What is the rationale for **SH**? Priest argues that the difference between motion and not-motion is that something like **SH** obtains for the former. A world wherein there would be a sequence of states cinematographically describable as if there were motion in it would all the same be a motionless world. I agree. A body is not moving if it is now here,

and only here, then there and only there, never really passing from here to there — never in a situation which can be described as intermediary between being here and being there.

Yet there are problems. Let us suppose b is travelling from its initial position p_1 to its destination p_n. At each instant i it occupies not just a position p_i but also positions which are in a neighbourhood of p_i — and which partly overlap p_i. Yet, since no degrees of truth are taken int   o account, b is equally at all those places. And since Priest tells us that the spread is bound to be small, for each point outside the series of stretches b occupies at i it is downright and wholly false that such a point lies in one of the position b occupies at i. There is a clearcut, crisp, trenchant boundary separating b's series of positions at i from the positions it does not have at i [at all].

All of that seems to me implausible. The rationale for taking motion to be contradictory was that we cannot ascribe to the travelling body a unique position at an interval. With instants that is not clear — although probably the best way to understand what "happens" at an instant is a derivative one. Since Priest himself allows of a consideration of intervals, let me henceforth regard **SH** with a further modification, reading 'interval' for 'instant'.

Now, b is travelling (with uniform speed) and its travel begins at noon and ends at 13pm. Let b be of length 1. At the interval I, 12:25—12:35, it occupies a number of partly overlapping positions, the stretch they form being that between points x and z. Let p_1 be a position of length 1 such that the distance between x and the first point of p_1 equals the distance between the last point of p_1 and z. Clearly p_1 is central. So at I as a whole b can more properly be said to be at p_1 than at any position lying outside p_1. To see this more clearly compare what happens at I with what happens at intervals 12:23-12:28 and 12:33-12:38. Positions corresponding to only one of those intervals are less typical of b at I than the central position p_1. Now, in accordance with **SH**, at I b has also positions (partly) overlapping the set of positions at some other intervals. It is clear that b is not to the same extent in all of them, but that the lesser the overlapping between position p and position p_1 the less true it is that b is at p at I. Yet, since the travel is (let us assume) uninterrupted, and there is no cut, probably b to some extent occupies at each subinterval of its travelling time each of the positions in the whole span of its trajectory — but to infinitely many degrees. Even should such a conjecture be downright false, there would still be no cut, no interruption, since the set of positions at I would partly overlap with that of positions at a contiguous interval — but unlike what happens with Priest's instant account, there would be differences of degree.

It seems to me that something along those lines would be more attractive than the jumps which after all Priest countenances, for within his original account, when moving, the body does not gradually decrease its

presence at some positions, gradually increasing its presence at other positions, but all of a sudden, at each instant, completely loses certain positions and completely acquires new ones, at all of which it lies, at such an instant, to the same extent.

We can put such a consideration in a different way. It was Spinoza too who wondered whether the body both acquires and loses a position at the same time. Motion can be taken to be just that. At each interval during its travel ꞗ acquires some position. Yet since it does not remain at that position, at that time interval it is already leaving it, or at least it is beginning to leave it. So it is not entirely at any position. Still, at the considered interval some of those positions are being acquired rather than left, and conversely. With truth degrees the story becomes smoother and more plausible.

I am not going to discuss some other sides of Priest's treatment of motion, like his remarks on the instant of change (pp. 200ff) — which could not be maintained without modification if strong negation was posited — or his elaboration on Leibniz's continuity principle (pp. 207ff) or his rejection of symmetry — the set of positions at an instant extending only on one side, to those of the past — which seems to me very Bergsonian — in order not to infringe the principle that what happens until a moment is independent of what happens afterward, a principle which of course Leibnizians and many other philosophers reject.

To sum up. I think Priest is right when he claims that without contradictions there would be no motion (although he only implicitly uses a principle to the effect that, in so far as a body has a position, it does not have other positions). But with just three truth-values a contradictorial account seems to me incredible and committed to a leap view of continuous motion.

§7.— **Juridical and deontic logic**

Chapter 13 (pp. 227ff) is given over to discussing legal and moral dilemmas and the contradictions they are supposed to yield.

I think that Priest is right when he claims that there exist such dilemmas and they entail the existence of true contradictions. Yet his arguments could be strengthened and improved upon.

One of the reasons his arguments on this subject are somehow weak is that he seems to share the view of such people as oppose the existence of moral dilemmas, namely that an overridden duty is no duty, or only a *prima facie* duty, or the like. Thus, if one law is of higher rank than another, or later, or can be plausibly interpreted as containing exceptive clauses which accommodate the law with which it clashes, then (pp. 233-4) there is only an apparent conflict. What Priest claims is that there is no guarantee that all apparent legal conflicts can be solved in any of those ways. I agree. There is in fact a lot of evidence that they cannot. Legal disputes will spring to the

mind which show to what a point a claim of hierarchical precedence is doubtful in a number of cases.

Yet the most important point is not that, but the fact that even the overridden claim or right is all the same a legal claim or right. True, jurists are so accustomed to reason in the classical terms of all or nothing that they fancy that only a complicated casuistry can decide what claims hold in the end, and which ones do not, the latter being then looked upon as no claims at all. They are wrong. Their mistake has tremendously serious practical consequences, since it further reinforces the erroneous rule of "all or nothing". In many cases, it is not a question of black and white. This is clear in international disputes, e.g. concerning border demarcations. And many legal paradoxes — which give rise to slippery slope arguments — invite a natural and sensible solution by avoiding hard lines, by choosing fringes, transitions, gradations.

Even if, all in all, country A has a stronger claim to this territory than country B, it does not follow that B's claim is [altogether] null and void. Some compromise may be negotiated which somehow reflects the different (grounded) claims instead of giving all to A and nothing to B. Many legal disputes ought to have sensible solutions by adopting scales of graded allocations — of ownership, or guilt, or whatever — rather than the dry all-or-nothing.

Juridic progress since the 18th century goes in that direction. It used to be the case that almost every infringement of the law entailed maximal penalty — the gallows or the galleys —, whereas our more civilized ways do in effect introduce a notion of degrees of guilt.

The reader has sensed what is the gist of my objection. Again. Ignoring degrees as the source of the true contradictions places us in a very difficult situation. If we want to prove that there are legal conflicts and we share the idea that inferior claims are no claims at all, we need to find cases where it can be shown that no claim is higher or of superior ranking. Although I am sure such cases exist, and are frequent, each of them will be contentious. Playing with where is the burden of the proof is not that interesting. The most important thing to say is that even when in fact one of the claims is overridden, it may be a good, *bona fide* claim, giving the claimant a partial right.

This is more evidently so in the case of moral conflicts. Even if, all in all, a course of action, A, is better and more dutiful than B, it does not follow that we are under no obligation **at all** to do B, that refraining from doing B is completely justified just because B clashes with a greater obligation, that of doing A.

The second reason why Priest's line of argument for moral and legal contradictions seems to me to be in need of an overhaul is that his choice of principles for juridical and deontic logic is not the best one. He adheres

to two principles (where '***d***' means 'It is a duty to do', '***p***' means that it is permissible or licit)

(**1**) $d\text{p} \wedge d\text{q} \rightarrow d(\text{p} \wedge \text{q})$

(**2**) $\text{p} \rightarrow \text{q}, d\text{p} \vdash d\text{q}$

He rejects:

(**3**) $d\text{p} \rightarrow p\text{p}$

And even (at least implicitly) weakened forms, like

(**3'**) $d\text{p} \Rightarrow p\text{p}$

I deem such a choice unfortunate. But I hasten to add that deontic logic is tricky and that such principles among those I now reject which are espoused in Priest's approach once seemed to me right.

The problem with (**1**) (**aggregation**) — which Priest discusses on pp. 238-9 — is that it may be to degree d obligatory to do p, obligatory to degree d' to do q, but not obligatory at all to do both, if doing both is altogether impossible (perhaps not metaphysically impossible in the sense of an abstract possibility but concretely impossible). This is clear in the case of moral dilemmas. I am obliged to rescue Julia, also to rescue Mariana, but not both, which may be utterly impossible. Perhaps Julia's claim is higher, perhaps both claims are equal. Whatever I do — since I have not had moral luck — I fail an obligation. But, please, do not blame me for failing to rescue both Julia and Mariana. Do not blame me for failing to help hungry people in Sudan, and in Ethiopia, and in Peru, and in Mozambique, and in Tchad, and in Haiti, and… Blame me for each of those failings separately, not for failing to do the impossible.

Blame the famous young man of Sartrean memory for failing his patriotic duty if he gives preference to his mother, or his filial duty otherwise. Do not blame him for this: failing to comply both with his patriotic and his filial duties.

As for (**2**), or the closure rule, it gives rise to paradoxes such as the good Samaritan, which Priest thinks can be solved through scope distinctions. I (now) disagree. Many of those distinctions become unbelievable epicycles, while new counterexamples arise which challenge the principle. Yet if we jettison (**2**) we need something in its place. The main idea is that you are not allowed to do A if doing A prevents someone from fulfilling his duty, or from enjoying his rights. So a causal connection seems to me to be involved here, with a resulting principle of non-hindrance — or something like that.

Priest's argument against (**3**) is that it multiplies contradictions. Since he adheres to the maxim of avoiding contradictions as far as possible (see below, §9), his line here is parallel to his distrust towards the exclusion principle we have gone into above.

But suppose Jonathan is obliged to A and yet also not permitted at all to A. That means he is bound to A and also completele bound not to A. That is certainly impossible. A course of action is wholly and fully obligatory only if its negation is not obligatory at all.

What is more, without (**3**) there is no good reason for thinking that moral or legal conflicts entail true contradictions. Priest broaches this subject (pp. 230-1) and replies that, without (**3**), there may be other reasons why legal conflicts give rise to contradictions. E.g. if **X** enjoys legal priority — in virtue of some legal disposition — and yet **Y** also enjoys priority — in virtue of a different disposition —, we can infer that **Y** does not enjoy priority (since **X** does) and likewise **X** does not enjoy priority (since **Y** does). Each of them both has and lacks priority.

But why? Doubtless a principle is being assumed, something like this: 'If somebody else has priority, you do not have priority'. But what is the rationale? And what is its general formulation — applying to other matters, not just to cross-road driving priority? I can only figure out variations of (**3**). The idea is that, since **X** has a right to cross before **Y**, **X** is not entirely bound not to cross before **Y** does — i.e. **X** is not wolly bound to defer to **Y**'s priority; and so, since **Y** has [to some extent] priority, i.e. the right to cross before **X** does, and **X** must (up to a point) defer to such a right, **X** both has and does not have the duty to give way to **Y**.

§8.— **Other grounds for true contradictions**

Although Priest tends to focus on *a priori* grounds for "dialetheias" — except for motion and moral and legal dilemmas — at one point at least he broaches a different reason, namely complying with only some among a variety of criteria for satisfying a predicate. He offers two illustrations, one about temperature, the other on left vs right in politics. The former seems to me extremely dubious but I do not want to discuss it. As for the latter, the idea is that if being left-wing in politics is complying with conditions c_1, …, c_n, and being right-wing is complying with **not-c_1**, and, …, and **not-c_n**, then a group **G** which for some $1<j<n$ satisfies c_1, …, c_j, **not-c_{j+1}**, …, **not-c_n** will be both left and not left.

I guess that only a few classicists will be convinced by such an argument. They are likely to rejoinder that being left and being right are contraries, not contradictories, and that **G** is neither. Call such a reply the "neitherist" ploy.

The trouble with such a reply is that it waives the principle that, in so far as a political group is not right-wing, it is left-wing. If whatever is neither belongs to the "centre", then surely it is going to be very hard to find political groups outside the "centre". In fact, the set of requirements on criteria is open-ended. Very often left-wingers have conservative leanings as regards sexual morality, and sometimes family relations, etc. On economical issues, some right-wingers may fail to support purely private free market

economies, while preferring some sort of regulated market, or the like. In fact there are not many people who qualify as either left-wingers or right-wingers on all and every demarcation criterion. Thus, the neitherist ploy, together with the facts of the matter, entails that there is almost no right and no left. And about the same could be said for many a similar classification (rich/poor, pleasant/unpleasant, literate/illiterate, rural/urban, ancient/modern, and so on).

The problem with Priest's treatment is again that it does not allow of degrees. But suppose that G_0 satisfies all criteria on left-wing-ness; G_1 complies with all except two (it, e.g., opposes abortion), whereas G_2 fails those two criteria and moreover a 3d one (it does not favour free compulsory education for all, or only with a number of restrictions); G_3 shares all the positions of G_2 except that it advises private ownership of means of production within strict limits; G_4 has the same position as G_3 except that the limits for private ownership are higher and the regulations looser, and so on; at the other end, group G_n satisfies all conditions for right-wing-ness, but G_{n-1} favours some restraints on private ownership, and so on. The natural thing to say is that G_n is wholly right-wing, G_{n-1} less right-wing than G_n is, … G_2 less right-wing than G_3 but more so than G_1, … and G_0 completely left-wing. (Of course almost everybody is in between the extremes.)

Moreover, it is not a matter of either entirely satisfying a criterion or else completely failing to satisfy it. There are degrees. But even if the criteria were so crisp that they did not admit of degrees there would be an "all-in-all" consideration. Of course it may happen that in some respects **G** is more right-wing than **G'**, in some other respects **G** is less right-wing than **G'**. (If truth-values are scalar, rather than tensorial, such a possibility is hard to be accounted for.) But in a number of cases some "all-in-all" consideration is in order and plausible. (Otherwise we could say nothing about whether a colleague is a good academic, a student is promising, a radio broadcast interesting, a scientific theory innovative, a software program useful, and so on, except that «in some respects it is, in other it isn't [at all?]». In many such cases an "all things considered" viewpoint is permissible and based on what things are, and what emerges is that, all in all, this colleague is a better academic than that one, this software programme is better than that one, and so on. Not an undifferentiated magma of "good-and-not-good". Almost everybody is good and not good, but doubtless some people are worse than others.)

Doing with just the trichotomic classification Priest offers us is not going to successfully cope with anything of the sort. We'd be bound to regard all groups in between G_0 and G_n as equally left-and-not-left, none being more right-wing than any other. I do not deny that such a trichotomy is an improvement over the classical all-or-nothing approach. Yet, is the improvement really a great one? I do not think so. In some sense, it still is an all-or-nothing approach: if two groups are such that neither complies with all conditions for being right-wing and yet also neither complies with all

conditions for being-left-wing, then — within Priest's approach — they are on the same footing, with nothing being said of the one which is not said of the other, too. ('Less… than' is not an expression of which Priest's logical theory takes any notice.)

Adding **grey** to **black** and **white** is fine. But of course it is not enough. There are degrees. Some grey things are blacker than others. And 'less… than' is a matter of logic, since there are logically valid inferences essentially involving comparatives.

§9.— **The Confinement Policy and Using DS**

There are two ways of drawing a line between the contradictions we want to uphold and those we do not want. One of them is to rule that such contradictions as involve strong negation — i.e. **over**contradictions — are beyond the pale, and deserve rejection. The other ones are logically unobjectionable. (But notice that a formula of the form ⌜p∧~p⌝ may be an overcontradiction, even if '~' is natural negation — namely, if ⌜p⌝ is of the form «To some extent, q».) That line is not taken by Priest, since he tries to avoid strong negation — although, as I argued before, he seems to be committed to accepting strong negation, which is definable in his whole system, once Peano arithmetics and semantical predicates are introduced. Thus he opts for the alternative policy, namely to avoid contradictions as far as possible. That is the purpose of his Methodological Maxim (**M**) (p. 145), viz.:

> (**M**) Unless we have specific grounds for believing that the crucial contradictions in a piece of quasi-valid reasoning are dialetheias, we may accept the reasoning.

I do not want to tarry on the details here. The idea is as follows. Suppose from set A of premises a conclusion can be validly deduced to the effect that ⌜p∨.q∧~q⌝, with some constraints being imposed which avoid that irrelevant contradictions ⌜q∧~q⌝ creep in (p. 150). When such constraints are complied with, the contradictions involved are crucial. Then the quasi-valid reasoning allows us to draw conclusion ⌜p⌝ forthright from set A. And the rationale is that more often than not true contradictions fail to arise. As Priest puts it (p. 144):

> The reason is a simple one: the statistical frequency of dialetheias in normal discourse is low. Dialetheias appear to occur in a quite limited number of domains: certain logico-mathematical contexts, certain legal and dialectical contexts, and may be a few others.

My first comment on such an argument is that, if the kind of examples of true contradictions which were provided above, in §8, is right, then true contradictions pop up in all domains. Not "a few others". No domain exists in which true contradictions do not arise.

My second comment is that, if contradictions are admitted in the field in which they were supposed to be least likely to arise and most damaging

— mathematics — there is no good reason for us to be coy about them in other domains. Of course, it just might be the case that contradictions were true only where they were expected the least to arise. Some highly improbable things happen sometimes. Yet very often when situations of some kind emerge even where they were the least expected, they are likely to occur elsewhere, in many other domains. E.g. actual infinities were supposed to be ruled out in virtue of Euclides' principle that the whole is greater than any part thereof. Once they arose in mathematics, with the calculus, they had to be admitted in all domains — although nowadays some people think that quantum mechanics has again dislodged them from physics, it remains to be seen how long people are going think that.

My 3d comment is that, if contradictions are not bad — in general — there is no reason to be afraid of them. If they arise in the *sanctum sanctorum*, mathematics, surely it is not irrational to have a contradictory belief. Then when people say that it rains and it does not rain, that the man yonder is and is not bald, that this paper is and is not white, that such a course of action is and is not dangerous, etc., why are we bound to construe what they say in devious ways or to scorn them for their purported irrationality?

My 4th comment is that empirical evidence shows an enormous amount of utterances which are literally contradictory. Philosophers used to allege that, duly (charitably) construed, they were not. But such an exegetical approach was enforced by the view of contradictions as horrible, awful, irrational, utterly rejectable. If there are true contradictions — and in mathematics! — surely it is not irrational to have contradictory beliefs. Thus no need for charity here. There may be special and cogent reasons why some apparently contradictory utterances are, upon consideration, taken not to be genuinely contradictory. But the huge amount of literal contradictions people utter in everyday communication seems to render implausible the idea that all or most of them deserve to be paraphrased away in a charitable manner. Priest advises us — ibid — to 'consider a random sample of the assertions [we have] met in the last few days and see what percentage might reasonably be thought to be dialetheic'. My own — fallible — assessment yields a high percentage — esp. on radio interviews, very often when academics have to answer questions. (Ill-formulated questions, which debar the interviewed person from a simple 'Yes' or 'No' answer? Maybe. Yet, …)

In addition to the just considered argument that contradictions are infrequent, Priest offers a second argument (p. 144-5) to the effect that they are unlikely and so (**M**) is broadly reliable: the sheer fact that people reason using **DS**. If quasi-valid reasonings were widely unreliable, a lot of damage would surely follow.

Nevertheless, the argument assumes that uses of **DS** involve natural or weak negation rather than strong negation. Now, in spoken language, 'not' — with perhaps a prosodic or suprasegmental signal, which stands for the modifier 'at all' — may also express strong negation. It depends on the

context. Written language lacks prosodic resources — only a pale reflection of them is available, but with nothing like the richness of speech. Perhaps this is one of the reasons which have led to overlooking the difference between strong and natural negation, and so that between overcontradictions and simple contradictions.

§10.— **Conclusion**

The reader has rightly realized that, despite my objections, I share most of G. Priest's views and many of his arguments — with mitigations. It is very probable that the reader disagrees with us both, finding all contradictions inadmissible. What cannot be said, though, is that the subjects G. Priest studies in his book are of no importance for contemporary philosophy. In fact, I cannot see any subject more important than the question of whether or not there are true contradictions. Priest's book can be ignored by no one. Fervent adherents of Aristotelian logic are invited to take it seriously, and to discuss it rationally — not to rend their garments.[3]

Lorenzo Peña

Spanish Institute for Advanced Studies (CSIC)

Madrid, Spain

<laurentius@pinar1.csic.es>

SORITES

NOTES TO POTENTIAL CONTRIBUTORS

All submitted manuscripts will be refereed either by members of the Board of Advisors or by other specialists; as far as possible, each manuscript will be refereed by philosophers not unsympathetic to the paper's philosophical outlook or orientation.

No manuscript may be submitted if it is being considered for publication elsewhere.

Once accepted, papers may not be printed or displayed elsewhere or incorporated into a book, an anthology or any other publication of any sort until the **SORITES** team has accorded the author(s) permission to that effect — which in normal cases will be done routinely, provided **SORITES** is duly acknowledged as the primary source. By submitting a paper, the author agrees to the points, terms and conditions contained in the Copyright Notice included in each issue of **SORITES**.

All submitted papers must be written in English. The author's local variety of English (including the spelling) will be respected — be it Indian, Filipino, Australian, American, Western-African, British, Southern-African, Eastern-African, Jamaican, etc. All editorial material will be written in BBC English, which is the journal's «official» dialect.

There is no settled length limit for papers, but we expect our contributors to stand by usual editorial limitations. The editors may reject unreasonably long contributions.

We expect every submitted paper to be accompanied by a short abstract.

We welcome submissions of in-depth articles as well as discussion notes.

Ours is a journal granting a broad freedom of style to its contributors. Many ways of listing bibliographical items and referring to them seem to us acceptable, such as '[Moore, 1940]', or '[M:5]' or '[OQR]'. What alone we demand is clarity. (Thus, for instance, do not refer to '[SWT]' in the body of the article if no item in the bibliography collected at the end has a clear '[SWT]' in front of it, with the items sorted in the alphabetic order of the referring acronyms.) We prefer our contributors to refer to 'Alvin Goldman' rather than 'Goldman, A.', which is obviously ambiguous. We dislike

implied anachronisms like [Hegel, 1989]' or '[Plato, 1861]' — but you are entitled to ignore our advice.

How to submit?

(1) We will be thankful to all contributors who submit their papers in the form of [I.B.M.-PC] WordPerfect 5.1 files. There are several convertors which can be used to turn docs from other word processor formats into WP5.1 format. (Notice that with WP5.1 you can write not only almost all diacritically marked characters of any language which uses the Latin script, but moreover all of Greek and virtually all symbols of mathematical logic and set theory.)

(2.1) In case a contributor can neither use WP5.1 nor have their doc converted into WP5.1 format, they can send us their file in its original format (be it a different version of WordPerfect or another sort of word-processor, such as MS-Word, MS-Word for Windows, WordStar, AmiPro, XyWrite, DisplayWrite, .rtf, etc). We'll try (and hopefully in most cases we'll manage) to convert those files from other formats into WordPerfect 5.1.[1]

(2.2) When WP5.1 format is not available and we have been unable to use the original file, a good idea is for the author to have their doc converted to a .html file (there are lots of HTML editors and document-to-HTML converters from a great many formats — PC-Write, [La]TeX, MS-Word and Windows-Word etc). We expect HTML files to bear the extension '.htm'.[2]

(2.3) Another solution is to use [stripped and extended] ASCII format, which means: text files (not binary ones) written using any printable ASCII characters of Code-page 437 (USA or default), i.e. any character except ASCII_00 through ASCII_31; with CRs (carriage returns) only between paragraphs — not as end-lines. Such files will here be called 'ASCII files'. We expect them to bear the extension '.ASC'.

(2.4) Another alternative (which is in itself worse, but which nevertheless may be more practical in certain cases) is to use the DOS text format, with no character outside the range from ASCII_32 through ASCII_126, no hyphenation, a CR at the end of each line and two CRs separating paragraphs. Such files will be here called 'text files'; we expect them to bear a '.txt' extension.

---

[1]   Unfortunately we cannot yet handle TeX or LaTeX files. The convertors we've tried have proved useless.

[2]   At our home site, **ftp.csic.es**, there is — hanging from our main directory **/pub/sorites** — a subdirectory, **WWW**, which, among other files, contains one called 'HTML.howto', wherein the interested reader can find some useful information on HTML editors and convertors.

(3) In cases (2.2) and (2.4) the contributor can include their paper into an e_mail message sent to our editorial inbox (<sorites@fresno.csic.es>)

(4) Before sending us their file the contributor is advised to compress it — except in case they are sending us a text file through procedure (3) above. Compression reduces disk-storage and shortens transmission time. We can extract and expand files archived or compressed with Diet, ARJ (both warmly recommended), Tar, Arc, Zip (or PKZip), GZip, Compress (i.e. .Z files), LHA, Zoo, RaR, and some versions of the MAC archivers PackIT and StuffIT.

(5) The most expedient way for contributors to send us their submitted paper is through anonymous FTP. At your host's prompt, you enter 'ftp ftp.csic.es'; when you are prompted for your username, you answer 'ftp' or 'anonymous'; when you are next prompted for your password, you answer with your e_mail address; once connected, you enter 'cd pub/sorites/incoming', then 'binary', and then 'put xxx' — where xxx is the file containing your submitted paper and a covering letter. (If the file is an archive, the extension must reveal the archiving utility employed: '.gz', '.Arj', '.RAR', etc. (DIETed files needn't bear any special denomination or mark; they will always be automatically recognized by our reading software.)

(6) Whenever a paper is submitted, its author must send us a covering letter as an e_mail message addressed to one of our editorial inboxes.

(7) If a contributor cannot upload their file through anonymous FTP, they can avail themselves of one of the following alternatives.

(7.1) If the file is a '.htm' or a '.txt' file (i.e. in cases (2.2) and (2.4)), simply include it into an e_mail message.

(7.2) In other cases, an 8-to-7 bits converter has to be used, upon which the result can also be included into an e_mail message. 8-to-7 bits convertors «translate» any file (even a binary file) into a text file with short lines which can be e-mailed. There are several useful 8-to-7 convertors, the most popular one being UUenCODE, which is a public domain software available for many different operative systems (Unix, OS/2, DOS etc). Perhaps the most advisable at this stage is PGP ['Pretty Good Privacy'], which also allows authentication (signing). Another good such convertor, very easy to use, is Mike Albert's ASCIIZE. We can also decode back into their binary original formats files encoded into an e-mailable ASCII format by other 8-to-7 bits convertors, such as: Mime, TxtBin, PopMail, NuPop, or University of Minnesota's BINHEX, which is available both for PC and for Macintosh computers. Whatever the 8-to-7 bits encoder used, large files had better be

previously archived with Arj, Diet or any other compressor, the thus obtained archive becoming the input for an 8-to-7 bits convertor.[3]

(7.3) An alternative possibility for contributors whose submitted papers are WordPerfect 5.1 or WordPerfect 6 docs is for them to use a quite different 8-to-7 bits convertor, namely the one provided by the utility Convert.Exe included into the WordPerfect 5.1 package. (WordPerfect corporation also sells other enhanced versions of the convertor. WordPerfect 6.0 has incorporated a powerful conversion utility.) A separate e_mail message is mandatory in this case informing us of the procedure. The result of such a conversion is a 'kermit-format' file.[4]

(8) You can also submit your manuscript in an electronic form mailing a diskette to the Editor (Prof. Lorenzo Peña; CSIC, Institute of Philosophy; Pinar 25; E - 28006 Madrid; Spain.) Diskettes will not be returned.

---

[3] For the time being, and as a service to our readers and contributors, we have a directory called 'soft' hanging from our home directory /pub/sorites at the node ftp.csic.es. The directory contains some of the non-commercial software we are referring to, such as archivers or 8-to-7 encoders (or 7-to-8 decoders).

[4] In the case of WordPerfect 5.1, the procedure is as follows. Suppose you have a file called 'dilemmas.wp5' in your directory c:\articles, and you want to submit it to **SORITES**. At your DOS prompt you change to your directory c:\articles. We assume your WordPerfect files are in directory c:\WP51. At the DOS prompt you give the command '\wp51\convert'; when prompted you reply 'dilemmas.wp5' as your input file whatever you want as the output file — suppose your answer is 'dilemmas.ker'; when prompted for a kind of conversion you choose 1, then 6. Then you launch you communications program, log into your local host, upload your file c:\articles\dilemmas.ker using any available transmission protocol (such as Kermit, e.g.). And, last, you enter your e_mail service, start an e_mail to to <sorites@fresno.csic.es> and include your just uploaded dilemmas.ker file into the body of the message. (What command serves to that effect depends on the e_mail software available; consult your local host administrators.)

　　With WordPerfect 6 the conversion to kermit format is simple and straightforward: you only have to save your paper as a 'kermit (7 bits transfer)' file.

SORITES

An Electronic Quarterly of Analytical Philosophy

Copyright Notice and Legal Disclaimer

team is required for such a display, which may be in the form of FTP, Gopher, http-WWW or any other electronic means.

Madrid. April 10, 1995

(Updated December 31, 1996)

The **SORITES** Team

ANNOUNCEMENTS
_____

FOURTH INTERNATIONAL WORKSHOP ON DEONTIC LOGIC IN COMPUTER SCIENCE
(DEON '98)
Bologna, Italy, 8-10 January, 1998
First CALL FOR PAPERS

The biannual DEON workshops are intended to promote research and cooperation in a rapidly expanding interdisciplinary area, linking the formal study of normative concepts and normative systems with computer science and artificial intelligence. This area now commands the attention of a variety of researchers: some are interested in formal analyses of normative concepts and normative systems per se, such as legal theorists, deontic logicians and formal semanticists, and some are interested in applications of such analyses in AI models of normative reasoning, or in formal models of norm-governed behaviour of computer systems (including their interaction with humans and other computer systems). So far three DEON workshops have been held: in Amsterdam, December 1991, Oslo, January 1994 and Sesimbra, January 1996. With the fourth edition (DEON '98) we hope to further the development of this exciting interdisciplinary field. We intend to have the proceedings from the workshop published by an international publisher.

The Program Committee invites papers concerned with:

(a) any theoretical aspects of the logical study of normative reasoning, including:
— formal systems of deontic logic, logic of action, or other areas of logic, provided that their connections with deontic logic or normative reasoning are made clear;
— Formal analysis of normative concepts and normative systems.

or

(b) any logical aspects of Artificial-Intelligence models of normative reasoning, such as;
— formal representation of legal knowledge, contracts or other regulations;
— formal analysis of defeasible normative reasoning

or

(c) any aspects of the application of logical systems to normative aspects of computer science and public or private administration, including:
— formal specification of normative systems, comprising artificial and/or human components
— formal specification of systems for management of bureaucratic processes
— formal analysis of database integrity constraints or aspects of security
— formal representation of agency, norm-governed interaction, power, authorization, delegation, and responsibility
— deontic aspects of protocols for communication, negotiation and multi-agent decision making.

A special workshop session is planned in honor of the late Carlos Alchourron, with an invited talk by David Makinson. Therefore the program committee also invites the submission of papers on issues which Carlos Alchourron studied during his lifetime, such as:
— Logical characterization of normative systems, including their dynamic aspects
- Logic of norms without truth
- The logic of defeasible conditionals

We would also welcome papers that critically discuss Carlos Alchourron's own work in these areas.

SUBMISSION DETAILS

Authors are invited to submit five hard copies or one electronic PostScript version of original, unpublished papers, written in English, and not exceeding 7500 words, to the address specified below. Each copy must have an initial page containing: paper title; names, addresses, and if possible e-mail addresses and FAX numbers of all authors; an abstract of no more than 10 lines; and a list of content areas.

Authors wishing blind reviewing of their paper should make the initial page separate from the rest of the paper, and start the second page with the same title, abstract and list of content areas, but with no self-identifying references from that page forward.

ADDRESS FOR PAPER SUBMISSIONS:

Hard copies:
Paul McNamara
Department of Philosophy, University of New Hampshire
Durham, NH 03824, USA.
Postscript version: <paulm@christa.unh.edu>

IMPORTANT DATES:

Deadline for submission of papers: May 15, 1997;
Notification of acceptance: July 15, 1997;
Workshop: January 8-10, 1998.

PROGRAM COMMITTEE CO-CHAIRS:

Henry Prakken, c/o Faculty of Law, Free University Amsterdam, The Netherlands. Tel. +31-20-44-46216 Email: henry@rechten.vu.nl.

Paul McNamara, Department of Philosophy, University of New Hampshire, Durham, NH, USA. Tel. 603-743-4288. Email: paulm@christa.unh.edu

PROGRAM COMMITTEE:

Mark Brown (Syracuse University); Daniel Bonevac (University of Texas at Austin); Jose Carmo (Technical University Lisbon); Frank Dignum (Technical University Eindhoven); John Horty (University of Maryland); Andrew Jones (University of Oslo); Lars Lindahl (University of Lund); Tom Maibaum (Imperial College London); John-Jules Meyer (Utrecht University); Giovanni Sartor (Queen's University Belfast); Krister Segerberg (Uppsala University); Marek Sergot (Imperial College London); Lennart Åqvist (Uppsala University).

ORGANIZING COMMITTEE:

Giovanni Sartor (Queen's University, Belfast) (Chair); Alberto Artosi (University of Bologna); Guido Governatori (University of Bologna).

INVITED SPEAKERS:

Donald Nute (University of Georgia); David Makinson (UNESCO, France); Georg Henrik von Wright (University of Helsinki).

MORE INFORMATION:

For more information, contact one of the program chairs, or consult the workshop WWW page, at <http://www.cirfid.unibo.it/~deon98>

♣ ♣ ♣ ♣ ♣ ♣ ♣

PHILOSOPHICAL PAPERS

Philosophical Papers is an international journal based in South Atrica which is issued three times a year and publishes papers in all branches of Philosophy within the broad analytical tradition.

EDITOR: Michael Pendlebury (University of the Witwatersrand).

EDITORIAL CONSULTANTS: David Armstrong, Tyler Burge, Romane Clark, Max Cresswell, Brian Ellis, Frank Jackson, Jaegwon Kim, David Lewis, John Perry, Philip Pettit, Mark Sainsbury, John Searle, Ernest Sosa, Charles Taylor. Richard Wasserstrom. Eddy Zemach.

SOME RECENT AND FORTHCOMING ARTICLES:

Terry Horgan and Mark Timmons «Taking a Moral Stance»

Tomis Kapitan «Intentions and Self-Referential Content»

Alfred R. Mele «Socratic Akratic Action»

Glen Newey «Reasons Beyond Reason? 'Political Obligation' Reconsidered»

Tony Skillen «Passing Likeness»

Andrew Woodfield «Which Theoretical Concepts Do Children Use?»

SUBSCRIPTIONS:(1997 rates)

Institutions US $66, Individuals US $22, Students US $14 (or equivalent amounts in other tradable currencies). Send to:

Philosophical Papers

Rhodes University,

P.O. Box 94

Grahamstown, 6140

South Africa.

Books Received

Brandl, J. L. / Hieke A. / Simons P. M. (Eds.), *Metaphysik. Neue Zugänge zu alten Fragen*. Sankt Augustin: Academia Verlag, 1995.

Halbach, Volker, *Axiomatische Wahrheitstheorien*. Berlin: Akademie Verlag, 1996.

Jarvie, I. C. and Laor, Nathaniel (Eds.), *Critical Rationalims, Metaphysics and Science. Essays for Joseph Agassi. Volume I*. Dordrecht: Kluwer Academic Publishers, 1995.

Jarvie, I. C. and Laor, Nathaniel (Eds.), *Critical Rationalims, the Social Sciences and the Humanities. Essays for Joseph Agassi. Volume II*. Dordrecht: Kluwer Academic Publishers, 1995.

Leonargi, Paolo and Santambrogio, Marco, *On Quine. New Essays*. Cambridge: Cambridge University Press, 1995.

Peacocke, Christopher (Ed.), *Objectivity, Simulation and the and the Unity of Consciousness. Current Issues in the Philosophy of Mind*. Oxford: Oxford University Press.

Priest, Graham, *Beyond the limits of thought*. Cambridge: Cambridge University Press, 1995.

Sachs-Hombach, Klaus (ed.), *Bilder im Geiste. Zur kognitiven und erkenntnistheoretischen Funktion piktorialer Repräsentationen*. Amsterdam - Atlanta, GA.: Rodopi, 1995.

Shoemaker, Sydney, *The First-Person Perspective and Other Essays*. Cambridge: Cambridge University Press, 1996.

Sluga, Hans and Stern, David G. (Eds.), *The Cambridge Companion to Wittgenstein*. Cambridge: Cambridge University Press, 1996.

Smith, Barry and Woodruff Smith, David, *The Cambridge Companion to Husserl*. Cambridge: Cambridge University Press, 1995.

Suppes, Patrick and Zanotti, Mario, *Foundations of probability with applications. Selected papers, 1974-1995*. Cambridge: Cambridge University Press, 1996.

RELEASE NOTICE

This issue of *SORITES* is made available in several formats, but its only official version is that released with filename:

sorite07.wp5

which is the only file within the archives: **sori07wp.zip**, **sori07wp.arj**, **sor#07wp.gz**, etc. A print-file (**sorite07.ps**), also released, and generated from the file **sorite07.wp5** can be found in the archive **sori07ps.zip** and printed with a PostScript printer.

Two whole «doc» versions of this issue of *SORITES* are provided, but they cannot truly or faithfully reflect the official WordPerfect 5.1 version, departing as they do from the authorized WP 5.1 document — in a more or less severe way, depending on the particular case. One of them, **sorite07.html** (or its equivalent, **sorite07.htm**), is an hypertext HTML version chiefly destined to be desplayed at the InterNet Web and viewed with http browsers. The other, **sorite07.txt**, is an empoverished version, with only ASCII symbols <Alt-32> through <Alt-126> being used, and a CR at the end of each line. Those two versions are archived, respectively, as **sor07htm.zip** and **sor07txt.zip**.

Several of those files are made available in UUenCODed (.UUE) and BinHexed (.HQX) translations, in order for them to be attached to e-mail messages.

Although each version, whether official or not — as initially released today (on Thursday 16 January 1997) by the **SORITES** team — is an entire seamless file, it may be splitted down into chunks in order to facilitate downloading, browsing, transferring or e-mailing. In such cases, the unity of this issue of **SORITES** as a whole must be preserved by keeping the ensuing collection intact.