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DEVIANT LOGIC AND THE PARADOXES OF SELF REFERENCE

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Letting a hundred flowers blossom
and a hundred schools of thought contend
is the policy for promoting the progress
of the arts and the sciences.

— Mao Tse-Tung

Dedicated to Bob Meyer on the occasion of his 60th Birthday

ABSTRACT. The paradoxes of self reference have to be dealt with by anyone seeking to give a satisfactory account of the logic of truth, of properties, and even of sets of numbers. Unfortunately, there is no widespread agreement as to how to deal with these paradoxes. Some approaches block the paradoxical inferences by rejecting as invalid a move that classical logic counts as valid. In the recent literature, this 'deviant logic' analysis of the paradoxes has been called into question.

This disagreement motivates a re-examination of the philosophy of formal logic and the status of logical truths and rules. In this paper I do some of this work, and I show that this gives us the means to defend the 'deviant' approaches against such criticisms. As a result I hope to show that these analyses of the paradoxes are worthy of more serious consideration than they have so far received.

One thing that Australia is growing to be famous (or infamous) for in the logical community is logical *deviance* — a phenomenon which is not easy to characterise, but which at least features an iconoclastic attitude towards classical logic. Australia is one of the centres of research into paraconsistent logic and we have on our shores some of the best relevant logicians alive. Exactly why this is so is a matter I'll gladly leave to others, except to observe that the study of non-classical logics is a great deal of fun. Evidence of this can be gleaned from any of Bob Meyer's papers. This is one of the joys of working in the area, but it is also a hazard because we 'deviants' can forget to explain to outsiders what is going on. More of our energy is spent on the task of finding a pleasing semantics for **RQ**, seeing how strong your logic can be while

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retaining a non-trivial naïve set theory, or discovering the idiosyncracies of an inconsistent arithmetic with only 3088 numbers.

This is not to say that the nuts and bolts of an apologetic for deviant logic haven't been worked out — they have. But they often appear in articles primarily about something else (Slaney 1991, Meyer and Martin 1986) or hidden in jokes that only an insider would appreciate, or they're written in a too-polemical fashion. None of this is helpful to the outsider looking in, who wonders what the fuss is about. This paper is intended to be a user-friendly introduction to a philosophy of logic that is behind some of this work on deviant logics, and a defence of the deviant position against those who take it to be misguided.

Another aim of this paper is to thank to Bob Meyer, whose work has not only shown me that there's something good about logical deviance, but that there's something good about having a laugh while you're doing it.

1. PARADOXES

The paradoxes of self-reference have provided one of the driving forces behind much of the semantic and set theoretic enterprise of the twentieth century. To the paradoxes we owe the type hierarchy of Russell and Whitehead, Zermelo-Frænkel set theory, and the Tarskian hierarchy of languages and truth predicates. These theories owe their central features to their own particular response to the paradoxes. Paradoxes provide the data that prospective theories must deal with. This sentiment was expressed by Bertrand Russell (1905).

> It is a wholesome plan, in thinking about logic, to stock the mind with as many puzzles as possible, since these serve much the same purpose as is served by experiments in physical science.

In this paper I will attempt to show that the puzzles provided by the paradoxes of self-reference don't just provide us with material useful in formulating theories in semantics and set theory — they also give us good reasons to explore logical systems that deviate from classical logic in particular ways. The paradoxes give us reason to hold that classical logic is not a good candidate for modelling valid inference.

Many definitions of the term "paradox" have been proposed, and we will do well to get the term clear. The definition I will work with is this:

Definition A *paradox* is a seemingly valid argument, from seemingly true premises to a seemingly unacceptable conclusion.

So, such disparate things as the paradoxes of self-reference, the sorites paradox (sometimes called the paradox of the heap), and the paradoxical decomposition of spheres are counted as paradoxes. Consider the paradoxical decomposition of spheres, which shows that it is a consequence of the axiom of choice that one solid sphere can be decomposed into a finite number of pieces, which can then be reconstructed into two solid spheres each of the same volume as the first. This example shows that a paradox does not have to be an argument to a *contradiction*, any seemingly unacceptable conclusion will do.

Given such a conception of paradoxes, we can see that a paradox can elicit any of a number of responses. The three viable ones are to:

(1) Explain why a premise is false.

(2) Explain why the argument isn't valid.

(3) Explain why the conclusion is acceptable.

All three approaches have been taken in the history our dealings with paradox. (Finding examples is an exercise left for the interested reader.)

2. THE LIAR

The paradoxes of self-reference are a particular group of paradoxes that each combine the apparatus of self reference with notions from semantics or mathematics to give untoward conclusions. We will focus on the liar paradox, although what we have to say can be adapted to other paradoxes in a relatively painless fashion. It is natural to hold that a theory of truth will give us

$$\vdash T\langle p \rangle \leftrightarrow p$$

for all sentences p, where $\langle p \rangle$ is a name of the sentence p.[1] This condition seems to capture at least some of what we mean by "true".

An assertion "$\langle p \rangle$ is true" seems to have the same inferential force as the assertion "p". The claim "Everything Bob Meyer said today is true" has just the same inferential force as asserting everything Bob said today. This assumption about truth leads to paradox, given a few reasonably plausible and well known steps. One culprit is the putative statement:

$$(\lambda) \text{ is not true} \qquad (\lambda)$$

Given our condition on truth it seems quite easy to deduce a contradiction to the effect that (λ) is both true and not true. So much is well known.

3. ORTHODOX PARADOX SOLUTIONS

There is almost universal agreement that no *obviously* correct account of the self referential paradoxes has yet surfaced. The central thrust of much of the current research in paradoxes is to attempt to deal with them inside the framework of classical logic. These solutions fall into two classes. The solutions in one class deny that the liar makes a statement. Others accept that we can state things using the liar, but that this does not lead to paradox. In what follows, I sketch an account of the shortcomings of these approaches.

'Non Expressible' Solutions

This class of solutions to the paradoxes attempt to formulate some reason why the paradoxical sentence cannot enter the realm of valid inference. They generally fall into the class of paradox solutions that seek to explain why a premise is false: in this case, the premise that (λ) is (or expresses) a proposition. Typically, this is attempted by way of a syntactic theory that counts the expression as not well-formed. As such, it gets off on the back foot, for it does seem to be a well-formed English sentence. However, some attempts are made. One uses the structure of type theory.

Ramified Type Theory. The theory of types introduces the plausible conditions of *typing* on its formal language. In other words, different syntactic objects have different types, and these correspond to different bits of the world. Typically, a type theory will have a domain E of entities, a domain P of propositions, and other domains of properties of different orders. Each domain can be quantified over in the usual way. If we have a single domain P of propositions, we have a problem. There seems to be nothing preventing us from formulating a proposition

$$\forall x^P (x = p \supset \sim x)$$

which is identical to the proposition p itself. This is a liar-like proposition. It states that any proposition identical to it is false. Given plausible conditions on identity (namely that $p = p$ is true, and if $p = q$ and p is true, then so is q) and classical predicate calculus, we can reason as follows:

(1) $\forall x^P (x = p \supset \sim x) \vee \sim \forall x^P (x = p \supset \sim x)$	Excluded Middle.
(2) $(p = p \supset \sim p) \vee \sim \forall x^P (x = p \supset \sim x)$	Instantiating a quantifier.
(3) $\sim p \vee \sim \forall x^P (x = p \supset \sim x)$	As $p = p$.
(4) $\sim \forall x^P (x = p \supset \sim x)$	$\sim p$ is $\sim \forall x^P (x = p \supset \sim x)$.
(5) $\exists x^P ((x = p) \wedge x)$	Classical quantification moves.
(6) p	By identity.
(7) $p \wedge \sim p$	From 4 and 6.

Propositional quantification is all we need to prove the paradox, once we have the proposition p. Denying that a proposition like p can ever exist is *ad hoc* unless it can be motivated by some other consideration. The structure of type theory gives us a way to do just that. The general principle is that the quantifier in a proposition of the form $\forall x^P \varphi(x)$ does not range over all propositions, but some subclass of that range *which does not include the proposition itself.* The intuition is that quantifiers have to range over completed totalities. As Thomason explains (1986 pages 48—49), if we think of the interpretation of a universally quantified statement as involving a process of constructing the corresponding proposition and somehow establishing the interpretation of all instances of the proposition, the process is not well-founded. One way out is to keep books on quantifiers, and have increasing domains P_1, P_2, P_3 . . . of propositions. In this case, the substitution cannot work, and we have no paradox.

However, it is a case of 'out of the frying pan and into the fire', as we lose the advantages of the type-theoretic approach. I will sketch some familiar, damning objections. Firstly, ramifying the types leaves us no way to state logical laws, as we can't quantify over *all* propositions. It is quantification over *all* propositions which helps us understand type theory, and give it a semantics. Secondly, it results in a general ban on self reference. Statements like "This is in English", and "Everything I say in this paper is true — including this" are unformalisable. Also, if I say "Everything Bob says is funny", and he says "Something Greg says is OK", then we cannot consistently assign types. This is overkill. A theory is odd when it claims that whether or not I make a statement depends on the past, present, *and future* actions of others.

This approach is in trouble, and it's not clear how it could be modified so that it will begin to be an adequate solution.

Hidden Variable Solutions

Other approaches attempt to admit the liar as an authentic statement, but argue it has a different truth value or semantic status to the claim that (λ) is not true:

$$(\lambda) \text{ is not true} \qquad (\lambda')$$

This is done by way of making explicit some property that the sentences don't share. As the relevant property is not obvious to the naked eye, I've called these *hidden variable solutions*. These can be taken to explain why a premise is false, provided that we take the claim that (λ) is true if and only if it is not true as a premise of our argument. Alternatively, if we take there to be some *argument* to this conclusion, this approach will explain why that argument is invalid.

Barwise & Etchemendy. In their book *The Liar* (1987), Barwise and Etchemendy give refreshingly different accounts of propositions and their relationship to sentences. The account they favour lends inspiration from J. L. Austin's work on truth. On this account, a proposition is modelled by an object $\{s; T\}$, where s models a *situation* (a chunk of the world determined by the utterance and pragmatic features of linguistic practice), and it is a collection of (things that model) states of affairs, or *situation types*. T models a *situation type* (some kind of restriction on situations). The proposition $\{s; T\}$ is true if and only if $T \in s$. That is, if the situation picked out is of the type specified.

Some of states of affairs are of the form $[Tr, p; 1]$ and $[Tr, p; 0]$, where p is a proposition. These are the states of affairs that obtain when p is true, and false, respectively. Actual states of affairs are *coherent*, in that if $[Tr, p; 1] \in s$ then p is true, and if $[Tr, p; 0] \in s$ then p is false. A liar proposition will be of the form:

$$\lambda = \{s; [Tr, \lambda; 0]\}$$

where s is some actual situation. By the coherence condition, if $[Tr, \lambda; 0] \in s$, λ is false, and so $[Tr, \lambda; 0] \notin s$. So λ is false. However, it doesn't follow that λ is true, for the state of affairs $[Tr, \lambda; 0]$ is not a part of the situation s. The state of affairs obtains, but the liar shows us that it cannot be a part of the situation that is talked about. It follows that the true claim λ', to the effect that λ is false talks about an *expanded* situation which includes the falsity of λ. So, the hidden variable is the situation, which differs from λ to λ'.

This is an interesting attempt at solving the paradox. It is coherent, and it retains a lot of our intuitions. However, it fails. Firstly, there are no propositions about the whole world — each proposition is about a particular situation, which is only a proper part of the world, as situations are modelled by sets, and the whole world is modelled by a proper class. This seems to be an artefact of the modelling, and unless it is given some justification it will not be able to withstand the weight that is put on it. One such weight is the analysis of the liar. It is argued that the state of affairs of the falsity of the liar cannot be a part of the situation the liar describes — on pain of contradiction. This is a consistent approach, but it doesn't give us an independent *explanation*. It really seems that the liar is general, and not context-bound in the same way as "Claire has the three of clubs" is. More explanation must be given if this is going to count as a *reason* for blocking the paradoxical inference. An account of what states of affairs feature given situations must be given. *Prima facie* it seems that situations are T-closed. That is, if $\{s; T\}$ is true, then the state of affairs of $\{s; T\}$ being true is a part of the situation *s*. What else is needed? If a substantive theory of the paradoxes is to be given, some kind of principled explanation of the behaviour of truth in situations must be given. Situation semantics provides an ingenious place to stop the derivation but the analysis falls short of telling us why we should want to.

There are more views, none of which is particularly hopeful. (Which doesn't mean work on them should be discouraged. The analyses are often useful for enriching our vocabulary of concepts.) We need to examine the alternatives to see if they are more promising.

4. THREE 'DEVIANT' APPROACHES

To stop the paradoxical deductions at a propositional step, there are three possible positions. One is the move from "(λ) is true iff (λ) is not true" to "(λ) is true and (λ) is not true", another is the move from "(λ) is true and (λ) is not true" to an arbitrary proposition q, and the last is to deny the validity of *modus ponens*. The places are:

$$\dfrac{\dfrac{\dfrac{\dfrac{p \leftrightarrow \sim p}{p \rightarrow \sim p}}{\sim p} \quad (2) \quad \dfrac{p \leftrightarrow \sim p}{\sim p \rightarrow p}}{p} \quad (3) \quad \dfrac{\dfrac{\dfrac{p \leftrightarrow \sim p}{p \rightarrow \sim p}\ (1)}{\sim p}\ (2)}{p \rightarrow q}\ (3)}{q}$$

I will call the approach that halts the derivations step (1) the *paracomplete* solution. The reason usually given here is that the proposition p has some kind of 'defective value', such that $p \rightarrow \sim p$ is true without $\sim p$ following. This is commonly described as p being 'neither true nor false'.

The *paraconsistent* solution will either deny the validity of step (2) or (3). The reason for denying (2) is that the proposition p is both true and false and so, $\sim p$ ought not deliver us $p \rightarrow q$. This is the approach that *must* be taken for the paraconsistent reasoner if the conditional satisfies *modus ponens*, as this is the only rule left. Denying (3) is the last resort for the paraconsistent solution to the paradoxes. Not much will be said about this kind of paraconsistency, for it seems that even if some conditionals (such as the material conditional) don't satisfy *modus* *ponens*, others do. Consider $p \Rightarrow q$, meaning that the argument from p to q is valid. It would be *very* odd to hold that this doesn't satisfy *modus ponens*. Our observations in Section 2 lead us to the conclusion that p and $T\langle p \rangle$ have the same inferential force, so the inference from one to the other is valid, so we ought to have $p \Rightarrow T\langle p \rangle$ and *vice versa*. Whatever we think of other varieties of paraconsistency, the species that rejects this kind of detachment is an alternative we ought only consider as a last resort.

Versions of the paracomplete solution have been advocated by Saul Kripke[2] (1975) Penelope Maddy (1983) and others. The paraconsistent solution has been put forward by Graham Priest and Richard Sylvan (Routley): see (Priest 1987 and 1991) and (Priest, Routley and Norman 1989). These solutions to the paradox can be seen as explaining how the argument is invalid, or in the paraconsistent case as explaining how the conclusion (that (λ) is both true and not true) is not as bad as we might think.

5. THREE OBJECTIONS

Is there any *prima facie* reason against such a solution as either of these? Vann McGee in his *Truth, Vagueness and Paradox* (1991, page 100—104)[3] gives three objections to a particular paracomplete solution due to Kripke and others. The objections are:

The Difficulty of Learning the 3-Valued Logic

Here the difficulty is a *practical* one, with regard to how hard it would be to reason in the 3-valued logic. McGee writes:

> The first obstacle is simply how difficult it would be, in practice, for us to use the 3-valued logic in place of the familiar logic. Classical logic has served us well since the earliest childhood, yet we are asked to abjure it in favour of a new logic in which many familiar and hitherto unproblematic modes of inference are forbidden. (page 100)

McGee responds to his own objection by showing how a particular formulation of the 3-valued logic can be given, using rules of proof similar to a classical system. However, McGee's objection is directed against any 'deviant' approach to the paradoxes, and it ought to be considered.

The Unavailability of Scientific Generalisations

This objection hinges upon a feature of the 3-valued logic used in the solution McGee criticises. The feature is that if p and q are evaluated as 'neither', then so is $p \rightarrow q$. This is a problem. McGee writes:

> Consider Jocko. Jocko is a tiny fictional creature that lives right on the border between animals and plants. Jocko has many of the features we regard as characteristic of animals and many features we regard as characteristic of plants. Jocko's animallike characteristics are those we expect to find in protozoa, so that Jocko is also on the border between protozoa and nonprotozoa. It is natural to say that Jocko is neither in the extension nor in the anti-extension of 'animal' and that Jocko is neither in the extension nor in the anti-extension of 'protozoon'; if that is so, then
>
> (Jocko is a protozoon → Jocko is an animal)
>
> will be neither true nor false. Hence,
>
> $(\forall x)$ (x is a protozoon → x is an animal)
>
> will be neither true nor false.
>
> Jocko's story is fictional, but it is a realistic fiction . . . if we do not have any good reasons to suppose that there is no creature in the position in which we have imagined Jocko, then we do not have any reason to suppose that
>
> $(\forall x)$ (x is a protozoon → is an animal)
>
> is true. The generalisation
>
> All protozoa are animals.
>
> becomes highly suspect.
>
> 'All protozoa are animals' is not an accidental generalisation. It is a basic taxonomic principle that is about as secure as a law of nature could ever be. To forbid the assertion that all protozoa are animals is to outlaw science. (pages 101—102)

This example is spot-on. The solution as espoused by Kripke and others invalidates things that are particularly fundamental to the way we reason — not only in semantics, or set theory, and other fields where the paradoxes arise — but also in science.

However, there are other paracomplete solutions that have none of these worries. Once we reject the naïve view that the truth value of the conditional is a function of the truth values of its antecedent and its consequent (or we expand the set of truth values to the interval [0, 1], as in Łukasiewicz's infinitely valued logic) we are able to support scientific generalisations even when they include borderline cases. And, we are able to explain the truth of claims such as

All protozoa are animals.

and

If (λ) is true, then (λ) is true.

and even

If a conjunction is true, so are its conjuncts.

which are each taken as truth-valueless on the 3-valued approach. Similarly, a paraconsistent approach need not fall to this objection. So, this objection deals with a naïve approach without a decent conditional, but fails to count against the accounts with more sophisticated logical machinery.

The Degradation of Methodology

This is the most telling objection. It questions the entire notion of 'changing logic' to give desired results.

> [This objection] is based on an admonition of Field (1972) that our methodological standards in semantics ought not be any lower than our methodological standards in the empirical sciences. We shall contravene this admonition if we attempt to cover up the deficiencies of our naïve theory of truth by abandoning classical logic.
>
> Imagine that we have a genetic theory to which we are particularly attached, perhaps on political grounds, and that this theory tells us that, if a certain DNA molecule has an even number of nucleotides, then all fruitflies are brown; that, if that particular molecule does not have an even number of nucleotides, then all fruitflies are green; and that fruitflies are not all the same colour. It would surely be absurd to respond to this circumstance by saying that our cherished genetic theory is entirely correct and that classical logic does not apply when we are doing genetics. What we have to say instead is that the genetic theory has been refuted . . .
>
> As preposterous as it would be to respond to the embarrassment faced by the genetic theory by saying that classical logic no longer applies when we are doing genetics, it would be no less preposterous to respond to the liar paradox by saying that classical logic no longer applies when we are doing semantics. The liar paradox refutes the naïve theory of truth. It is our duty to come up with a better theory of truth. It is a dereliction of duty to attempt to obscure the difficulty by dimming the natural light of reason. (pages 102—103)

The first and third of these objections have some force. To answer them, we need to take an excursion into the philosophy of logic.

6. FORMAL LOGIC AND REASON

It seems to me that many of the comments about deviant logic and the rationality of a deviant approach to the paradoxes stem from a fundamental misconception of the nature of formal logics and their relationship to reason and rationality. In this section I shall sketch an account of formal logic which will help us evaluate McGee's criticisms. This account follows the lead of Sue Haack in her *Deviant Logic* (1974)

> ... logic is a theory, a theory on a par, except for its extreme generality, with other, 'scientific' theories ... (page 26)

She is right. A system of formal logic is simply a theory. It is not different in kind from any theory in physics, biology or sociology. It differs in subject matter: formal logic is about arguments. The goal of any formal logic is to provide us with a way of representing arguments in a formal system, and to give us a principled way to distinguish the valid forms from the invalid. Haack continues the analogy by taking a pragmatist view of logic, as she is sympathetic to the pragmatist account of science. Unlike Haack's, our account of logic is not tied to any particular philosophy of science.

Excursus: Another lesson can be learned from this conception of logic. It seems to follow from this view that the issue of what it is that *makes* arguments valid, the *ground* of logical validity — whether it's just the meanings of the logical constants, or human convention, or their status in our web of belief — does not have to be answered by a formal logician. This is not to say it isn't an interesting and relevant issue. The analogy can be made with physics. What it is that *makes* the universe the way it is, and the *ground* of physical law is not an issue for physical theories. The general theory of relativity is consonant with the view that laws are Humean regularities and the view that laws are the patterns in the action of a Deity who is sustaining the universe. Physical theories constitute a description of the Way the World Is, without giving a metaphysical description of Why it is that particular way. Similarly, the source of logical validity, although an interesting issue, is largely independent of the task of logical formalising.

One exception is the possibility that our logical theorising itself has some effect on the truthmakers of valid argument — which may be the case if some kind of logical conventionalism is true. There are many interesting issues here, which need space of their own in order to do them justice. □

It should be easy to see that it is wrong to equate classical propositional logic with Reason. Classical logic is a *theory* about the validity of arguments. Similarly, intuitionistic logic, Łukasiewicz's three-valued logic, and any of a whole horde of formal systems are theories about particular classes of valid arguments. It would be as wrong to equate classical logic with Reason as it would be to equate the general theory of relativity with the Way the World Is. The general theory of relativity may describe the Way the World Is in a clear and perspicuous way, it may fit the facts; be ideally useful, maximally coherent or whatever — but it isn't to be identified with what it is intended to describe. Similarly, classical logic ought not be identified with what it is intended to describe, no matter *how* successful it may be.

In fact, the formalism of classical logic on its own does not amount to a theory of valid argument. It must be coupled with a principled collection of translation rules, which can provide a reasoned justification for the formalisms that are chosen for each natural language argument. This is a highly non-trivial task. For example, you need to give an account of why the rule "From $p \wedge q$ you can validly deduce q" doesn't licence the deduction of "I'll shoot" from the premise "One false move and I'll shoot." No doubt, such an explanation can be given. But the *fact* that this has to be done shows that formal systems are theoretical idealisations.

For a formal system to be correct, it would have to account for our valid argument (in its domain). In other words, for any of our valid arguments in the domain of the logic in question, ther should be a formalism that accounts for its validity. For any invalid argument in the domain in question, the formalism should deliver some kind of counterexample.

Let's apply this to the case of classical logic. It is difficult to check, for there are too many arguments. However, some kind of recursive procedure might convince us. This is how we impress the truth of classical logic on our Logic 1 students. We tell them stories about truth values, truth value assignments and truth preservation, and we show them the truth-tables of the connectives, which gives a procedure for generating the valid inferences of classical logic. This is reasonably convincing (except for the table for "$\supset$"). But is it correct? What's more, is the story so clear that no alternatives are to be countenanced under any circumstances? Clearly not. This kind of introductory presentation of classical logic contains many assumptions that can be rationally doubted and it leaves room to countenance alternative formal systems.

Firstly, in one basic presentation of classical logic, an *evaluation* is defined in terms of a mapping from the set of sentences of the formal system into the values T and F. On interpretation this means that each proposition is either true or false — the *Principle of Bivalence*. It is easy to convince Logic 1 students of this principle. This is often achieved by presenting them with statements such as

Snow is white.
Queensland has won the Sheffield Shield.[4]
$2+2=4$.

If, on the other hand, we showed our students statements like

The size of the continuum is $2^{\aleph_0}$.
Graham Priest is taller than Bilbo Baggins.[5]
That colour patch is red. (When pointing to a borderline case)
The present King of France is bald.
There will be a sea battle tomorrow.
This electron is in position x with momentum p.
This sentence is false.

we may at least elicit indecision about the principle of bivalence. Some of the best minds have at least been hesitant in these cases. It may be thought that each of these examples can be explained under the hypothesis of bivalence — and so we have no reason to reject bivalence — but this would be beside the point. At this stage of our logical theorising bivalence is not something we can defend, for it is not in our possession. Bivalence is an assumption that needs to be argued for just as much as any alternative. Robert Wolf gives a helpful illustration (1977 page 336—337):

It is the lack of positive support for classical logic and, more importantly, the fact that there is no felt need to support classical logic as more than a mathematical system that is the unspoken assumption of most of the discussions of rival logics, including Haack's (1974). It is generally assumed — and very rarely argued for — that classical logic is itself philosophically acceptable and that the rival logics must dislodge classical logic before they are acceptable as more than just curiosities . . .

The conceptual situation can perhaps be captured in an image. Defenders of classical logic are like soldiers in a heavily entrenched fortress, while proponents of rival logics are like besieging forces intent on razing the fortress to erect their own on the spot. In the absence of overwhelming force and complete victory, the fortress stands and the defenders remain undislodged. Arguments on rival logics operate on a "possession is nine-tenths of the law" principle, placing the entire burden of proof on those in favour of a rival logic. The proponents of classical logic need only take up a defensive stance and snipe away at the enemy without venturing forth and putting their own positions into question.

It should be apparent from the images chosen that another view is possible. It need not, and we think *should not*, be taken for granted that classical logic is itself any more acceptable than its rivals.

Wolf's analysis of the situation is correct. Classical logic is a simple formalism that has difficulty with accounting for all the facts, but became popular. It is not *prima facie* superior to all other logical systems.

The process of formalising logical systems too often involves feeding our intuitions with simple cases of 'laws', like bivalence, to convince ourselves that they hold in general. *Then* we try to resolve into our scheme cases that don't seem to fit. Sometimes this strategy works, and it is interesting to see how odd statements can be handled in a classical manner, but it's just as important to see what can be done without the simplifying assumptions of classical logic. In the presence of the odd statements we have seen it is as important (and rational) to consider formal systems that are not founded on the principle of bivalence as those that are. Bivalence is a substantive and significant claim about propositions. If we have formal systems that can model our valid reasoning, yet are weaker than classical logic, we have a reason to adopt them over and above classical logic, all things being equal, because these systems make *fewer* assumptions about propositions. It is not yet clear whether deviant logics not founded on the principle of bivalence can model our own valid argument, and I will consider this soon. Before this we must examine another kind of deviance from the classical norm.

This deviance centres on inconsistency. I must admit, it is hard to see what it would be for a contradiction to be true. But faced with the lair, and either without a prior training in classical logic or an open mind, someone could be convinced. If this line is taken, some kind of response has to be made to the arguments that from a contradiction you can validly derive anything. Admittedly, this is an odd artefact of the classical apparatus, but there is at least one interesting argument to this conclusion, made famous by C. I. Lewis.

$$\dfrac{\dfrac{p \land {\sim}p}{{\sim}p} \qquad \dfrac{\dfrac{p \land {\sim}p}{p}}{p \lor q}}{q}$$

The most suspicious looking rule in this context is the deduction of q from ${\sim}p$ and $p \lor q$, called *disjunctive syllogism*. How is this justified? Most often as follows: $p \lor q$ is true, so it must be either p or q that is true. We have ${\sim}p$, so it can't be p that's true — so it must be q that's true. This seems like a plausible argument. (For the moment ignore the fact that it's merely another instance of disjunctive syllogism.)

How does this justification fare in the context of Lewis' argument? It doesn't apply, because the reasoning breaks down at the step from the truth of ${\sim}p$ to it not being p that makes the disjunction true. Under the assumption of the truth of $p \land {\sim}p$, this fails. Under this assumption, it is p that grounds the truth of $p \lor q$, so we can't just go ahead and deduce q. Lewis' argument is not going to convince sceptics, who wonder why it would be that a contradiction would entail anything at all, provided that the sceptics are reflective enough to ask why it would be that some take disjunctive syllogism as valid.[6] As with bivalence, it is interesting to see how much reasoning can go on without the assumption of consistency being made.[7]

To sum up this conception of formal logic, I'll use a remark by John Slaney, who wrote this arguing for the rationality of the enterprise of deviant logic. (1991 page 5)

> The starting point of all logic is the question of which are the valid (perfect, reliable, necessarily rational) forms of argument. What we do in answer to this question is to think up some argument forms which seem good to us, isolate what we take to be the logical constants involved, formulate rules of inference to govern the behaviour of these and thus arrive at a formal calculus . . . We somehow have the impression that our logic is inexorable, so that to question it is not even intelligible. But clearly this inexorability is an illusion. The formal theory goes a long way beyond the intuitive reflections that

gave rise to it, so that it applies to many arguments of sorts not considered at all when we so readily assented to the rules . . . when we considered resolution or the disjunctive syllogism we may have thought: yes, I reason like that; I would regard it as quite irrational not to. But of course, we were not then thinking of reasoning situations that involve taking inconsistent assumptions seriously.

There are enough problems at the core of the project of logical formalisation to cast doubt on the primacy of classical logic. Classical logic is not something that we have fixed and established by a huge weight of evidence that is beyond dispute — or even something that we need a great deal of evidence to 'dislodge'. It relies on a number of generalisations that might seem initially plausible, but have trouble dealing with all the data at hand — especially the paradoxes of self reference.

The methodology of formal logic is (or ought to be) some kind of inductive procedure involving the gathering of plausible argument forms, the formation of systems that capture these forms and somehow explain why these argument forms are valid, and then the testing of these formalisms against more data. None of this procedure is beyond criticism.

From this perspective I would like to echo a famous plea: let a hundred flowers blossom. Dogmatism is out of place in logic. It is rational to consider a menagerie of formal systems, to see how each fare in a wide range of reasoning situations. However, I should make clear that it does not follow that nothing is fixed or firm in logic. Some may think so (Mortensen 1989) but it does seem that a number of rules follow from the way we use the logical connectives. These are plausibly

$p \wedge q \vdash p, \quad p \wedge q \vdash q,$
If $p \vdash q$ and $p \vdash r$ then $p \vdash q \wedge r,$
$p \vdash p \vee q, \quad q \vdash p \vee q,$
If $q \vdash p$ and $r \vdash p$ then $q \vee r \vdash p,$
$p; q \vdash r$ if and only if $p \vdash q \rightarrow r$.[8]

This formalisation contains core logical principles that seem beyond doubt; they seem to survive, given whatever odd propositions you substitute into them. It is broad enough to encompass the vast majority of systems seriously proposed as propositional logics. My thesis is that we don't have enough information to single out one system in this range. We ought to compare and contrast systems to see their strengths and weaknesses as models of our own valid argument. Before using this picture of formal logic to deal with McGee's arguments, let me fend off a few objections.

Aren't Mathematicians Classical Logicians?

It may be argued that the only way to make sense of 20th Century mathematical practice is to assume the validity of classical logic. If mathematicians prove truths using valid means, and they avail themselves of all the moves of classical logic, then we ought to take these as valid.

This is an interesting argument, but it fails. Mathematical reasoning is interesting in a number of respects. Firstly, by and large mathematicians treat their subject matter as consistent and complete. It seems that classical mathematicians have an assumption that for every proposition p that they consider, either p is true or $\sim p$ is true. To make sense of this practice we have no need to take $p \vee \sim p$ as a theorem, we simply can take it as an assumption that mathematicians make, and show that they validly reason from there. Mathematicians also seem to take it that if p and $\sim p$ were true, that would be disastrous for their subject matter. So, another of their assumptions is $p \wedge \sim p \rightarrow q$ for every q. To make sense of mathematical practice, we need not take these claims to be theorems of our logic — we need just add them as assumptions, and then note that under these assumptions their reasoning is valid.[9]

Some may balk at this proposal, but it merely represents mathematical reasoning as enthymematic. The hidden and assumed premises are unproblematic for the mathematician, who will readily assent to them if asked. This places mathematics on as strong a footing as does the classical position, and in this way we can make sense of mathematical practice. If there is something suspect about the propositions that are taken as the enthymemes, this is just as much a problem for the classical account of mathematical reasoning as it is for this one. All of these enthymemes are simple classical tautologies, which are taken to be beyond doubt by the classical orthodoxy. There's no difficulty with a deviant saying that the classical account is right as far as these *instances* of classical laws are concerned. It is the illicit generalisations that are mistaken.

There is no need for the deviant to engage in a *revisionist* programme in mathematics. The reasoning of mathematicians can be explained from the perspective of deviant logic, without having to conclude that mathematicians have 'got it wrong' at any stage. This option is open — a deviant may point to an assumption that has been made in some mathematical context and ask whether or not it is warranted, as constructivists do — but it is not forced by the acceptance of a deviant logic.

Excursus: Mathematical reasoning need not make consistency or completeness assumptions. Constructive mathematics seeks to recast mathematical reasoning without using nonconstructive assumptions. There is a more recent body of work on *inconsistent* mathematics, wherein inconsistent but non-trivial mathematical theories are tolerated. Results in this field are noteworthy; for example, there is a finitary non-triviality proof of Peano arithmetic in a paraconsistent logic — a result which is notoriously impossible in classical arithmetic (Meyer and Mortensen 1984) (Mortensen 1988). □

Isn't Excluded Middle Plausible?

A similar objection can be given which is closer to home territory for most of us. How do I explain the intuitive pull of the Law of the Excluded Middle if it isn't a logical truth? For example, I believe that either I've read all of the Nichomachean Ethics or not — without believing either disjunct (I just can't remember). Isn't this unwarranted?

Clearly not. From the perspective of deviant logic, there are two possible explanations. Firstly, it doesn't follow that because the law is not a theorem of my favoured logical systems, I cannot rationally believe many of its instances. It seems that for the vast majority events in my vicinity, either they happen or they don't. I'm quite rational in believing that the same is true in this case. In fact, the prevailing truth of excluded middles in the general vicinity of my world of medium-sized dry goods (where I don't look too closely at the borderlines of vague predicates) might lead me to think that they are generally true in that area of the Way Things Are. But the further away from my world I go, into the upper reaches of set-theory or to odd sentences in semantically closed languages or the borderlines of vague predicates, my expectation of the truth of the 'law' fades. None of this is irrational — it is more cautious than the classical approach.

The other explanation which may pay off is to note the problems of translating into 'formalese'. My utterance "p or q" may be better formalised as $\sim p \rightarrow q$ instead of $p \vee q$. When we utter a disjunction it often has the force of "if it isn't the first disjunct that's true, it's the second". In this case, my utterance of "p or not p" could be formalised as $\sim p \rightarrow \sim p$, which is a logical truth. Of course, in most deviant logics, this formalisation of is not going to satisfy everything that garden-variety extensional disjunction does, but it may be more appropriate for some of our utterances.

Aren't There Arguments for Excluded Middle?

Another potential problem for the deviant logician is the possibility that the choice of a formal logic should not proceed by way of inductive generalisation, but that there are good *a priori* arguments for particular logical laws. This might be thought to be the case with the Law of the Excluded Middle. Perhaps some deep thought about the nature of the bearers of truth might give us a valid argument whose conclusion is that every proposition is either true or false. I do not deny that this is possible, but any 'deviant' response to such arguments must be on a case-by-case basis. However, some programmatic remarks are in order. Arguments for the Law of the Excluded Middle without question seem to rely on instances of the law to get to the conclusion. This isn't begging the question or illegitimate as such, for the particular instances may be less problematic than their generalisation. In this way, the arguments may have some bite. However, it seems that the particular instances that are used in these arguments are just as problematic as what they attempt to prove, and *this* begs the question.

7. ANSWERING McGEE'S OBJECTIONS

Now we can answer McGee's objections.

The Difficulty of Learning the 3-Valued Logic

The first answer is that by-and-large we do not reason by using a formal system. To say so is to put the cart before the horse. The formal system is there to explain and model our valid reasoning, and to perhaps aid us in it. The deviant logician is attempting to model the *same* reasoning that the classical logician attempts to model. To engage in Reason we do not have to learn *any* formal system, whether classical or not. However, the objection can't be brushed aside immediately. McGee reiterates an objection made by Feferman, that in the 3-valued logic under consideration:

> Nothing like sustained ordinary reasoning can be carried on. (page 100)

Even if we note that nothing like sustained reasoning really happens in first-order predicate logic either (the vocabulary is too poor), McGee still has an objection. It can be rephrased as the claim that deviant formal systems cannot formalise the sustained ordinary reasoning we regularly engage in, in contrast to classical logic which can. As we've already seen while dealing with the objections from the practice of mathematics, this is false. Many deviant logics can formalise our ordinary reasoning without difficulty, given a few plausible assumptions that we would probably agree with anyway. Provided enough instances of the law of the excluded middle are assumed, the 3-valued logic that Feferman and McGee object to becomes as strong as classical logic. So reasoning can be explained from that point of view if it can be explained classically. It only differs in that the arguments used are interpreted as enthymemes. The objection does not have any force against the deviant position.

The Degradation of Methodology

Recall McGee's objection to the practices of the geneticist who rejects a logical law in order to keep alive a favoured theory. Here is another example:

> Imagine we have an entomological theory to which we're particularly attached, perhaps on political grounds, and that this theory tells us that, if one fruitfly sets off in a straight line to a mango tree, and sends out a particular signal to a fruitfly some metres away (not on its flightpath) then this second fruitfly will make a *parallel* journey in the same direction, but that the theory also tells us that the two fruitflies will meet at their destination. It would surely be absurd to respond to this circumstance by saying that our cherished entomological theory is correct and that Euclidean geometry doesn't apply when doing entomology.

This is just as convincing as McGee's example. It is as silly for an entomologist to deny claims of geometry that are quite acceptable in their domain for the sake of a cherished theory as it is for a geneticist to deny claims of a logical nature that are quite acceptable in that domain. Yet it has been rationally countenanced that Euclidean geometry doesn't apply when doing cosmology. So the argument form doesn't deliver its conclusion. A case has to be made as to how logic differs from geometry in some relevant respect. No case like this has been given. As things stand, if this argument works, it works as much against the practices of modern cosmology as it does against those of us using deviant logics in our analyses of the paradoxes.

Excursus: The comparison between geometry and logic is fruitful. Enterprising mathematicians considered geometries which differed with respect to the parallel postulate. Years later, these geometries proved useful to physicists. Our theories about points and lines can vary quite a lot and make sense *as geometries* and not as mere formal abstractions. Non-classical logics are similar. Our theories about conjunction, disjunction, negation and implication can vary while still making sense *as logics*. □

McGee's objection is a little stronger than we've seen so far. He countenances the case where the sheer weight of scientific observation might convince us to abandon classical logic. Semantics is less successful than the other sciences because the data is so scarce that it cannot apply the needed 'pressure'. He writes:

> In genetics we have a huge body of empirical data that our theories are attempting to explain. We can imagine this body of data by its sheer bulk pushing classical logic aside . . . Now, the pressure to abandon classical logic in semantics does not come from an overwhelming body of linguistic data but rather from our metaphysical intuitions about truth. In metaphysics, we scarcely have any data. All that we have to take us beyond our preanalytic prejudices is our reason, and now we are asked to modify the rules of

reason so that they no longer contravene our preanalytic prejudices. In the end, the role of reason in metaphysics will be merely to confirm whatever we have believed all along. (page 103)

There's some nice imagery there, but it won't do the job. Classical logic is not the heavy bulk of Reason that has to be pushed aside. It is a theory *about* Reason. A quick inspection of the justifications people give it shows that it is grounded by the same metaphysical intuitions that ground our semantics. Reason itself is never to be moved about, but it's not clear what Reason itself has to say in the case of the liar paradox. We can argue from the liar and the *T*-scheme to the truth and falsity of the liar from the premise that it's either true or false. Reason at least delivers that. Some say that Reason assures us that the liar is either true or false, and that there is No Way that it could be both true and false. If that is the case, then assuredly, the liar gives us a refutation of the *T*-scheme. However, as I have argued, it is not obvious that Reason says this. To take a deviant approach to the paradoxes is not to abandon Reason, but to question one of its formalisations.

8. CONCLUSION

It should be clear that the deviant account of the paradoxes is coherent, and better methodologically grounded than any approach that takes classical logic as 'privileged'. Classical logic is a formalism that has served well in limited domains (principally, classical mathematical reasoning) but which is founded on general principles that are doubtful at best. At worst they are ill-founded generalisations which are in need of replacement.

Once this is granted, we are not committed to being irrational or to reject truths which we have long held dear. Instead, we ought to treat the project of logical formalisation in the same way as we do any other science. The task is to construct theories and test them against the data. The paradoxes are most useful in this, as Russell has taught us. Instead of taking classical logic as a *given*, to which any account of paradoxes must conform, we would do well to take the paradoxes as what they are — experimental data to deal with as a part of the task of providing an adequate account of valid inference. Given the baroque structures that emerge when the paradoxes are treated in the context of classical logic, we can be sure that such an adequate account isn't the classical one.

NOTES

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1 If you are one of those who grate at this use of the word "sentences" — which includes me at times — feel free to replace the word with a suitable substitute. I find "statement", or "proposition expressed by this sentence in this context" to work marvellously. However, in all honesty, the answer you give to the question of the nature of the bearers of truth, or the contents of propositional attitudes are tangential to most of the issues in this paper.

2 Kripke, to be sure, would not like his proposal to be characterised as espousing the use of a deviant logic. In his "Outline of a Theory of Truth" he expresses surprise that some people have so described his position. His defence is that sentences expressing propositions behave in a purely classical way. Only the odd sentences that fail to express propositions receive the value "neither true nor false". This is some kind of defence, but as the odd sentences are meaningful, can be believed (in some sense), and can function in valid arguments, this defence is not convincing. The logic for determining the validity of arguments involving these sentences is not classical, and so, the proposal is deviant.

3 Subsequent references to *Truth, Vagueness and Paradox* will be by page number only.

4 Well, only in Australia, and only when looking for an obvious falsehood.

5 The difficulty is that Graham Priest is an existing (rather tall) human being, and Bilbo Baggins is a fictional hobbit.

6 This analysis of disjunctive syllogism and Lewis' argument is given by Mike Dunn (1986) and John Slaney (1991).

7 A fair amount of work is going on in this area, some of it *very* interesting. For examples, consult (Meyer and Mortensen 1984), (Priest 1987), (Mortensen 1988), (Priest, Routley and Norman 1989) and (Slaney 1991).

8 Where '$\vdash$' represents logical consequence, and the semicolon represents some kind of premise combination, whose properties are left undecided. For example, $(p; p); q$ need not have the same deductive force as $q; (p; p)$, $p; q$ or $p; (p; q)$. See John Slaney's "A General Logic" (1990) for an account of this characterisation of formal logics.

9 This is not quite all. The conditional that is used in mathematical contexts is notoriously non-modal and irrelevant. Moves such as deducing $q \rightarrow p$ from p are widespread in mathematical contexts. To explain this we must either equip our logic with a conditional that will validate the required moves, or simply assume them for mathematical propositions. Again, this is not a problem.

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