On Preserving:
Essays on Preservationism and Paraconsistent Logic

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Edited by
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ON PRESERVING

One

Introduction to the Essays

PETER SCHOTCH

The essays in this volume are intended to serve as an introduction to a research project that started a long time ago, back in the 20th Century. But it didn't start as a project about paraconsistent logic or about what we have come to call preservationism.

1.1 The Origins of Preservationism

Instead it started in the summer of 1975, at Dalhousie University, when Ray Jennings[1] and Peter Schotch decided to write a 'primer' of modal logic—a project that never quite made it into the dark of print. So it was modal and decidedly not paraconsistent logic which was uppermost in the minds of Jennings and Schotch. In this connection one should keep in mind that neither of the intrepid modal logicians had so much as heard the word

[1]Jennings was then (and now) a member of the philosophy department at Simon Fraser University, but he had accepted a summer-school job at Dalhousie in order to work with Schotch.

'paraconsistent' at the time we are considering.[2]

What got the ball rolling, was the discovery by Jennings, which he instantly communicated to Schotch, that so-called normal modal logic was not suitable for certain philosophical applications of a deontic cast. In particular, there seemed to be distinctions which one might draw intuitively which evaporate in normal modal logic.[3] Now the only restriction on the kind of modal logic involved was that it be normal, so enabling distinctions which cannot be made in that logic was not going to be a simple matter of dropping some modal axioms or, on the semantic side, relaxing a frame condition.

If we are talking about normal modal logic in general, then there are no frame conditions to relax. The fundamental normal modal logic, often called K, is determined by the class of all frames. Period.

But later that afternoon, over a glass of beer, it started to seem that perhaps the semantics of normal modal logic did impose some restrictions after all. To begin with, there was the restriction that the universe of the frame, its set of 'possible worlds,' must be non-empty. That didn't seem a very promising condition to relax, though there certainly have been those who were prepared to give it up, if only to see what happens to modal logic.[4] No, it would have to be a condition on the frame relation which did the trick. Which condition was it? There can be only one, and that is the condition that the frame relation be binary. And there it was.

Jennings and Schotch looked at each other with a wild surmise and then began trying to work out what the truth-condition

[2]In fact the first time they heard that word was at the 1978 meeting of the Society for Exact Philosophy, held in Pittsburgh. And in all truth, they paid little enough attention to the word the first time they did hear it.

[3]The particular example mooted by Jennings had to do whith two modal formulas, one of which had a necessity operator outside of a conditional and the other having the necessity distributed over the conditional. Perhaps the easiest version of this kind of distinction, and the one which became absolutely central to the program, is the one between $\Box\alpha \supset \neg\Box\neg\alpha$ and $\neg\Box\perp$. The latter claims that no contradiction can be necessary while the former asserts that if any formula is necessary, its negation is not.

[4]Charles Morgan has constructed this kind of modal logic.

for necessity would have to be when the frame relation was not binary but say, ternary. Later that evening they had narrowed it down to only one possibility:

> $\Box\alpha$ is true at a point x in a model M if and only if for every ordered pair $\langle y, z\rangle$ such that $Rxyz$, either α is true at y or α is true at z.

The generalization to n-ary frame relations is trivial.

In the hot flush of discovery, Jennings and Schotch imagined that, like stout Cortez, they were treading on *terra incognita* but as Goethe reminds us,

> All the great thoughts have already been thought. Our job is merely to think them again.

n-ary frames had already been introduced by Bjarni Jónsson in his doctoral thesis, which results were later reported in a publication authored jointly with the thesis supervisor Alfred Tarski—(Tarski and Jónsson, 1951). In that work n-ary frames are a way of representing Boolean Algebras with n-1-ary operators. Modal logic following Kripke had rediscovered (a decade after the original discovery) the special case of this idea, for binary frames.

The problem for Jennings and Schotch, was how to axiomatize a fragment of the Jónsson n-ary frames, the *diagonal* fragment. In other words, in the original work, the extra operator is n-1-ary, let's call it $\boxplus$. So for every n-1-tuple of formulas $\alpha_1, \ldots, \alpha_{n-1}$ the 'modal' formula is $\boxplus(\alpha_1, \ldots, \alpha_{n-1})$. This notion, as interesting and useful as it no doubt is, isn't the modal notion selected by Jennings and Schotch. Theirs, the unary $\Box\alpha$, is represented in Jónsson's terms as $\boxplus(\alpha, \ldots, \alpha)$ where the $\ldots$ represents n-3 iterations of the formula α.

This was not a problem which had ever been considered before so Jennings and Schotch were happily refuting Goethe, or at least attempting to do that. The thing is: using the resources of normal modal logic, the problem is extremely difficult.

If we stick to the ternary case for ease of exposition, one quickly finds that the principle of *complete modal aggregation*

[K] $\vdash \Box\alpha \wedge \Box\beta \supset \Box(\alpha \wedge \beta)$

doesn't hold, although the rule of monotonicity (sometimes called regularity) and the rule of (unrestricted) neccessitation (also called the rule of normality):

[RM] $\vdash \alpha \supset \beta \implies \vdash \Box\alpha \supset \Box\beta$
[RN] $\vdash \alpha \implies \vdash \Box\alpha$

both hold in ternary frames (and indeed in n-ary frames).

So now comes the problem: How do you axiomatize this semantics? It turns out to be quite a trick without [K]. In fact, it turns out to be impossible without some kind of aggregation principle, as Jennings and Schotch realized early on in the project. The correct version of aggregation seemed to them to be the principle:

[K3] $\vdash (\Box\alpha_1 \wedge \Box\alpha_2 \wedge \Box\alpha_3) \supset (\Box(\alpha_1 \wedge \alpha_2) \vee \Box(\alpha_2 \wedge \alpha_3) \vee \Box(\alpha_1 \wedge \alpha_3))$

But how to prove this conjecture? The situation is entirely different in K where all the instances of the principle of complete modal aggregation that you might need, follow transparently from [K]. In the general case, one needs to show that every aggregation principle which holds in virtue of 'pigeonhole reasoning'[5], (the way [K3] does) follows from [K3]. And that, gentle reader, is a non-trivial piece of combinatorial work.

While pondering this question and writing a few papers on modal aggregation (none of which solved the problem) Jennings and Schotch chanced to attend the meeting of the Society for Exact Philosophy in Pittsburgh which was held in June of 1978. It was there that they first heard of paraconsistent logic via a paper on the subject presented by Robert Wolf. It was also there that they first saw normal modal logic axiomatized by the rule:

[5]By this is meant merely the principle that if there are k objects to distribute over $k-1$ containers, at least one container must contain at least two objects

$$[\text{N}]\ \Gamma \vdash \alpha \implies \Box[\Gamma] \vdash \Box\alpha$$

(where $\Box[\Gamma]$ stands for $\{\Box\gamma | \gamma \in \Gamma\}$)

when it was used in a paper presented by Brian Chellas.[6] In the question period following the presentation by Jennings and Schotch, Barbara Partee happened to remark that it all seemed to her like an attempt to represent a kind of non-trivial reasoning from inconsistent data, since, without complete modal aggregation one cannot construct $\Box \perp$ from $\Box\alpha$ and $\Box\neg\alpha$. So in particular, if one thought of the $\Box$ as some sort of belief operator then .... Jennings and Schotch took note of how the audience perked up at this, and kindly thanked Professor Partee for the observation.

It was on the flight back to Halifax[7] that Jennings urged Schotch to consider the Chellas axiomatization in connection with their n-ary modal logic (in all honesty Schotch had barely noticed the rule while listening to Chellas' presentation). What Jennings had seen and Schotch had not, is that the rule [N] characterizes necessity in terms of some notion of inference.[8] For the necessity of normal modal logic in the usual sense, the 'corresponding'[9] inference relation is simply the classical one. But Jennings presumed, since the n-ary semantics lacked complete modal aggregation, the inference relation that characterized the new notion of necessity cannot be classical.

This turned out to be an absolutely crucial insight, and one which led ultimately to the program now called preservationism. Schotch began trying to discover an inference relation which bears the same relation to n-ary modal logic, as the classical relation bears to normal modal logic. There will of course be infinitely many such relations corresponding to the different orders of the frame relations. The project didn't take long since it was

[6] Chellas later attributed the rule to Dana Scott.

[7] Where Jennings was teaching summer school again.

[8] An algebraist would say that the two concepts inference and necessity, are Gallois connected.

[9] Depending upon how precise one wishes to make the notion of correspondence used here, one might be restricted to the necessity operator of the logic K, rather than the necessity operator of any normal modal logic.

possible to in effect, read off, the relation from the modal semantics.

1.2 How Parconsistent Inference Fell Out of Modal Semantics

The trick is to begin as though you already have the relation in question and then start the 'hard' direction of the fundamental theorem for all, let's say, ternary modal logics. These will be all the extensions of the base logic K3 which logic is axiomatized by the single rule (on top of classical sentence logic);

[N3] $\Gamma \rhd_3 \alpha \implies \Box[\Gamma] \vdash \Box\alpha$

In order to discover just what properties $\rhd_3$ has to have, we see what will be needed to prove:
$\Box\alpha \notin \Sigma \implies \langle \mathcal{M}_{K3}, \Sigma \rangle \nvDash \Box\alpha$

Where $\mathcal{M}_{K3}$ is the K3 canonical model and Σ is an element of the K3-canonical domain, which is to say a maximal K3-consistent set of formulas. The converse of this is trivial so long as we define the K3-canonical relation, R_{K3} over the elements of K3-canonical domain, in the obvious way as:
$R_{K3}\Gamma\Delta_1\Delta_2 \iff (\forall\alpha)(\Box\alpha \in \Gamma \implies \alpha \in \Delta_1 \text{ or } \alpha \in \Delta_2)$

So assume $\Box\alpha \notin \Sigma$. We need to find a pair of maximally K3-consistent sets to which Σ is related, neither of which contains α. The existence of such a pair amounts to the failure of $\Box\alpha$ at the element Σ of the K3-canonical model, which is what we are trying to show. To show that a pair of maximally K3-consistent sets exists to which Σ stands in the canonical relation, it suffices to show that there is some partition of the set $\Box(\Sigma) = \{\beta | \Box\beta \in \Sigma\}$ both cells of which are K3-consistent. In such a case both cells can be extended to maximal K3-consistency in the usual way. We also require that α not belong to either of the maximally K3-consistent sets thus constructed. The only way to ensure this is to add $\neg\alpha$ to each cell of the partition. From this point we argue by *reductio*:

Suppose that no such partition can be found, which is to say that for every partition of $\Box(\Sigma)$ into two K3-consistent cells, at least one of the cells is not K3-consistent with $\neg\alpha$. By classical reasoning, this amounts to saying that at least one cell of every partition of $\Box(\Sigma)$ into two K3-consistent sets (classically) proves α. If our axiomatization of K3 is going to work, what we just wrote must amount to a statement that $\Box(\Sigma) \rhd_3 \alpha$. This is because if and only if the inference relation is defined in that particular way, then by appeal to the rule [N3], we must have

$$\Box[\Box(\Sigma)] \vdash \Box\alpha.$$

But the set on the left of $\vdash$ is a subset of Σ since it collects those formulas in Σ which are 'guarded' by $\Box$ (which is to say more precisely that it strips off the leading $\Box$ before collecting the formulas) and then it puts back the $\Box$. This operation leaves us with exactly those formulas in Σ which have an initial $\Box$.

Now if a subset of Σ proves $\Box\alpha$ then Σ itself must likewise prove that formula by monotonicity of inference.[10] But, by classical reasoning, every maximally K3-consistent set is a theory, which is to say that it contains every formula that it proves. So $\Box\alpha \in \Sigma$ which contradicts the hypothesis. Thus K3 is determined by the class of all ternary frames.

Now in order for $\rhd_3$ to be a paraconsistent inference relation, it must be an inference relation. There is cheerful news on that matter, the relation satisfies all the classical structural rules. So yes it is inference of some kind, but is it really paraconsistent?

Well, yes and no. It is at least a partially paraconsistent relation, since if your premises contain only two contradictory premises without either being self-contradictory, then the closure of the set under the relation $\rhd_3$ will not contain every formula.

On the other hand the premise set might contain formulas like $\alpha_1, \neg\alpha_1 \wedge \alpha_2, \neg\alpha_1 \wedge \neg\alpha_2$ any one of which is consistent by itself but each is inconsistent with the other two. It obviously follows that

[10] This is, in effect, an appeal to the classical structural rule of dilution.

for every partition of this set into two consistent cells, anything at all—including that at least one of the cells proves β. This is because there are no partitions into only two consistent cells. To get consistent cells, there must be at least three.

Now of course there will be an inference relation, call it $\triangleright_4$ which does what we want but it will fall prey to sets of 4 formulas each consistent in isolation but explosive in combination with any of the others.

Are we to be driven to infinity here? No, but before we see why not, let us come clean. There are premise sets such that no partition into consistent cells (even allowing infinite partitions) is possible. These are the sets that contain formulas which are, by themselves, inconsistent. Such formulas are often called *absurd* with $\alpha \wedge \neg\alpha$ serving as the most popular classical example.

Those inconsistent sets, the ones containing absurd formulas, cannot be 'fixed.' No matter which version of our new relation we take, the consequences of such a a set under that relation will be the set of all formulas. If you take the goal of paraconsistent logic to prevent even absurd formulas from 'exploding,' then our relations are, none of them, paraconsistent. Fortunately, neither Jennings nor Schotch (nor the angels, we are tempted to say) have that goal.

Thus was a great gulf fixed between the preservationist approach, and the so-called 'dialethic' approach championed by several people in the Southern Hemisphere.[11] It is characteristic of the latter view that both a sentence and its negation might be true, and that there could be a way of making $\alpha \wedge \neg\alpha$ true while at the same time making β false. Hence, even absurd formulas might be redeemed. It is tempting to refer to this program as 'non-negative' on the ground that whatever else may be going on, if you succeed in making both α and $\neg\alpha$ true, then you must be using the symbol $\neg$ to represent something other than negation. This temptation is bolstered by the fact that the

[11] The name Graham Priest springs to mind.

dialethists insist on referring to the preservationist program as 'non-adjunctive.'

1.3 The Concept of Level

In the previous section it seemed as though there was no limit upon how large we might have to make a partition in order to assure that every premise set (not containing an absurdity) could be partitioned into that many consistent cells. That is no doubt true but not entirely to the point. What we clearly need to do is to treat every premise set on its own merits, rather than assuming that there is some fixed number n such that the premises can be partitioned into no more than n consistent cells.

Rather, let us say that the *level* of a set, say Γ of formulas, modulo some technicalities which will be explained in detail below, is the least number of cells (if there is such a thing) such that Γ can be partitioned into that many consistent cells. If there is no such number, which is to say if Γ contains an absurd formula, then assign the symbolic value ∞ to be the level.

And now we have what we might regard as a whole-hearted paraconsistent inference relation. We refer to this relation as the *forcing* relation indicated by $[\Vdash$ and it is defined not in reference to some fixed number of cells, but to partitions into the number of consistent cells equal to the level of the premise set. If, on every such partition, at least one of the cells proves α, then $\Gamma [\Vdash \alpha$.

1.4 Preservation

At this point the modal logical motivation has fallen away. In the first place, the version of the generalized form of the rule [N], which has forcing on the left of $\Longrightarrow$, doesn't correspond to any particular n-ary modal semantics, but rather to a semantics in which the frame relation is allowed to vary. This is a bit of an odd duck, modally speaking. In the second place, the problem which started the whole thing off, that of showing that the n-ary semantics can be axiomatized using [RN]. [RM], and the appropriate n-ary form of the aggregation principle [KN] is still unsolved!

As a matter of historical fact, several years had to pass before that problem was finally solved.[12]

So where does preserving come in? During several presentations that Jennings and Schotch made in the early 1980's they more than once heard the complaint that their notion of paraconsistent inference seemed at odds with the 'truth-preservation' account.[13] Once this complaint was considered outside of heated debate, it seemed to come apart in one's hands.

To say that forcing (or any of its 'fixed level' modal cousins) does not accord with truth-preservation is to say that some inferences licensed by the latter are not licensed by the former. This is without doubt true, but those inferences, are precisely the ones that forcing is designed to block. In other words the complaint seems to come down to the fact that forcing doesn't behave as stupidly as classical inference in the face of (many) inconsistent premise sets. To which Jennings and Schotch reply 'And . . . ?'

But since merely preserving truth opens the door to a lot of inferences we'd rather not draw, perhaps forcing & Co. can still be thought of as preserving something worthwhile. And it is relatively easy to see that forcing preserves *level* which includes preserving truth, when the premises taken all together, are capable of being true.[14] So level preservation can be thought of as a way of fixing truth-preservation at the precise place where it is broken—when there is no truth to preserve. In that case, forcing finds something else, or rather something in addition, that can still be preserved even while truth is on vacation.

Thus forcing is a kind of inference for somebody who is committed to the idea that exactly those inferences are correct which

[12]See the essay 'n-ary Modal Semantics' in this volume.

[13]This paradigm is considered at much greater length in the essay 'On Preserving' in this volume.

[14]Which is of course to say that the set of premises is consistent. This is taken up in much more detail in 'On Preserving' in this volume

preserve some property which is worth preserving. Now the dialethists can and do argue that *they* are the ones who really do fix truth-preservation, since their notion of paraconsistent reasoning preserves truth and not something other or over and above, like level. Unfortunately this claim stands or falls with the claim that both a sentence and its negation can be true, which seems to many a serious barrier. Not to put too fine a point on the matter, an uncharitable person might remark that dialethism, rather than fixing truth-preservation, breaks it beyond any hope of repair.

1.5 The Essays

In the essay 'Paraconsistency: Who Needs It?' Ray Jennings and Peter Schotch present in a more informal way, the core motivating examples that lead to a preservationist approach to paraconsistency. They take up the usual examples of belief and obligation and also a not so usual one in recasting the motivation of C.I. Lewis for his notion of strict-implication. In effect, they suggest that Lewis was a pioneer of paraconsistency.

In 'On Preserving' Gillman Payette and Peter Schotch examine the somewhat shaky status of the truth-preservation paradigm for right reason and suggest alternatives. They treat the concepts of level and forcing with much greater precision and generality than we manage above.

In 'Level and Compactness' Gillman Payette and Blaine D'Entremont explore an analogy between level and *measure* and prove a number of crucial things about compactness in the context of level. These results are used to derive a very important theorem concerning the place of forcing among all other similar inference relations.

In 'n-ary Modal Semantics' Dorian Nicholson presents the axiomatization of the Kn logics in terms of their aggregation principles and and its consequences for the axiomatization of the forcing relation. This was the longest standing open problem in the preservationist program.

In 'Multiple Conclusion Forcing' Dorian Nicholson and Bryson Brown show how to extend many of the results on forcing, including axiomatization of the forcing relation, to the more general sequent-logic like case, in which the relation is reconceived as a relation between sets of formulas (premise sets) and sets of formulas (conclusion sets). This important essay corrects a number of mistakes in the earlier 'On Detonating' (Schotch and Jennings, 1989).

'Forcing and Practical Inference' is the essay in which Peter Schotch takes up the issue of modifying the definition of forcing in two different ways. In the first modification, the notion of level, upon which the forcing relation is based, is required to respect certain natural 'clumps' of premises in the sense that no logical cover of a set of premises can break up any of these clumps. The second variation recognizes that logical consistency is a very weak idea and that in ordinary and technical reasoning both, we recognize that the collection of 'bad guys' extends far beyond the logically absurd. This recognition requires an obvious change in the definition of logical cover.

The essays are, generally speaking,[15] capable of standing alone though there is a considerable overlap. All of them give short weight to some or other important feature (or features) of the preservationist program though none performs such disservice to all the important features. In fine, the essays act together to fix each others' expository flaws. While the flaws themselves make the essays more readable than completeness would allow.

[15] J. S. Minas used always to add the gloss: 'which is to say, in no particular case.'

Two

Paraconsistency: Who Needs It?

RAY JENNINGS AND PETER SCHOTCH

Abstract

In this essay Ray Jennings and Peter Schotch present some of the central motivation for what has come to be called the preservationist approach to paraconsistent logic.

2.1 Introduction

The classical account of consistency is not adequate in many situations in which we certainly require a notion *like* it. This is tantamount to saying that there are situations in which classical logic is not adequate. While such an assertion might have enjoyed a certain amount of shock-value in the early 20th Century, things have changed since those heady days. Now most people agree that some changes are often necessary in the classical account of inference, though there is nothing approaching wide-spread agreement on the precise nature of those changes.

One can distinguish two broad strategies for 'fixing' some logic which misbehaves. What we count as misbehavior here is the licensing of inferences which we imagine ought not to be licensed, or the refusal to recognize some inferences as valid which we ordinary reasoners find entirely congenial and unproblematic. And the strategies referred to just now we may term *replacement* and *revision*.

When a logic misbehaves in the second way, we most frequently revise it. We don't need to look far for an example: The logic of 'unanalysed sentences' fails to recognize the correctness of the move from 'All women are mortal' and 'Xantippe is a woman' to 'Xantippe is mortal.' Annoyed by this denial of such an obvious example of correct reasoning, we call for the logic of sentences to be extended to include terms and individual quantifiers. We are inclined to take as characteristic of the revision strategy that the revised logic contain the one which had revision visited upon it.

When a logic misbehaves in the first way, things aren't quite so clear. At first blush it seems that all we can do is sue for replacement of the offending logic. This is certainly what C.I. Lewis seemed to be doing in the early 20th Century when he proposed that so-called material implication be replaced with what he called strict implication. Nor was he alone in pointing to perceived flaws in the classical account of reasoning and suggesting that it be given up, at least partially, if not root and branch. The purveyors of many-valued logics and the intuitionists to name just two, were also early adopters of the replacement strategy. We save a discussion of later calls to the barricades for a bit.

Now 'replace' is a strong word, and we must temper our understanding of it in this context with the realization that the classical account of inference is as inclusive as it could be, barring triviality. If we were to take any inference at all which is not classically valid and add it to classical logic as an extra principle, the logic so constituted would allow any formula to be derived from any premise set—which is to say it would be trivial. The upshot of this fact, first proved by Post (in his doctoral thesis) is that any non-trivial logic over the classical language, is almost sure to be a sublogic of classical logic. So a logic offered for replacement is not going to depart from the classical canon in the sense of being disjoint from it. This is certainly the case with intuitionistic logic and many-valued logics, which are easily shown to be included in classical logic.

These leaves all those logics, which like the Lewis ones, wish to replace the classical $\supset$ with a connective which is more like what we 'really' mean when we use words like 'implication.' In many, but not all of these cases replacement is the order of the day. But even in these cases, it has sometimes turned out that the proposed replacement is actually a revision, with the new notion of implication representable as some species of modalized classical conditional. This befell the C.I. Lewis program, much to his chagrin one suspects, but we shall revisit his motivation a bit later in this essay to see if any lessons remain to be learned from it.

There is an important socio-historical point to be made here. Classical logic is not so-called because of its antiquity;[1] it is in fact an invention of the late 19th and early 20th Century. When the first calls for replacement went forth, philosophers and mathematicians were not being asked to jettison some cherished theory handed down by their respected forefathers. Instead, they were being asked to reconsider their allegiance to a relatively new and untried theory. That situation has changed. By the turn of the 21st Century classical logic has become entrenched. By this is meant that the scientific community now has a very large investment in the theory. In practical terms it means that replacement theorists are going to have uphill sledding and even revisionists will have to endure a certain lack of goodwill if not downright surliness.

Of all the replacement theories, one must give pride of place to intuitionistic logic. There is no single reason for this notable suc-

[1] As one wag has put it, classical logic is so-called because of its name.

cess, but one may discern at least two strands to the story. The first is that intuitionism began early enough not to have to fight an entrenched opponent. Trench warfare being what it is, this is a weighty consideration. The second is that what we might call the structures which match the formalized account of the intuitionist calculus[2], seem to arise in many different parts of formal science.[3] This is often taken to be an outward and visible sign of the worthiness of the logic displaying it.

2.2 The Strange Case of C. I. Lewis

Of all the replacement theories which have been co-opted by classical logic into revision theories, the best known is modal logic. C. I. Lewis lived long enough to see his recommendations concerning strict implication dissolve, by turns, into a proposal to treat the modals as new connectives to be added to the classical store. Of course the proposal even in this new revisionist garb was far from non-controversial. Quine and his students were the most prominent adversaries of this idea and they (and others) were able to cast a pall over modal logic as an area of research. This pall had dispersed by the late 1960's however and the area has been as respectable as any other since then.

To put the early controversy in a nutshell, Lewis thought that the classical account of implication was wrong. By this he meant that some of the classical theorems were wrong when we read the symbol $\supset$ as 'implies.' In defence of this view he would produce what had come to be called 'paradoxes of material implication.' Most prominent among these were:

$$\alpha \supset (\beta \supset \alpha)$$

$$\neg\alpha \supset (\alpha \supset \beta)$$

[2]But what irony that the formalized part of the intuitionistic program carry so much of the weight of promoting the popularity of that program. Intuitionism arose historically as a protest against formalism.

[3]Topology, category theory, and quantum physics are three examples.

where these were required to bear the respective interpretations 'A true proposition is implied by any proposition' and 'A false proposition implies any proposition.'

According to Lewis, genuine or what he called 'strict' implication must be interpreted in terms of deduction. In his informal semantics of implication then, we would only say that α implies β provided there is some way of deducing the latter from the former.

To briefly rehearse some of the more puissant of Quine's criticisms of this position: Implication is a relation rather than a connective. When we read $\supset$ as 'implies', as Russell often seems to do, this is nothing more substantial than a *façon de parler*. When we read, for instance $\alpha \supset \beta$, as α implies β, we intend nothing more mysterious than the assertion that α and β (in that order) are connected by $\supset$. As for 'real' or 'genuine' implication who knows what that is?[4]

Actually, as devastating as the criticism might be, Quine could have done rather better than this. Alas, he was one of those who had been so steeped in the 19th Century approach to logic that he took the so-called logical truths to be what logic is about. Once we rid ourselves of that unfortunate affliction, we may say to Lewis: It's a bit silly to say the we have a genuine implication (or strict implication if you must) between the propositions α and β only when one can deduce β from α. This is silly because we already have a representation for that in our logic, namely $\vdash$. So your strict implication is simply another word for provability—and why would we need to rename provability?

Thus do we sound the death-knell for Lewis' complaint about classical logic. It is simply based on a mistake or, to be as charitable as possible, a confusion between which matters belong to the object language and which to the metalanguage.[5] Once we see

[4]This is the Quinean sense of not knowing what X is, on which I don't know what X is, and if you think you do, you're mistaken.

[5]This is understandable for Lewis, who never really accepted the concept of a metalanguage. At least there is no textual evidence that he did.

this, we can dismiss the complaint with its fear-mongering talk of paradox as airily as Quine and his followers. Or can we?

As the old saying has it, there is no smoke without fire. Even if Lewis was partially blinded by the smoke, there might yet be a hot coal or two which remains once the smoke has been contemptuously dispersed. To say this is to say that perhaps all this talk of paradox reverberates into the metalanguage. We shall consider this radical idea more carefully.

Start with 'a false proposition implies an arbitrary proposition' which is impressive enough to have the Latin name *ex falso quodlibet*. There is an exegesis of this which puts it squarely at the metalinguistic level: 'If all we know about some premise set is that it contains a falsehood, then that is sufficient to allow the inference of any formula at all as a conclusion.' Symbolically:

$$\{\alpha, \neg\alpha\} \vdash \beta$$

When Lewis says, as he does of the 'paradox', that there is no genuine sense of deduction in which this assertion makes sense, he echoes generations of beginning logic students who think that it is cheating to use *reductio* to derive some formula α from a set of assumptions which antecedently contains a contradiction before we assume (for *reductio* as we blithely say) $\neg\beta$. There is no sense in which the assumption of the hypothesis *leads to* an absurdity. To assert the contrary is like saying of a man born with a single arm, that the first sight of his mother's face led to his losing an arm.

We must take care here, since Lewis would distinguish the case in which all we know is that a premise is false, from the case in we know that a premise is *necessarily* false. For Lewis, the paradigm of a necessarily false premise is a self-contradictory or, as we often say, an *absurd* one. In this case, says Lewis, bolstered by a famous proof, we really and truly can deduce, using entirely non-controversial rules of proof, anything at all.

The dual 'paradox' that if all we know is that a conclusion is true, then it may be derived from any premises at all, requires an approach to inference in which we allow sets of conclusions on the right hand side of $\vdash$. These right-handed sets are understood dually to the left handed ones. In other words while the comma in $\{\alpha_1, \ldots, \alpha_n\}$ is understood 'conjunctively' if the set is left-handed, it is understood 'disjunctively' if the set is right-handed. So to say that all we know is that a (right-handed) set contains a truth, is to say that the set in question contains something like $\{\alpha, \neg\alpha\}$ and in that case classical logic tells us that $\Gamma \vdash \{\alpha, \neg\alpha\}$ for any set of premises Γ.

If we follow Lewis once more we must distinguish this case from the one in which the conclusion set contains a dual-self-contradiction, namely a tautology.

Now the fact is, as the complaints of our students indicate, classical logic does not permit us to make any distinction of this sort. But perhaps we might be able to find reasons to criticize classical logic on that ground.

2.3 The Inconsistency of Belief

We begin with an historical example of some prominence—Hume's Labyrinth:

> I find myself in such a labyrinth, that, I must confess, I neither know how to correct my former opinions, nor how to render them consistent.

It is no good telling Hume that if his inconsistent opinions were, all of them, true then every sentence would be true. Even if he could be got to accept this startling claim, it would have brought him no comfort. The most which might be wrung out of it is that not all of his opinions could be true. This, so far from being news to Hume, was precisely what occasioned much of the anguish which he evidently felt. What we need in such circumstances is a way to cope.

Nor is it merely in the realm of metaphysics that inconsistent opinions charm us. In the physical sciences as well we seem to have inconsistent consequences thrust upon us by equally well confirmed hypotheses. And even if, as we reassure ourselves, the inconsistences are not ineluctable, we must live with them whilst awaiting the crucial experiment or the new paradigm or the new metaphor or merely promotion. Eventualities may render the inconsistencies resolvable or illusory or unimportant. In the meantime however, we must draw inferences from them which do not depend upon their inconsistency, but which are informed by it.

A more humanistic logic is required. Such a logic would accord with what we must frequently do, namely the best we can with data which, although inconsistent are nevertheless the best data we are able to command. We would like to be able to reflect but also judge the reasonings of ordinary doxastic agents. We wish to offer them standards of correctness which survive without triviality, conflicts of belief. This humane approach need not pander. It would accept classical consistency as an ideal without the pretense that it is pervasive or even especially common in actual belief sets.

We say nothing controversial when we claim that often, perhaps usually, the set of sentences to which someone will actually assent would, if scrutinized, be discovered to be classically inconsistent. The question is: What are we to make of this observation? There appear to be two distinct reactions. The first is to say that some such apparently inconsistent belief sets are real and constitute a genuine difficulty for the received theory of inference. The second takes such inconsistencies to be mere appearance—a fanciful mask over the underlying consistent reality.

Our choice between these is important, for our ordinary understanding of rationality involves drawing inferences from our beliefs. We are not positively *required* to draw these inferences but we *are* required to accept the conclusion once the validity of such an inference has been made known to us. But how is non-trivial reasoning possible in the face of inconsistency? We are like jurors made trusting by cruel penalties. To the extent that we accept the classical account of reasoning, to that extent we resist the view that the set of a person's beliefs can really be inconsistent in a full-blooded sense. We want to either to acquit or somehow diminish responsibility.

We have recourse to such strategies as distinguishing between *active* beliefs, which must indeed be consistent and mothballed beliefs which lie strewn about in their cells unregarded and out of mind. For inconsistencies among this disused bricabrac or between them and the brighter ornaments of present thought, we will not keep the agent long in dock.

It is tempting to think of this distinction as being congruent with the distinction sometimes made between explicit and implicit contradiction. We are happy to offer forgiveness if not pardon, for the latter but visit upon the former harshness without mitigation. We must step carefully here for the implicit/explicit distinction can be variously applied to confusing effect. We may wish to say that there is an explicit contradiction in a person's beliefs when she actively adheres to two or more mutually contradictory opinions. Implicit contradictions, on this score, would be those between two inactive beliefs or between one which is active and another which isn't. This is not Hume's predicament.

His problem lay in his *active* acceptance of contradictory opinions. On the latest account of the distinction, Hume's contradictions were explicit. Yet he did not recognize whatever harsh penalties later logic would have meted out to him. It did not trivialize his inquires. It simply provided him with 'a sufficient reason to entertain a diffidence and modesty in all my decisions.'

There is an alternative account of the distinction according to which Hume's contradictions would count as implicit rather than explicit. On this account an explicit contradiction is a single sentence which is self-contradictory; while for one's set of beliefs to contain an implicit contradiction one must maintain mutually inconsistent opinions, whether actively or inactively. The two ways of drawing the distinction are not always kept separate and the fear of explicit contradictions of the second sort may make us wary of the first sort also.

The prohibition against explicit contradictions of the second sort may be expressed as the requirement that every member of a set of beliefs must be logically capable of being true. This stricture seems plausible enough but it does not preclude sets of beliefs which contain implicit contradictions of the second sort. Moreover, it seems likely that implicitly inconsistent belief sets of the second sort are quite common.[6]

On the other hand the notion of someone believing an explicit contradiction of the second kind seems devoid of content. One might argue that it is part of the root meaning of 'belief' that the sentence expressing it must be at least capable of being true. If the classical penalties of triviality are to imposed anywhere, let them fall upon a transgression which no one is capable of committing. We feel no qualms at the prospect of requiring anybody who believed an explicit contradiction to believe everything.

The stronger restriction upon belief sets corresponds to the requirement that such sets be satisfiable, which is to say that all the members be capable of simultaneous truth. It is this condition which flies so directly in the face of our ordinary experience of belief. We have now arrived at a crux.

If we let ordinary experience (not to say common sense) be our guide allowing that our belief sets to contain implicit contradiction (of the second kind) but not explicit contradictions, we must of necessity abandon classical logic. For by means of the classical picture of inference we may always derive an explicit contradiction from a set of beliefs which is implicitly inconsistent. Thus classical modes of reasoning trample what is manifestly a useful distinction.

How far ought this felt need for a non-classical logic to be indulged? Appeals to experience can easily lead us astray. After all, almost anybody who proposes some species of non-classical logic is tempted to argue that since the world is non-classical so

[6] Some have argued that to be rational one must have such a belief set. See (Campbell, 1980).

should our logic be. To travel to far along this road is to invite some form of the genetic fallacy.

If our arithmetic or our syllogisms are not in accord with the classical canons, the fault might well lie in our inability to calculate or to construct Venn diagrams. The observation that the reasonings of actual people are sometimes non-classical, in order to bite, must be coupled with evidence that such reasoning is correct, or at the very least not evidently mistaken. If invention is to be mothered let it be through necessity and not through the failure to take suitable precautions.

Nevertheless some of the evidence is compelling. Even when our stock of beliefs is inconsistent we routinely draw a distinction between what follows from it and what does not, and regard certain inferences from our beliefs as improper, in spite of the circumstances. If our procedures were genuinely classical we would not, indeed could not, make such a distinction. There is no classical issue to be taken with any inference from an inconsistent set.

Failure to take this sort of evidence into account seems as misguided as any of the worst excesses of those who would base logical theory democratically upon the actual inferential practice of the proletariat. It is no doubt true that if the rule

> If A then B; B; therefore A.

(sometimes known as Modus Morons) were from time to time invoked in country districts or local government, we should nevertheless resist viewing it as a ratiocinative dialect, or alternative inferential lifestyle. But if humanity universally manages to distinguish, at least in some cases, valid arguments from inconsistent premises on the one hand from invalid arguments from those same premises, this is another and weightier matter.

The view that everyone commits a logical error in making such a distinction is an adaptation for logic of the doctrine of original sin.

We have already considered one non-classical view of the matter, namely the view that when we argue from our beliefs we disregard some, reasoning only from the active portion of our belief set. That this is non-classical is evident from the fact that it is non-monotonic. Whatever sense of 'inferable' we adopt, it is clear that we do not think of our conclusions as inferable in that significant way from an inconsistent set. The reason for such a non-monotonic stance is precisely that the whole of one's belief might be seen to be inconsistent if examined, that an inference drawn from the set taken as a whole might be trivial. To escape this consequence we must take the further step of claiming that the only real beliefs are the active ones.

This protestant view, which is really an attempt to deny that beliefs can be inconsistent, suffers from two flaws. The first is that on this doctrine it is nigh on impossible to identify a belief set. For suppose someone's apparent belief set is inconsistent. Which subset of the set of apparent beliefs constitutes the *real* belief set? (assuming that the real beliefs are also apparent; otherwise the problem becomes even worse). Even if we insist that the real beliefs form a maximal consistent subset of the apparent beliefs we do not thereby guarantee uniqueness. We must elaborate more sophisticated side conditions to ensure this, perhaps invoking the probability calculus to assist. In this case we must have handy some interpretation of probability other than one of its belief interpretations. The project seems a mare's nest. But these difficulties would not deter us from the task were it not for the second flaw.

The second flaw is that the basic premise is false. Try any thinking beyond the wallpaper or musak variety of daily life, and inconsistent pairs of beliefs *do* turn up, the one belief as active as the other.

2.4 Inconsistency and Ethics

It seems to be part of our ordinary moral experience that we can find ourselves in a dilemma, which is to say a situation in which the demands of morality cannot (all) be heeded. Let us suppose that such situations might extend beyond the world of everyday all the way into philosophy.[7] Let us next ask the question of whether or not obligations[8]are 'closed under logical consequence.' This is an example of a question which is both of interest to ethical theory, and also to logic. Some philosophers, even some ethicists, might object that the question is one that only a logician could love, and that they, as ethicists, have not the slightest interest in it. Such a position betrays a lack of thought.

The issue is really quite deep. If you deny that the logical consequences of obligations are also obligations, then you are saying that moral argument, at least moral argument of a certain standard kind, is impossible. When you and I enter into a moral dispute, a dispute over whether or not you ought to bring it about that P say, then what often, perhaps usually, happens is that I try to demonstrate that P follows from something, some general moral principle, to which you subscribe. In stage one then, we obtain the subscription.

> Don't you think that we ought to help those who cannot help themselves?
> Not in every case, suppose somebody is both helpless and not in need of help?
> Very well, we ought to help those in need who cannot help themselves?
> Wonderfully high minded, but impossible I'm afraid.

[7]Such a supposition is by no means beyond the bounds of controversy. There are moral philosophers who argue that such 'conflicts of obligation' are merely apparent.

[8]We are sacrificing rigor in the cause of clarity in this example. Strictly speaking we are not talking about 'obligations' at all, since those are typically *actions*. We should be talking rather about 'sentences which ought to be true.' This is a much more awkward form of words however and it would interfere with the flow without adding very much to what we are saying.

> Why impossible?
> We are overwhelmed by the numbers don't you see?
> The numbers of what? Those in need?
> Precisely! We would spend all our resources helping the needy only to join their ranks before long.
> Ah, well how about this modification then: We ought to help those in need who cannot help themselves, but not to the point of harming ourselves or even seriously inconveniencing ourselves?
> Yes, it doesn't sound so high-minded, but now it's something I can support.

Now we close in for the kill.

> Would you say that Jones is in need of help?
> Yes, not much doubt about that, poor bloke he.
> We should pass the hat, don't you think? Everybody could put in what they can easily spare.
> I don't see the point in that.
> Do you imagine then that Jones can pull himself up by the bootstraps, that all he needs is sufficient willpower?
> No, I wouldn't say that, not in his present state at least.
> Then, we ought to help him, wouldn't you say?
> Well somebody needs to help him if he's going to be helped, but I don't see how that got to be my problem.

What has been shown is that 'We help Jones' is a logical consequence of 'We help those in need who cannot help themselves and whose help is not a significant burden to us.' This much is non-controversial. Having first agreed upon 'We ought to help those in need ...'. But the remaining step, the step from the forgoing to 'We ought to help Jones' will only follow if the logical consequences of obligations are themselves obligations. It is just crazy to say that the issue is of no interest to ethicists, unless the ethicists in question have no interest in arguments like our sample.

Nor can the relevance of logic to ethics be exhausted by this one issue. Suppose that in exasperation, our stubborn ethicist agrees that the issue is of interest after all. But the interest is over once we see that we must answer 'yes' to the question of whether or not the consequences of obligations are themselves obligations, isn't it? Not at all. Recall how this got started.

We were wondering about moral dilemma, about *conflict of obligations*. In view of what we just said about obligations and logical consequences, we now have a problem. Suppose I am in a moral dilemma, that there are two sentences P and Q which I believe should both be true (I believe that each, individually, *ought* to be true). But the two cannot be true together—hence the dilemma.

This is bad enough, but it gets worse. One of the consequences of the set of sentences which we say ought to be the case, the set of obligations, is the sentence $P \wedge Q$. But since the two sentences cannot both be true at the same time, this consequence is equivalent to $P \wedge \neg P$. Since the consequences are also obligations, $P \wedge \neg P$ is an obligation. But *everything* follows from $P \wedge \neg P$ so in the case of a moral dilemma, one is obliged to do *everything*. This seems a bit burdensome.

2.5 Summing Up

The unhappy fact of the matter is that sometimes, through no fault of our own, we are simply stuck with bad data. This can happen in any of the variety of circumstances in which we gather information from several sources. Should the sources contradict each other, we may have a way of measuring their relative reliability in some reasonable way. Equally often however either the means at our disposal will not settle the matter conclusively (as in the case of beliefs) or we simply have no means for the adjudication of conflict between our sources.

The classical theory of inconsistency must retire in such a situation since non-trivial classical reasoning is impossible in the face of inconsistent premise sets. All that the classical logician can advise in these circumstances is to start over again with a consistent set of premises. Sometimes this is wise counsel but more often it is of a piece with what the doctor said when told by a patient: 'It hurts when I do this.'

To get back to our earlier example, both belief and obligation seem to call for the Lewis distinction (at least the one that remains after we 'fix' the original claim). We need to be able to distinguish premise sets which contain full-blooded self-inconsistencies from those which do contain pairs of premises which are inconsistent although the individual premises are not.

This is the path which we shall follow—the one we shall eventually call *preservationist*. But we should point out that there is another path which is quite distinct from ours but which claims for itself all the merits of our approach and perhaps others besides.

This other account may be distinguished from ours by being a whole-hearted replacement theory, where ours is a revision theory. According to the account dialethism,as it's defenders have come to call it, there can be premise sets in which both a sentence A and its negation not-A appear without any ill effect. Without that is, the premises having as the set of consequences, the set of all sentences. What is more, such a set will also contain the single conjunction 'A and not-A'. Subscribers to this account suppose that the true meaning of 'not' is such as to make 'A and not-A' capable of being true. This move allows the satisfiability condition to reappear since many more sets (perhaps even every set) will be satisfiable.

As a replacement theory this has all the drawbacks of such when the target of replacement is classical logic. But quite apart from the resistance of 'vested interests,' the dialethists must also endure the scorn of even the naive and uncommitted. The very idea of 'true contradiction' seems to be about as counterintuitive as one could imagine, unless one is addicted to continental philosophy of the more literary sort.

In mincing negation, this scheme avoids the classical consequences of contradiction by changing the meaning of 'contradiction.' Let us say at once that the dialethists have many virtues. The boldness of their approach is admirable, and they have been far from lazy. Both the theory underlying the move and its inferential consequences have been energetically worked out and widely published. But where one ought to feel release and fill one's lungs, one feels a kind of dull unease. For even if classical negation is not primordial in human language, but merely a contrivance of latter days, it is nonetheless with us and even if not all, at least some of our contradictions seem to be on that scale.

Three

On Preserving

GILLMAN PAYETTE AND PETER SCHOTCH

Abstract

In this essay Gillman Payette and Peter Schotch present an account of the key notions of level and forcing in much greater generality than has been managed in any of the early publications. In terms of this level of generality the hoary notion that correct inference is truth-preserving is carefully examined and found wanting. The authors suggest that consistency preservation is a far more natural approach and one furthermore than *characterizes* an inference relation. But an examination of the usual account of consistency reveals problems which, in general, can be corrected by means of an auxiliary notion of inference (forcing) which relies upon a kind of generalization of consistency, called level. Preservation of the latter is shown to be another of the properties which characterize a logic and forcing is shown to preserve it. The essay ends with a sketch of a result which locates forcing among all possible level-preserving inference relations.

3.1 Introduction

The (classical) semantic paradigm for correct inference is often given the name 'truth-preservation.' This is typically spelled out to the awe-struck students in some such way as:

> An inference from a set of premises, Γ, to a conclusion, α, is correct, say *valid*, if and only if whenever all the members of Γ are true, then so is α.

This understanding of the slogan may be tried, but is it actually true? There is a problem: the way that 'truth' is used in connection with the premises is distinct from the way that it is used with the conclusion. In other words, this could be no better than a quick and dirty gloss. The chief virtue of the formulation is that of seeming correct to the naive and untutored.

But what of the sophisticates? They might well ask for the precise sense in which truth is supposed to be preserved in this way of unpacking. On the right hand side of the 'whenever' we are talking about the truth of a single formula while on the left hand side we are talking about the truth of a bunch of single formulas. Is it the truth of the whole gang which is 'preserved?'

Of course it is open to the dyed-in-the-wool classicalist to reply scornfully that we need only replace the set on the left with the conjunction of its members. In this way is truth preserved from single formula to single formula as homogenously as anyone could wish.

It is open, but not particularly inviting. In the first place, this strategy forces us to restrict the underlying language to one which has conjunction and conditional connectives—which must operate in something like the usual (which is to say classical) way. There are enough who would chafe under this restriction, that a sensitive theorist would hesitate to impose it.

Apart from the objection, we are inclined to think of this business of coding up the valid inferences in terms of their 'corresponding conditionals'[1] as an accident of the classical way of thinking and that it is no part of the *definition* of a correct account

[1] We think this usage was coined first by Quine.

of inference. We also notice that on the proposal, we are restricted to finite sets.

Setting aside this unpalatable proposal then, we ask how is this notion of truth-preservation supposed to work? Since there is no gang on the right we seem to be talking about a different kind of truth, individual truth maybe, from the kind we are talking about on the left—mass truth perhaps. Looked at in that somewhat jaundiced way, there isn't any preserving going on at all, but rather a sort of transmuting.

The classical paradigm *really* ought to be given by the slogan 'truth transmutation.' In passing from the gaggle of premises to the conclusion, gaggle-truth is transmuted into single formula truth. It may be more correct to say that, but it makes the whole paradigm somewhat less forceful or even less appealing.

What we need in order to rescue the very idea of preservationism is to talk entirely about sets. So we shall have to replace the arbitrary conclusion α with the entire *set* of conclusions which might correctly be drawn from Γ. We even have an attractive name for that set—the *theory* generated by Γ or the *deductive closure* of Γ. In formal terms this is

$$\mathbb{C}_{\vdash}(\Gamma) = \{\alpha | \Gamma \vdash \alpha\}$$

Now that we have sets, can we say what it is that gets preserved—can we characterize classical inference, for instance, as that relation between sets of formulas and their closures such that the property Φ is preserved?

We can see that gaggle-truth would seem to work here in the sense that whenever Γ is gaggle-true so must be $\mathbb{C}_{\vdash}(\Gamma)$, for $\vdash$ the classical notion of inference at least. We are unable to rid ourselves, however, of the notion that gaggle-truth is somewhat lacking from an intuitive perspective. Put simply, our notion of truth is carried by a predicate which applies to *sentences*, or formulas if we are in that mood. These are objects which might indeed belong to sets but they aren't themselves sets. So however we construe the idea of a true set of sentences (or formulas) that construal will involve a stipulation, or more charitably, a new definition.

Generations of logic students may have been browbeaten into accepting "A set of sentences is true if and only if each member of the set is true." but it *is* a stipulation and is no part of the definition of "true." It doesn't take very much imagination to think that somebody might actually balk at the stipulation. Somebody who is attracted to the idea of *coherence* for instance, might well want to say that truth must be defined for (certain kinds of) sets first and that the sentential notion is derived from the set notion and not conversely. All of which is simply to say that a stipulation as to how we should understand the phrase "true set of sentences" is unlikely to be beyond the bounds of controversy.[2]

It may gladden our hearts to hear then, that there is another property, perhaps a more natural one, which will do what we want. That alternative property is *consistency*.

3.2 Making A Few Things Precise

By a *logic* X, over a language $\mathcal{L}$ we understand the set of pairs $\langle\Gamma, \alpha\rangle$ such that Γ is a set of formulas from the language $\mathcal{L}$ and α is a formula from that same language, and $\Gamma \vdash_X \alpha$. In the sequel we frequently avoid mention of the language which underlies a given logic, when no confusion will thereby be engendered.

This set of pairs is also referred to as the *provability* or *inference relation* of X. In saying this we expose our extensional viewpoint according to which there is nothing to a logic over and above its inference relation. This has the immediate consequence that we shall take two logics X and Y which have the same inference relation, to be the same logic.

[2]It may be helpful here to consider an analogy between sentences and numbers, taken to be *urelementen*. We can define the idea of a prime number easily enough but be puzzled about how to define a prime *set* of numbers. Somebody might be moved to offer: "Why not simply define a prime set of numbers to be a set of prime numbers?" The answer is likely to be: "Why bother?" indeed the whole idea of a prime set of numbers seems bizarre and unhelpful. We can easily imagine circumstances in which we would require a set of prime numbers but the reverse is true when it comes to a prime set.

When X is a logic, we refer to the X-deductive closure of the set Γ by means of '$\mathbb{C}_X(\Gamma)$'

Unless the contrary is specified, Every logic mentioned below will be *compact*, which is say that whenever $\Gamma \vdash \alpha$ it follows that there must be some finite subset, say Δ, of Γ, which proves α.

In mentioning consistency, we have in mind some previously given notion of inference, say $\vdash_X$. Each inference relation spawns a notion of consistency according to the formula

> Γ is consistent, *in* or *relative to* a logic X (alternatively, Γ is X-consistent) if and only if there is at least one formula α such that $\Gamma \nvdash_X \alpha$.

To say this in terms of provability rather than non-provability we might issue the definition:

> Γ is *inconsistent* in a logic X if and only if $\mathbb{C}_X(\Gamma) = \mathbb{F}$, where $\mathbb{F}$ is the set of all formulas of the underlying language of X.

> Where X is a logic, the associated consistency predicate (of sets of formulas) for X, is indicated by CON_X.

We were interested in how an inference relation might be characterized in terms of preserving some property of sets. We have singled out consistency as a natural property of sets and having done that we can see that preservation of consistency comes very naturally indeed. The time has come to say a little more exactly what we mean by 'characterized.' In order to do this we shall be making reference to the following three *structural* rules of inference.

[R] $\alpha \in \Gamma \implies \Gamma \vdash \alpha$

[Cut] $\Gamma, \alpha \vdash \beta \;\&\; \Gamma \vdash \alpha \implies \Gamma \vdash \beta$

[Mon] $\Gamma \vdash \alpha \implies \Gamma \cup \Delta \vdash \alpha$

Unless there is a specific disavowal every inference relation we consider will be assumed to admit these three rules. It should be noted that on account of [Mon], if the empty set $\varnothing$ is inconsistent in X, then the inference relation for that logic contains every pair $\langle \Gamma, \alpha \rangle$. In such a case we say that X is the *trivial* logic over its underlying language. We shall take the logics we mention from now on to be non-trivial, barring a disclaimer to the contrary.

Let us say that an inference relation, say '$\vdash_X$', *preserves consistency* if and only if:

> If Γ is X-consistent (in the sense of the previous definition), then so is $\mathbb{C}_X(\Gamma)$.

It is easy to see that every inference relation with [Cut] and [Mon] must preserve consistency since if the closure of a set, Γ proves some formula, α, then by compactness some finite sequence of [Cut] operations will lead to the conclusion that Γ proves α. It may be that we end up showing that some subset of Γ proves α, which is why we require [Mon] in this case.

We say that X preserves consistency *in the strong sense* when the condition given above as necessary, is also sufficient.

It is similarly easy to see that since every set is contained in its deductive closure by [R], and since inconsistency is preserved by supersets, given [Mon], every inference relation satisfying the three structural rules preserves consistency in the strong sense.

This is all very well, but we haven't really gotten to anything that would single out an inference relation from among a throng of such all of which preserve consistency. In order to do that it will be necessary to talk about a logic X preserving the consistency predicate of a logic Y, in the strong sense.

A moment's thought will show us that when the preservation is mutual—X and Y preserve each other's consistency predicates, (Which implies that they share a common underlying language) then they must agree on which sets are consistent and which are inconsistent. For consider, if $\text{CON}_X(\Gamma)$ and Y preserves the X consistency predicate then $\text{CON}_X(\mathbb{C}_Y(\Gamma))$. Suppose that

Γ is not Y-consistent, then $\mathbb{C}_Y(\Gamma) = \mathbb{F}$. By [R] $\mathbb{C}_X(\mathbb{C}_Y(\Gamma)) = \mathbb{C}_X(\mathbb{F}) = \mathbb{F}$ which is to say that $\mathbb{C}_Y(\Gamma)$ is not X-consistent, a contradiction. Similarly for the argument that Γ is consistent in Y and X preserves the Y consistency predicate.

When two logics agree in this way, i.e. agree on the consistent and inconsistent sets, we shall say that they are *at evens*. Another moment's thought reveals that two logics which are at evens will preserve each other's consistency predicates. Assume X and Y are at evens and $\text{CON}_X(\Gamma)$ but $\overline{\text{CON}_X}(\mathbb{C}_Y(\Gamma))$, where the overline indicates predicate negation. Then $\overline{\text{CON}_Y}(\mathbb{C}_Y(\Gamma))$ because X and Y agree on inconsistent sets. Whence by idempotentcy of $\mathbb{C}_Y$ Γ is not consistent in Y, a contradiction. Thus we have,

Proposition 1. *Any two logics X and Y over a language $\mathcal{L}$ are at evens if and only if X and Y preserve each other's consistency predicates.*

This is nearly enough to guarantee that X and Y are the same logic. All we need is a kind of generalized negation principle:

Definition 1. *A logic X is said to have* denial *provided that for every formula α, there is some formula β such that $\overline{\text{CON}_X}(\{\alpha, \beta\})$.*

In such a case we shall say that α and β deny each other (in X, which qualification we normally omit when it is clear from the context). We will assume that any logic we mention has denial.

Clearly if a logic has classical-like negation rules then it has denial, since the negation of a formula will always be inconsistent with the formula. Of course classically, there are countably many other formulas which are inconsistent with any given formula—namely all those which are self-inconsistent. The generalized notion doesn't require that there be distinct[3] denials for each formula, only that there be some or other formula which is not consistent with the given formula.

Evidently, if two logics are at evens, then if one has denial, so does the other. In fact something stronger holds, namely:

[3] Distinct up to logical equivalence, it goes without saying.

Proposition 2. *If two logics X and Y are at evens, and X has denial then, for every formula α there is some formula β for which both $\overline{\text{CON}_X}(\{\alpha, \beta\})$ and $\overline{\text{CON}_Y}(\{\alpha, \beta\})$.*

Finally, we shall require of our logics that they satisfy the principle that denial commutes with provability in the right way:

[Den] $\Gamma \vdash_X \alpha \iff \overline{\text{CON}_X}(\Gamma \cup \{\beta\})$ where β denies α.

Now we are ready to state our result:

Theorem 1 (Generalized Consistency Theorem). *Two logics X and Y are at evens if and only if, X and Y are the same logic.*

Proof. For this argument we split the equivalence into its necessary and sufficient halves.

($\implies$) Assume $\mathbb{C}_X(\Gamma) = \mathbb{C}_Y(\Gamma)$ for every set Γ—which is to say that $X = Y$. To say that $\overline{\text{CON}_X}(\Gamma)$ is to say that the X-closure of Γ is $\mathbb{F}$. But then so must be the Y-closure of Γ. Similarly, to say that $\text{CON}_X(\Gamma)$ is to say that there is some α which is not in the X-closure of Γ, but then neither can α be in the Y-closure of Γ, hence $\text{CON}_Y(\Gamma)$. So X and Y are at evens.

($\impliedby$) Suppose then that X and Y are at evens. Let Γ be a consistent set, which means by the assumption, that it is consistent in both logics. Assume for reductio that $\Gamma \vdash_X \alpha$ and $\Gamma \nvdash_Y \alpha$, and let β deny α. Thus,by [Den] $\overline{\text{CON}_X}(\Gamma \cup \{\beta\})$ and $\text{CON}_Y(\Gamma \cup \{\beta\})$, a contradiction.

□

3.3 What's Wrong With This Picture?

To answer the question in the section heading, there really isn't anything wrong with an approach which characterizes inference in terms of preserving consistency. It's consistency itself, or at least many accounts of it, which casts a shadow over our everyday logical doings.

The way we have set things up, a set Γ of formulas is either consistent in a logic X, or it isn't. But it doesn't take much thought to see that such an all-or-nothing approach tramples some intuitive distinctions. In particular, we may find the *reason* for the inconsistency to be of interest.

In the logic X, for example, there may be a single formula δ which is, so to speak, inconsistent *by itself*. In other words $\overline{\mathrm{CON}_X}(\{\delta\})$. Formulas of this dire sort are sometimes described as being *self-inconsistent* (in X) or *absurd* in X. By [Mon] any set of formulas which contains a self-inconsistent formula is bound to be inconsistent.

Thinking of the possible existence of absurd formulas leads us to sharpen our previous notion of denial:

Definition 2. *We shall say that X has* non-trivial *denial if and only if for every non-absurd formula α there is at least one non-absurd formula β which is not X-consistent with α.*

We are now struck by the contrast between X-inconsistent sets which contain X-absurdities and those which do not. Isn't there an important distinction between these two cases? If we think of consistency as a desirable property which we are willing to trouble ourselves to achieve, then the trouble will be light indeed if all we need do is reject absurdities. On the other hand, an entire lifetime of angst may await those who wish to render consistent their beliefs or their obligations.[4] There is a great deal more that one could say on this topic and some of the current authors have said much of it. For now, we shall take it that the need for a distinction has been established and our job is to construct an account of consistency which allows it.

We have in mind building upon what we have already discovered instead of pursuing a slash-and-burn policy. This means, among other things, that the predecessor account should appear as a special (or limiting) case of the new proposal. The intu-

[4] In saying this, we assume that nobody is obliged to bring about anything impossible and that whatever is self-inconsistent cannot truly be a belief.

itive distinction bruited above, is clearly a distinction between different kinds of inconsistency, or perhaps different degrees. We might think of one kind being *worse* than the other, which leads to a rather natural way of classifying inconsistency.

3.4 Speak of the Level

The account of inconsistency which we propose is a generalization of the one first suggested in the 20th Century, in the work of Jennings and Schotch[5], namely the idea of a *level* (of incoherence, or inconsistency). The basic idea is that we can provide an intuitive measure of how inconsistent a set is by seeing how finely it must be divided before all of the divisions are consistent. What, in the earlier account is stated in terms of classical provability, we now state in terms of arbitrary inference relations which satisfy the minimal conditions given in the earlier section.

It all begins with the notion of a certain kind of indexed collection of sets being a *logical cover* for a set Γ of formulas, in the logic X. First we need a special kind of indexed family of sets (of formulas).

Definition 3. $A(\Delta) = \{a_0, a_1, \ldots, a_\xi\}$ *is an indexed set* starting with Δ, *provided* $a_0 = \Delta$ *and all the indices* $0 \ldots \xi$ *are drawn from some index set* I.

Definition 4. *Let* $\mathfrak{F}$ *be an indexed set starting with* $\varnothing$. $\mathfrak{F}$ *is said to be a* logical cover *of the set* Σ, *relative to the logic* X, *indicated by* $\text{COV}_X(\mathfrak{F}, \Sigma)$, *provided:*

> *For every element* a *of the indexed family,* $\text{CON}_X(a)$ *and*
>
> $\Sigma \subseteq \bigcup_{i \in I} \mathbb{C}_X(a_i)$

So an X-logical cover for Γ is an indexed family of sets starting with the empty set, such that there are enough logical resources

[5] See especially Schotch and Jennings (1980a, 1989)

in the cover to prove, in the logic X, each member of Γ. Evidently, given the rule [R], $\{\varnothing, \Gamma\}$ will always be a logical cover of Γ if the latter set is X-consistent, though it won't, in general be the least.

If $\mathfrak{F}_\Sigma$ is a logical cover for the set Σ, the cardinality $|I| - 1$ where I is the index set for $\mathfrak{F}_\Sigma$, is referred to as the *width* of the cover, indicated by $w(\mathfrak{F}_\Sigma)$.

In the special circumstance that all the members of a logical cover of Σ are disjoint, the cover is said to *partition* Σ.[6]

And finally we introduce the notion at which we have hinted since the start of this section.

Definition 5. *The* level *(relative to the logic X) of the set Γ of formulas of the underlying language of X, indicated by $\ell^X(\Gamma)$ is defined:*

$$\ell^X(\Gamma) = \begin{cases} \min\limits_{w(\mathfrak{F})}[\mathrm{COV}_X(\mathfrak{F}, \Gamma)] & \textit{if this limit exists} \\ \infty & \textit{otherwise} \end{cases}$$

In other words: the X-level (of incoherence or inconsistency) of a set Σ in a logic X is the width of the narrowest X-logical cover of Σ, if there is such a thing, and if there isn't, the level is set to the symbol ∞.

One might think that there will fail to be a narrowest logical cover when there is more than one—when several are tied with the least width, but this is a misreading of the definition. There might indeed be several distinct logical covers but there can only be one least width (which they all share). The uniqueness referred to in the definition attaches to the width, not to the cover, so to speak.

The only circumstance in which there might fail to be a narrowest logical cover, is one in which Σ has no logical covers at all. In this circumstance Σ must contain what we earlier called an absurd formula.

[6]This should be contrasted with a covering family *being* a partition. We can recover the latter notion from this one by intersecting the covered set with each of the disjoint sets in the logical cover.

This notion satisfies both the requirement that it distinguishes between inconsistent sets which contain absurd formulas, and those which don't, and the requirement that the predecessor notion of consistency relative to X appears as a special case. For it is clear that if Γ is an X-consistent set of formulas then a narrowest logical cover of Γ is $\{\varnothing, a_1\}$ where $\Gamma \subseteq \mathbb{C}_X(a_1)$ and $\text{CON}_X(a_1)$. So at least part of the earlier notion of $\text{CON}_X(\Gamma)$ is captured by $\ell^X(\Gamma) = 1$.

There is even an interesting insight which comes out of this new idea. For there are *two* levels of X-consistency, 0 and 1. In our earlier naive approach, we thought of consistency as an entirely monolithic affair, but once we give the matter some thought we see that the empty set does indeed occupy a unique position in the panoply of X-consistent sets. If we know only that Γ and Σ are both X-consistent, nothing at all follows about the X-consistency of $\Gamma \cup \Sigma$. But we may rest assured that both $\Sigma \cup \varnothing$ and $\Gamma \cup \varnothing$ are X-consistent. And the same goes for the X-consequences of the empty set, namely X-theorems. By our definition, $\ell^X(\Delta) = 0$ if Δ is empty or any set of X-theorems, and of course such sets are consistent with any X-consistent set of formulas. It is tempting to call these level 0 sets, *hyperconsistent*.

3.5 Level Preservation

So now that we have the concept of an X-level, should we be concerned about preserving such a thing? Perhaps there is no need for such concern, since it is at least possible that the logic X preserves its own level, isn't it? Well, in a word, no. It is not in general true that X preserves level beyond, of course the levels 0 and 1 of X-consistency. All the logics we consider not only do that, but are characterized by doing that.

Suppose the set Γ contains not only the formula α but also a denial β of α although it does not contain any X-absurdities. Now, since X has the rule [R], Γ must X-prove both α and β. If X permits the arbitrary conjunction of conclusions, then Γ will prove an X-absurd formula, namely $\alpha \wedge \beta$. Hence by the meaning of closure, that absurdity belongs to the X-closure of Γ, which must thus have X-level ∞. This amounts to a massive failure to preserve level.

The obvious question to raise is this: given that ℓ^X is a generalization of CON_X, is it the case that level characterizes logics in the same way that preserving consistency (in the strong sense) does? If not, then it seems that the generalization is not perhaps as central a notion as the root idea upon which it generalizes. Fortunately, for supporters of the general notion, we may prove the following generalization of the Generalized Consistency Theorem.

Theorem 2 (Level Characterization Theorem). *Suppose that X and Y are inference relations over the same language $\mathcal{L}$ and let ℓ^X and ℓ^Y be the level functions associated with the respective inference relations. Then*
$[\ell^X(\Gamma) = \ell^Y(\Gamma)$ *for every set* Γ *of formulas of* $\mathcal{L}] \iff X = Y$.

Proof. The proof depends upon the Generalized Consistency Theorem.

($\Longrightarrow$) Assume that $\ell_X(\Gamma) = \ell_Y(\Gamma)$ for every set Γ of formulas of $\mathcal{L}$. Then by definition the two agree on which sets have level 1 and level 0. But this is to say that X and Y agree on which sets are consistent. But by the Generalized Consistency Theorem, any two such logics (logics which we say are at evens) must be identical.

($\Longleftarrow$) Assume that $X = Y$ and Suppose that for some arbitrary set Γ of formulas of $\mathcal{L}$ $\ell^X(\Gamma) = \xi$, ξ some cardinal. Then, by the definition, there is a narrowest X-logical cover $\mathfrak{F}_\Gamma$ such that $w(\mathfrak{F}_\Gamma) = \xi$. Since $X = Y$, it must be the case by the Generalized Consistency Theorem that the two logics agree on consistency (in the strong sense). Further, by definition each $a_i \in \mathfrak{F}_\Gamma$ is such that $\text{CON}_X(a_i)$. But then, since X and Y are at evens,$\text{CON}_Y(a_i)$. Thus, $\mathfrak{F}_\Gamma$ must be a Y-logical cover of Γ of width ξ. Moreover, this must be the narrowest such logical cover or else by parity of reasoning, there would be an X-logical cover of cardinality less than ξ contrary to hypothesis. Since Γ was arbitrary it follows that ℓ^X and ℓ^Y must agree on all sets of formulas of the language $\mathcal{L}$.

□

This suggests that level is worth preserving, that it is a sort of natural logical kind, but doesn't show how the preservation may be carried out. It is time to repair that lack.

Perhaps the most straightforward route to preserving X-level is to define a new inference relation based on X. Evidently the definition in question must also connect somehow with the notion of X-level and thus ultimately to CON_X (from now on we shall mostly drop reference to the background logic, like X, when no confusion will result). The process might have been informed by the ancient joke:

Question: How do you get down from an elephant?

Answer: You don't get down from an elephant, you get down from a duck.

except in our case the question and answer would go:

Question: How do you reason from inconsistent sets?

Answer: You don't reason from inconsistent sets, since every formula follows in that case, you reason from consistent *subsets*.

In other words, an inconsistent set is one for which the distinction between what follows and what doesn't has collapsed. This lack of meaningful contrast means that it no longer makes sense to talk about drawing conclusions from such a set. In order to regain the distinction we are going to have to drop back to the level of consistency and the only way to do that, is to look at consistent subsets of the original set.

Absent the notion of level, there are different ways to do this. The one suggested in Quine and Ullian (1970) to deal with inconsistent sets of beliefs, involves two stages: At the first stage we discover the smallest subset of the inconsistent set which still exhibits the inconsistency. At the second stage we discard the member of the inconsistent subset with the least evidence, and repeat as necessary until the set is consistent. Having thus cleansed the belief set, we may now draw conclusions as we did before.[7]

We don't say that this process can't work. We do say that it doesn't seem to work in every case. There is a clear difficulty here when the two conditions on rational belief: consistency (which we might call the external condition) and evidential support, (the internal condition) pull us in different directions.

In the lottery paradox, for instance, we seem to have good evidence for each one of the lottery beliefs (ticket 1 won't win, ticket 2 won't win,. . . , ticket n won't win.) and we can make the evidence as strong as we like by making the lottery ever larger. Now conjoin the beliefs and we get 'No ticket will win.' which contradicts fairness. We could get consistency by throwing out the belief that the lottery is fair, but that would be cheating. The problem is that each of the lottery beliefs has exactly the same support as the others. They stand or fall as one, it would seem. If we let them all fall, then the rationality of buying a lottery ticket would seem to follow or at least the non-irrationality. But isn't it true that it isn't rational, according to the accepted canons at least, to buy a lottery ticket?[8]

Leaving aside the possibly controversial issue of the lottery paradox, take any situation in which we are unable to find a rationale for discarding one member of an inconsistent subset rather than another. Quine seems to suggest that in this situation, the counsel of prudence is to wait until we do find some way to distinguish among the problematic beliefs. Those with less patience

[7] This procedure was intended to apply to classical inference, but the method obviously generalizes to cover cases in which the base inference is X.

[8] Not for nothing have lotteries long been known as 'a tax on fools.'

seem to regard random discarding until at last we get to consistency, to be the path of wisdom.[9] We are inclined to reply to Quine that patience, for all that it is a virtue, is sometimes also a luxury we cannot afford or even a self-indulgence that we do well to deny ourselves.

To the others we say, consistency is not a virtue which trumps everything else. Suppose we might achieve consistency by throwing away one of α or β though we have no reason to prefer one over the other. Flipping a coin is a method for determining which goes to the wall, but we have no way of knowing if we have determined the correct one. We have left ourselves open to having rejected a truth or accepted a falsehood. 'Yes, but at least we now have consistency!' won't comfort us much if the consequences of picking the wrong thing to throw away are unpleasant enough.

Let us take up level once more.[10] In saying the level of the set Σ is k, we are saying two things. First that there is a way to divide the logical resources of Σ into k distinct subsets each of which is consistent. From now on we shall refer to these consistent subsets as *cells*. Second, that any way of thus dividing Σ must have at least k cells. Here we have got to the level of consistency but we've got there k times. Not only might we wonder which of the k-cells is the 'real' one, the one which best represents 'the way things really are,' but there may be lots and lots of distinct ways to form the k cells. Which of the possibly many ways should we privilege?

At this point, we cannot answer these questions, which means that we must treat the cells on an equal footing along with the various ways of producing them.[11] In saying this, we say that we shall count as a consequence of Σ in the reconstructed inference

[9] This seems to be the route advocated some of those in computing science who have devised so-called truth-maintenance systems.

[10] We realize that the Quinean suggestion is not the only one, though it might be the most well-known, to deal with inconsistent sets of formulas. We do not, however, intend this essay as a survey of all of so-called paraconsistent logic.

[11] Which is not to say that there is no way. Elsewhere one might find suggestions which narrow the range of ways of dividing up our initial set. In this connection see the discussion of A-forcing in Schotch and Jennings (1989).

relation, whatever formula follows (in the 'underlying' logic, say X) from at least one cell in every way of dividing Σ into k cells.

When the underlying logic is X, the derived inference relation is called X-level forcing, which relation in indicated by $[\Vdash_X$. We can give the precise definition as:

Definition 6. *$\Gamma[\Vdash_X \alpha$ if and only if, for every division of Γ into $\ell^X(\Gamma)$ cells, for at least one of the cells $\Delta, \Delta \vdash_X \alpha$*

It is easy to see that:

Proposition 3. *If $\Gamma[\Vdash_X \alpha$ then $\ell^X(\Gamma) = \ell^X(\Gamma \cup \{\alpha\})$*

Proof. Suppose the condition obtains and let $\ell^X(\Gamma) = k$. It follows from the definition that every division of Γ into k cells, results in at least one cell that X-proves α. But then we could add α to the cell in question without losing the cell property since X is a logic which preserves consistency. In such a case, after adding α we would have a division of $\Gamma \cup \{\alpha\}$ into k cells. Moreover there couldn't be a division of $\Gamma \cup \{\alpha\}$ into fewer than k cells without there being a similar division of Γ which would contradict the hypothesis. □

It obviously follows directly from this that:

Corollary 1. *$[\Vdash_X$ preserves X-level, in the sense that $\ell^X(\Gamma) = \ell^X(\mathrm{CL}_{[\Vdash_X}(\Gamma))$*

3.6 Yes, But Is It *Inference*?

To be perfectly honest, or at least honest enough for practical purposes, all we have shown is that X-level forcing is a relation that preserves X-level. There is a gulf between this, and the assertion that $[\Vdash_X$ is an inference relation which preserves X-level. The obvious problem for anybody wishing to assert such a thing resides in the fact that we haven't, for all our efforts at precision, actually *said* which relations count as inference relations. What we *have* said, is that we assume that the inference relations we mention admit certain rules. Shall we take the collection of these rules to be constituitive of inference?

We shall not because some, at least, of the rules which the underlying logic admits, simply don't make sense for the derived relation. This should not come as a surprise. It is our palpable annoyance with the underlying logic which leads us to propose $[\Vdash_X$. How silly then to require that the derived logic inherit everything from the underlying logic, since that would make the derived logic another source of irritation rather than the balm for which we hope.

Although it is easy to check that $[\Vdash_X$ inherits from its underlying inference relation X both [R] and [Cut], we can see that it fails to admit the rule [M] of monotonicity, which we would do better to label the rule of *unrestricted* monotonicity from now on. But this is one of those cases in which the rule ought not to apply to the derived relation. If we are allowed to dilute premise sets in an arbitrary way, there is nothing to prevent us from raising the X-level of such sets. But raising the level gives us, in general, smaller cells in each logical cover. What used to X-follow from at least one cell of every such cover might no longer do so, as we are cut off from vital logical resources by the finer division.[12]

This is not to say that no form of monotonicity makes sense for the derived relation. Quite the contrary in fact, what most emphatically *does* make sense is that X-level forcing consequence must survive any dilution which preserves the level of the premise set. Such a restricted version of monotonicity manifestly is such a rule for X-level forcing as is trivial to verify.

Along with level-preserving dilution, there are certain consequences which must survive any dilution at all, whether or not the X-level of the premise set increases. These are the consequences which dilution cannot affect, and we can say precisely

[12]Here is a concrete example where the underlying logic is classical. The premise set $\{\alpha, \alpha \supset \beta\}$ has the classical-level forcing consequence β since it has level 1. If we add the formula $\neg\beta$ the resulting set has level 2, and now there is a logical cover: $\{\varnothing, \{\alpha, \neg\beta\}, \{\alpha \supset \beta\}\}$ no cell of which classically proves β. Thus β is not a classical-level forcing consequence of the diluted set.

which they are: the X-consequences of the empty set and of any unit set will remain X-consequences of at least one cell of every logical cover of any set which contains any of these privileged sets. In earlier work these sets were called *singular*.

The other properties which we have been mentioning for the underlying logics are non-triviality and having denial. It should be clear that when the underlying logic is non-trivial so will be its derived forcing relation. In fact, keeping to our original definition of consistency, in passing from an underlying logic X to its derived $[\Vdash_X$, many of the sets which are X-inconsistent fail to be $[\Vdash_X$-inconsistent, which is after all, the whole point of the derived relation.

Which brings us to denial. If the underlying logic has denial, nothing follows about the derived forcing relation, which is not necessarily a bad thing. This is because in the underlying logic, inconsistent sets are (typically) relatively easy to come by, but in the derived logic, the only inconsistent sets have inconsistent unit subsets, or what we called X-absurd formulas. Having denial doesn't imply having absurdities. So the derived logic will have denial only if the underlying logic has absurd formulas, but in no case will the derived logic have non-trivial denial.

And since β denies α in the derived logic if and only if one or both of the two are absurd in the underlying logic, the principle [Den] must hold of the derived relation but it is much less interesting there, than it is in the underlying logic.

So for the derived relation, we would seem to be on solid ground when we require [R], [Cut], and the restricted version of monotonicity. Those we might well regard as the hallmarks of inference, or at least of *derived* inference. And let us not forget that the derived relation agrees exactly with the underlying relation X on the consequences of the X-consistent sets. So for this reason alone, we ought to admit the forcing relation into the fold.

Perhaps we should put it this way: Anybody who thinks that X is fine and dandy except for its failure to be properly sensitive to the varieties of inconsistent sets, *must* think that X-level forcing is an adequate account of inference. This is because, when premise sets are X-consistent X-level forcing just *is* X. And while it surely isn't X for (some) X-inconsistent sets, in those cases X isn't an inference relation. X has abdicated, throwing up its hands and retiring from the inferential struggle offering the hopeful reasoner nothing beyond a contemptuous 'Whatever!'

3.7 Forcing In Comparison With Other Level-preserving Relations

Finally, we consider the place of the X-level forcing relation compared with other possible relations which preserve X-level. We cannot claim uniqueness here, for there may be plenty of relations, even inference relations which preserve X-level. What we can claim however, is inclusiveness, in a sense to be made precise.

That precision will require another property[13] of the underlying logic X.

Definition 7. *A logic X will be said to be* productival *if and only if for every finite set Γ there is some formula π such that*

$\pi \vdash_X \gamma$ *for every* $\gamma \in \Gamma$, *and*

$\Gamma \vdash_X \pi$

Evidently being productival is another of those properties more honored at the level of underlying logics. If a productival logic X has denial, then X-level forcing will certainly not be productival. But of course at the underlying level, products are useful. For instance:

Theorem 3. *If X is productival then for any pair Γ, α with Γ a finite set of formulas and α a formula:*
If Y preserves X-level and admits level-preserving monotonicity, then $\Gamma \vdash_Y \alpha \implies \Gamma[\Vdash_X \alpha$

[13] If we regard deductive systems as categories, then to call a logic productival is simply to say that the category (logic) X has products. The first condition amounts to the assertion of canonical projections while the second amounts to the universal mapping property of products.

Proof. We shall content ourselves with a sketch only—a fuller treatment can be found in "Level compactness" by Payette and D'Entremont, (to appear). Assume for indirect proof that $\Gamma \vdash_Y \alpha$ and that Γ fails to X-level force α. From the latter we know that Γ has finite level, say k, and that there is a logical cover of Γ of width k such that none of the k cells X-proves α. Where β (non-trivially) denies α add β to each cell and then form each of the k products of the cells. The set of these products must have X-level k but the Y closure of the set must have X-level k+1. So Y fails to preserve X-level contrary to hypothesis. □

The restriction to finite premise sets will chafe us only until we see its removal in the more general result referenced above.

So while there may be many inference relations which preserve X-level, X-level forcing is the largest of them.

Four

Level and Compactness

GILLMAN PAYETTE AND BLAINE D'ENTREMONT

Abstract

The concept of compactness is a necessary condition of any system that is going to call itself a finitary method of proof. However, it can also apply to predicates of sets of formulas in general and in that manner it can be used in relation to level functions, a flavor of measure functions. In what follows we will tie these concepts of measure and compactness together and expand some concepts which appear in Blaine d'Entremont's master's thesis: *Inference and Level*. We will also provide some applications of the concept of level to the "preservationist" program of paraconsistent logic. We apply the finite level compactness theorem in this paper to get a Lindenbaum flavor extension lemma and a maximal "forcibility" theorem. Each of these is based on an abstract deductive system X which satisfies minimal conditions of inference and has generalizations of 'and' and 'not' as logical words.

4.1 Introduction

The concept of compactness is a necessary condition of any system that is going to call itself a finitary method of proof. However, it can also apply to predicates of sets of formulas in general and in that guise it can be applied to level functions. Level functions are set functions akin to measure functions. In what follows we will tie measure and compactness together (via level) and expand some concepts which appear in Blaine d'Entremont's master's thesis: *Inference and Level*. We will also provide some applications of level to paraconsistent logic. One such inference relation in particular, forcing *à* la (**?**) and (Schotch and Jennings, 1980b).

4.2 Measure to Level

A measure is a function $\mu : \mathcal{B}(E) \longrightarrow \mathbb{R} \cup \{\infty\}$ defined on a σ-algebra $\mathcal{B}(E)$ over a set E. Where:

Definition 8. *A σ-algebra on E is a collection of subsets of E, $\mathcal{B}(E)$ such that:*

1. $\varnothing \in \mathcal{B}(E)$,
2. *Any countable union of elements of $\mathcal{B}(E)$ is an element of $\mathcal{B}(E)$ and,*
3. *The complement of any element of $\mathcal{B}(E)$ in E is an element of $\mathcal{B}(E)$.*

For any set A the power set $\mathcal{P}(A)$ is a σ-algebra.[1]

Measure functions are required to have the following properties:

1. $\mu(A) \geq 0$ for $A \in \mathcal{B}(E)$, with equality if $A = \emptyset$
2. $\mu(\bigcup_{i=0}^{\infty} A_i) = \sum_{i=0}^{\infty} \mu(A_i)$ for any sequence of disjoint sets $A_i \in \mathcal{B}(E)$. (**?**)

[1] In measure theory of the unit interval the σ-algebra is some proper subset of $\mathcal{P}([0,1])$. Using the axiom of choice one can prove that not all subsets of [0,1] are, in fact, measurable. However, our generalization will not suffer this problem, with or without choice.

The additive property above is a very strong condition and will not survive generalization to a notion that we shall call *level*. We will, however, maintain the convention that all sets will have at least level 0 and, the empty set will have level 0.

Like measure, level is represented by a function called, what else, a level function, indicated by some notation like: 'ℓ.' Level is also defined over a field of sets, but our inspiration comes not from point sets this time, but rather from *formula* sets. What we mean by a field of formula sets is the collection of all sets of formulas of some language $\mathcal{L}$, which we will call $\mathcal{P}(\mathbb{F})$–the power set of the set of formulas $\mathbb{F}$. Relative to certain systems of inference, there are properties which a given set of formulas might enjoy or not. Properties of that kind were used to define the concept of level in the first instance (see (Schotch and Jennings, 1980b)).

Such properties will serve us as useful examples in the present essay, but we shall take level functions to be defined more generally in terms of abstract properties of sets of formulas.

The original property of interest to the 'preservationist' program of paraconsistent logic was classical consistency, indicated by $\text{CON}_{\vdash}$(where $\vdash$ is classical provability). What the level function does in that case is measure the level of inconsistency of the set of sentences in a sense that will be made clear. But before we get to level, we must take a detour through the concept of a *cover*.

A Tale of Two Covers

We consider two concepts of cover; one–the more common one, topological, and one–of our own devising, specific to logic. The general topological conception is: a family of sets $\mathfrak{F}_\xi = \{\Delta_i : 1 \leq i \leq \xi\}$ such that for every $\beta \leq \xi$, $\mathbf{\Phi}(\Delta_\beta)$ and $\Gamma \subseteq \bigcup \mathfrak{F}_\xi$, where $\mathbf{\Phi}$ is some property of formula sets. The ξ subscript here is to say that $\mathfrak{F}$ is a family over the ordinal ξ. The $\mathfrak{F}_\xi$ is called a ($\mathbf{\Phi}$)-cover. If $\Gamma = \bigcup \mathfrak{F}$ and for each $\Delta_i, \Delta_j \in \mathfrak{F}$, $i \neq j$, $\Delta_i \cap \Delta_j = \varnothing$ then we say that $\mathfrak{F}$ partitions Γ.

The notion of a logical cover, on the other hand, is one of a family of sets, indexed as before, although perhaps with a distinguished element (the earliest typically) which appears in every such family.[2] Once again each element of the family must have some property $\mathbf{\Phi}$, but rather than the covered set being a subset of the union of the family, we require that the covered set be included in the union of the deductive closures (relative to some specified inference relation) of the elements of the cover.

We can bring the two notions into conformity by appeal to the topological notion of an *open set*. When the points on which the topology is constructed form a lattice, then what corresponds to the usual notion of openness is, ironically, virtually the same as deductive closure under the appropriate inference relation. With this in mind, a logical cover becomes a species of *open cover* which is the very meat and drink of topology.

We refer to the deductive closure of a set Γ relative to a logic X as $\mathbb{C}_X(\Gamma)$. The $\mathbf{\Phi}$ that is important in the context of logical cover will be the consistency predicate of the logic X, CON_X. Thus we can give our logic relevant definition of cover.

Definition 9. *A logical cover* $\mathfrak{F} = \{\Delta_0, ..., \Delta_\xi\}$ *of* Γ, *where* ξ *is as before, is such that:*

1. $\text{CON}_X(\Delta_i)$ *for each* $i \leq \xi$,
2. $\Delta_0 = \varnothing$ *and,*
3. $\Gamma \subseteq \bigcup_{i \leq \xi} \mathbb{C}_X(\Delta_i)$.

Thus, for each $\psi \in \Gamma$ there is some Δ_i such that, $\Delta_i \vdash_X \psi$. If the logic in question has the structural rule of inference [R], that is, if $\psi \in \Gamma$ then $\Gamma \vdash_X \psi$, then a partition of Γ may serve as a logical cover (this will be of use later).

The Road to Level

To define level we first define a predicate which holds between sets and ordinals. In relation to a property $\mathbf{\Phi}$ we define $\text{COV}_{\mathbf{\Phi}}(\Gamma, \xi)$

[2] In the work of Brown and Schotch (1999), the empty set serves as such an element.

if and only if there exists a $\mathfrak{F}$ which is a $\mathbf{\Phi}$-cover of Γ and $|\mathfrak{F} - \{\Delta_0\}| = \xi$. Each Δ_i is referred to as a 'cell.' The ξ is referred to as the 'width' of the cover.[3] Now we can define level.

Definition 10 (General Level Function). *A level function ℓ is an ordinal or $\{\infty\}$-valued function that "measures" the level of $\mathbf{\Phi}$-ness of a set of formulas Γ where $\mathbf{\Phi}$ is a predicate of sets of formulas. The function is defined as:*

$$\ell(\Gamma) = \begin{cases} min\{\xi \mid \text{COV}_{\mathbf{\Phi}}(\Gamma, \xi)\} & \textit{if it exists} \\ \infty & \textit{othewise} \end{cases}$$

The value of ℓ is the minimum value of the widths of the $\mathbf{\Phi}$-covers. The definition of the level function can be used with respect to either definition of cover. However, there is a rationale for choosing one definition of cover over the other. We want to keep the convention that the empty set has measure or rather, level 0. But, there are non-empty sets of measure 0 which have special properties and we would like to maintain that.

Suppose that Γ is a set of classical tautologies. Such sets as Γ are consistent but in a special way. Given any other classically consistent set Δ, we know that $\Delta \cup \Gamma$ is also consistent. A general consistent set does not have this property. It is easy to see that the level of $\Delta \cup \Gamma$ is just the level of Δ. If we use logical covers, for any $\Gamma \subseteq \mathbb{C}_X(\varnothing)$, $\mathfrak{F} = \{\varnothing\}$ (the cover consisting of just the empty set) is a cover of Γ and $|\mathfrak{F} - \{\varnothing\}| = 0$. So sets of X-tautologies and the empty set have level 0. The definition of logical cover allows non-empty sets of "measure"(i.e. level) 0! The topological cover, on the other hand, only allows $\varnothing$ to have level 0 since the union of the cells must contain the set being covered.

4.3 Level Functions

The restrictions which we impose on $\mathbf{\Phi}$ are applicable to either definition of cover. We require the property $\mathbf{\Phi}$ to be two things:

[3]However, ξ can be transfinite but, in that case $\xi - 1 = \xi$; which is what we want.

1. $\mathbf{\Phi}$ must be downward monotonic, i.e. if $\Gamma \subseteq \Gamma'$ and $\mathbf{\Phi}(\Gamma')$ then $\mathbf{\Phi}(\Gamma)$ and,
2. The extension of $\mathbf{\Phi}$ must be non-empty.

The second requirement is trivial, but the first is to ensure the downward monotonicity of the level function relative to $\mathbf{\Phi}$.

Proposition 4. *If $\mathbf{\Phi}$ is downward monotonic then ℓ is monotonic. I.e. if $\Gamma \subseteq \Gamma'$ then $\ell(\Gamma) \leq \ell(\Gamma')$.*

Proof. Suppose there were a subset with a larger level than the whole set. If there were a cover of the whole set of size, say ξ then there would be a cover of the subset of size ξ by the downward monotonicity of $\mathbf{\Phi}$, which contradicts the assumption that the subset has a larger level. □

We shall assume in the sequel that $\mathbf{\Phi}$ is a downward monotonic property of sets of formulas; so level functions are monotonic.

What are the further properties of measure functions that should be preserved? Level functions are defined over the σ-algebra of the power set of formulas. The empty set has level 0 and there are non-empty sets of level 0 in certain contexts. Next we would like to see if level functions are countably additive. Level is not countably additive in general. One can see that it is not countably additive by considering the case of classical consistency. Assume the set $\Gamma = \{\psi, \varphi\}$ is consistent; but, neither formula is a tautology and they are not equivalent. Then the unit sets $\{\varphi\}$ and $\{\psi\}$ are both subsets of Γ and disjoint. These sets have level 1 since they can be covered by $\{\varnothing, \{\psi\}\}$ and $\{\varnothing, \{\varphi\}\}$ respectively. But, neither by $\{\varnothing\}$ alone. Then each subset of Γ is also consistent. But, $1 = \ell(\Gamma) = \ell(\{\varphi\} \cup \{\psi\}) \neq \ell(\{\varphi\}) + \ell(\{\psi\}) = 2$.

The next phase is to prove a compactness theorem for level. First we must consider what compactness means in the context of level. In terms of consistency, compactness means: Γ is consistent if and only if every finite subset of Γ is consistent. Thinking of level as a generalization of consistency, we say the level of the whole set is less than or equal to a certain number if and only if each finite subset's level is also less than or equal to that number. However, as we shall show, we can only make sense of this when two things obtain: the level is finite and $\mathbf{\Phi}$ is compact.

Let us first distinguish the case where $\ell(\Gamma) = \omega$ and $\ell(\Gamma) = \infty$. To say $\ell(\Gamma) = \omega$ means the widths of the "smallest" possible ($\mathbf{\Phi}$)-covers of Γ is ω. This does not mean that $\ell(\Gamma) = \infty$ that is reserved certain types of sets, e.g. self-inconsistent ones like $\{\psi \wedge \neg\psi\}$ if $\mathbf{\Phi}$ is classical consistency. In the general case $\ell(\Gamma) = \infty$ means there are no $\mathbf{\Phi}$-covers of Γ. There being no $\mathbf{\Phi}$-covers just means that at least one of the unit subsets is not a $\mathbf{\Phi}$ set.

Classical logic provides an example of a set with level ω. Consider $\left\{(\bigwedge_{i<n} \neg P_i) \wedge P_n \,|n \in \omega\right\}$. This set has ω unit sets, each inconsistent with any other unit set, but each unit set is consistent. Thus, the minimal width of a cover is ω.

Now we can explain why the level must be finite. Suppose we want 'level compactness' to mean: if $\ell(\Gamma) = \xi$ then there is a finite subset of Γ with level ξ. If this is our intent we will have a problem if the set in question has level ω. Any finite set, which does not have level ∞, must have a finite level; since all of the unit sets are $\mathbf{\Phi}$-sets the largest its level could be is the cardinality of the set. Thus, if a set Γ has level ω no finite subset of Γ can have level ω. Therefore, our attempt at a description of level compactness at the beginning of the paragraph cannot succeed for infinite levels (and that is ok).

The reason we want level compactness to be characterized by: if $\ell(\Gamma) = n$ then there is a finite subset of Γ with level n, is because we want it to be equivalent to: $\ell(\Gamma) \leq n \iff \forall \Gamma' \subseteq \Gamma$ that are finite, $\ell(\Gamma') \leq n$ for all $n \in \omega$. This can only occur if the level is finite (as we saw above) and when $\mathbf{\Phi}$ is compact. Thus we assert: 1) $\mathbf{\Phi}$ must be compact and 2) we must restrict our application of level compactness to the finite values of the level function. So the level function is only compact on a subset of $\mathcal{P}(\mathbb{F})$.

Using finite levels we can make the following assumption about level.

Proposition 5. *If $m \in \omega$, $\ell(\Sigma) = m < \ell(\Sigma \cup \Gamma)$, and $\mathbf{\Phi}(\Gamma)$ then $\ell(\Gamma \cup \Sigma) = m + 1$.*

Proof. If $m = \ell(\Sigma) < \ell(\Sigma \cup \Gamma)$, and $\mathbf{\Phi}(\Gamma)$ then let $\mathfrak{F}_m = [\Delta_0, \Delta_1, ..., \Delta_m]$ be a minimal cover for Σ, then $\mathfrak{F}'_m = [\Delta_0, \Delta_1, ..., \Delta_m, \Gamma]$ is a minimal cover of $\Sigma \cup \Gamma$, of size $m + 1$. We cannot distribute Γ over the other cells of any min cover because, if we could, $\ell(\Sigma) < \ell(\Sigma \cup \Gamma)$ would be false. Hence $\ell(\Sigma \cup \Gamma) = m + 1$. □

Thus, one may assume, without loss of generality, that the set being added is consistent because if the level goes up at all, shy of ∞, it must go up by at least 1 and, there will always be a $\mathbf{\Phi}$-set contained in the set being added. Not only does the above proposition hold but, if Γ is a 0 set and Π has some finite level then, $\Gamma \cup \Pi$ will have the level of Π. This is left as an exercise. Now we will show our restrictions to be worthwhile since with them the before mentioned descriptions of level compactness are equivalent.

4.4 Level Compactness

Theorem 4 ((?) Theorem 10). *The following are equivalent when $\ell(\Gamma) < \omega$ and $\mathbf{\Phi}$ is compact:*

1. *$\ell(\Gamma) \leq n \iff \forall \Gamma' \subseteq \Gamma$ that are finite, $\ell(\Gamma') \leq n$.*
2. *If $\ell(\Gamma) = n$ then $\exists \Gamma' \subseteq \Gamma$ which is finite, such that $\ell(\Gamma') = n$.*
3. *If there is a finite subset Γ^* of Γ such that for any other finite $\Gamma' \subseteq \Gamma$, $\ell(\Gamma') \leq \ell(\Gamma^*)$ then $\ell(\Gamma^*) = \ell(\Gamma)$.*

Proof. We proceed by showing the equivalence in a triangle. All n, m, k etc. are elements of ω.
$1 \Rightarrow 2$ Assume 1 and assume for reductio that $\ell(\Gamma) = n$ and that there is no finite subset of Γ which has level n. We know by monotonicity of level that all of the subsets of Γ must have level less than that of Γ, so there is an upper bound. This upper bound will also apply to finite sets. Call this upper bound m. This m

is strictly less than n because otherwise there would be a finite subset of level n which there isn't. With 1 we get $\ell(\Gamma) \leq m < n$, which is a contradiction.
$2 \Rightarrow 3$ Assume 2 and the existence of a Γ^* as in the antecedent of 3. There must be, by 2, a finite $\Gamma' \subseteq \Gamma$ with $\ell(\Gamma') = \ell(\Gamma)$ but then, $\ell(\Gamma) = \ell(\Gamma') \leq \ell(\Gamma^*)$. By monotonicity of ℓ we get $\ell(\Gamma^*) \leq \ell(\Gamma)$ hence, $\ell(\Gamma^*) = \ell(\Gamma)$.
$3 \Rightarrow 1$ Assume 3. The only if direction of 1 follows from monotonicity of ℓ thus, assume that for every finite $\Gamma' \subseteq \Gamma$ $\ell(\Gamma') \leq n$. Let
$m = max\{k|\ell(\Gamma') = k \ \& \ \Gamma' \subseteq \Gamma \ finite\}$ This must exist since there is an upper bound, viz. n. Let $\Gamma^* = \Gamma'$ such that $\ell(\Gamma') = m$. Then we have satisfied the conditions for 3 thus, $\ell(\Gamma^*) = \ell(\Gamma) = m \leq n$. □

Using these equivalences we can actually prove that, for finite level, the level function is compact in the way mentioned as equivalence 1) in the theorem above. In the case of logical covers we will make the assumption that the consistency of Γ means that $\mathbb{C}_X(\Gamma) \neq \mathbb{F}$. The compactness theorem holds for either notion of cover we use; topological or logical.[4]

Theorem 5 (d'Entremont Theorem 9 (?) Finite Level Compactness). *If Γ is a set of formulas with $\ell(\Gamma) < \omega$ and $\mathbf{\Phi}$ is compact then: $\ell(\Gamma) \leq n$ if and only if for every finite subset Γ' of Γ, $\ell(\Gamma') \leq n$.*

Proof. Theorem 1 says that: the following are equivalent for all $n \in \omega$. (We will use these at various stages.)

1. $\ell(\Gamma) \leq n \iff \forall \Gamma' \subseteq \Gamma$ that are finite, $\ell(\Gamma') \leq n$.

2. If $\ell(\Gamma) = n$ then $\exists \Gamma' \subseteq \Gamma$ which is finite, such that $\ell(\Gamma') = n$.

3. When $\ell(\Gamma) = n$; if there is a finite subset Γ^* of Γ such that for any other finite $\Gamma' \subseteq \Gamma$, $\ell(\Gamma') \leq \ell(\Gamma^*)$ then $\ell(\Gamma^*) = \ell(\Gamma)$.

[4]However, we provide the proof for the logical version since it is inclusive of the topological version. The only difference is in the basis step of the induction.

We will proceed by induction on the level of Γ. For $\ell(\Gamma) = 0$ then we know that $\Gamma = \varnothing$ or $\Gamma \subset \mathbb{C}_{\mathbf{\Phi}}(\varnothing)$,[5] so $\varnothing \subseteq \Gamma$ and is finite with $\ell(\varnothing) = 0$, and by the monotonicity of $\mathbf{\Phi}$ any nonempty subset of a nonempty Γ will have level 0. Thus we get the result for the basis step by showing 2. Assume that $\ell(\Gamma) \leq k$ if and only if, For every finite subset Γ' of Γ, $\ell(\Gamma') \leq k$ for $k \leq n$. Assume that $\ell(\Gamma) = n + 1$. We will show that 3 holds to get the result for this stage. Assume as in the antecedent of 3 that there is a finite $\Gamma^* \subseteq \Gamma$ and for any finite $\Gamma' \subseteq \Gamma$, $\ell(\Gamma') \leq \ell(\Gamma^*)$. Clearly $\ell(\Gamma^*) \leq \ell(\Gamma)$ If $\ell(\Gamma^*) = \ell(\Gamma)$ then we are done. So assume $\ell(\Gamma^*) < \ell(\Gamma) = n + 1$. Thus, $\ell(\Gamma^*) \leq n$. This means that every finite subset of Γ has level $\leq n$, but by inductive hypothesis, and using 1 we get that $\ell(\Gamma) \leq n < n + 1 = \ell(\Gamma)$. But that is a contradiction. Thence, $\ell(\Gamma^*) = \ell(\Gamma)$ Which is what we wanted. So we have shown 3 for $\ell(\Gamma) = n + 1$ which is equivalent to $\ell(\Gamma) \leq n + 1$ if and only if, For every finite subset Γ' of Γ, $\ell(\Gamma') \leq n + 1$. Therefore for all $n \in \omega$, $\ell(\Gamma) \leq n$ if and only if, For every finite subset Γ' of Γ, $\ell(\Gamma') \leq n$. □

Thus, the compactness of the predicate $\mathbf{\Phi}$ will carry over to make the level function defined on it compact. Thus, we can look at some applications of the finite level compactness theorem.

4.5 Applications

These concepts were developed in the context of logic so of course they find their application there. So, from here on let X be some logic over a countable language $\mathcal{L}$. Then we have the logic's syntactic consistency predicate and inference relation as was defined earlier. We take the extensional meaning of 'logic' so that a logic is the set of pairs $\langle \Gamma, \psi \rangle$ such that $\Gamma \vdash_X \psi$. Taking the syntactic consistency predicate of X as $\mathbf{\Phi}$ we can force $\mathbf{\Phi}$ to be compact since it is common to consider proofs finite. Concerning X we

[5] This assertion of a $\mathbf{\Phi}$-closure is for the case where we use logical covers and $\mathbf{\Phi}$ is CON_X, to consider the topological covers we restrict the basis case to $\varnothing$ and just use $\mathbf{\Phi}$ and no closure operator.

will allow $\vdash_X$ to be reflexive, monotonic and transitive that is, allow the rules [R], [M] and [T] $\grave{a}$ la (Scott, 1974). As a corollary to the compactness theorem we can get an extension lemma. For this we need a definition.

Definition 11. Γ^+ *is a maximal level preserving extension (*MLPE*) of* Γ *if and only if;*

1. $\Gamma \subseteq \Gamma^+$
2. $\ell(\Gamma^+) = \ell(\Gamma)$ *and,*
3. *for any formula* ψ*, if* $\ell(\Gamma^+ \cup \{\psi\}) = \ell(\Gamma^+)$ *then* $\psi \in \Gamma^+$*.*

The "absolute" definition, one not relative to any Γ, just satisfies the third clause. Such sets are maximal with respect to level, i.e. you can't add anything to them without raising the level. As one would expect we can produce one of these Γ^+ sets by the following construction. Recall that the language that we are using is countable.

Lemma 1 (The Lindenbaum Level Lemma). *Let* Γ *have level* $m \in \omega$ *then it can be extended to a* Γ^+*.*[6]

Proof. Let Γ have level $m \in \omega$. Then Let $\psi_0, \psi_1, ..., \psi_k, ...$ such that $k \in \omega$ be an enumeration of the formulas of the language. Form sets Σ_n for $n \in \omega$ by:

$$\Sigma_0 = \Gamma$$

$$\Sigma_n = \begin{cases} \Sigma_{n-1} \cup \{\psi_n\} & \text{if } \ell(\Sigma_{n-1} \cup \{\psi_n\}) = \ell(\Gamma) \\ \Sigma_{n-1} & \text{othewise} \end{cases}$$

Let $\Gamma^+ = \bigcup_{n \in \omega} \Sigma_n$. Claim: this set is a level preserving maximal extension of Γ. By the recursive construction and compactness $\ell(\Gamma^+) = \ell(\Gamma)$. There are three cases

1. $\ell(\Gamma^+) = \infty$

[6] *Notice that if the level of* Γ *is* ω *then the extensions will be everything except those formulas which have level* ∞*.*

2. $\ell(\Gamma^+) \geq \omega$

3. $\omega > \ell(\Gamma^+) > \ell(\Gamma)$

If 1. then a formula was added which cannot be covered but that is impossible. If 2. then for any $n \in \omega$ there is a finite subset of Γ^+ of level n. If there was a finite upper bound on the levels of the finite subsets of Γ^+ then the compactness theorem says the whole set would have that finite level, which is contrary to assumption. So choose $n > \ell(\Gamma)$, the finite subset of level n will be contained in some Σ_k so $\ell(\Sigma_k) > \ell(\Gamma)$ which is impossible. Finally, if 3. there was a finite set which was the culprit, and it would be contained in some Σ_n, which is also impossible. Thus $\ell(\Gamma^+) = \ell(\Gamma)$.

Lastly, suppose $\ell(\Gamma^+ \cup \{\varphi\}) = \ell(\Gamma^+)$, then either a) $\varphi \in \Gamma$ or, if not, b) $\varphi = \psi_n$ for some $n \in \omega$. If a) then $\varphi \in \Gamma^+$ *a fortiori*. If b) then φ was considered for membership at stage n, and since adding it to Γ^+ does not change its level, adding φ to Σ_{n-1} does not change the level of Σ_{n-1} since $\ell(\Sigma_n) = \ell(\Gamma) = \ell(\Gamma^+)$. Thus, φ was added at stage n. Therefore, $\varphi \in \Gamma^+$. □

So we can always extend sets with finite level to an MLPE .

To make level applicable to inference we consider the general concept of "Forcing." Given a logic X we can define a forcing relation ($[\Vdash_X$) on the provability predicate $\vdash_X$. We do this using the definition of logical cover and level where $\mathbf{\Phi}$ is CON_X.

Definition 12. *Γ X-Forces ψ, $\Gamma[\Vdash_X \psi$ if and only if every logical cover of Γ of width, $\ell_X(\Gamma)$ contains a cell, $\Delta_i \in \mathfrak{F}$, such that $\Delta_i \vdash_X \psi$.*

So we can rephrase this to say: Γ X-Forces ψ iff all the 'narrowest' covers contain a cell which X-proves ψ. We will abbreviate the class of narrow covers of Γ as $NAR(\Gamma)$. The level function ℓ_X defined with respect to CON_X is implicit.

An important feature of forcing is that it is an inference relation which preserves X-level. The following result proves this.

Lemma 2. $\ell_X(\mathbb{C}_{[\Vdash_X}(\Gamma)) = \ell_X(\Gamma)$ *That is, the X-Forcing closure of Γ has the same level as Γ. From here on we will omit the subscripts in the proofs, since we take it to be understood which X we are working with.*

Proof. Assume $\ell(\Gamma) = n$ finite. So then $\psi \in \mathbb{C}_{[\Vdash}(\Gamma)$ iff $\Gamma[\Vdash \psi$. Let $\mathfrak{F} \in NAR(\Gamma)$. Then, by definition of forcing, for each $\psi \in \mathbb{C}_{[\Vdash}(\Gamma)$ there is $\Delta_i \in \mathfrak{F}$ such that $\Delta_i \vdash \psi$. Thus, $\mathfrak{F}$ is a cover of $\mathbb{C}_{[\Vdash}(\Gamma)$. Further, it is easy to see that $[\Vdash$ has [R], so $\Gamma \subseteq \mathbb{C}_{[\Vdash}(\Gamma)$. So by monotonicity of level $\ell(\Gamma) \leq \ell(\mathbb{C}_{[\Vdash}(\Gamma))$ but, $\mathfrak{F}$ is a cover of $\mathbb{C}_{[\Vdash}(\Gamma)$ of width $\ell(\Gamma)$ therefore, $\ell_X(\mathbb{C}_{[\Vdash_X}(\Gamma)) = \ell_X(\Gamma)$. □

As a corollary to this lemma we have:

Corollary 2. *If* $\Gamma[\Vdash_X \psi$ *then* $\ell_X(\Gamma \cup \{\psi\}) = \ell_X(\Gamma)$.

One can see in this context that level is a generalization of consistency. The first two levels, 0 and 1, use CON_X but 'after' that; to say that a set Γ is inconsistent with another set Π is to say that $\ell(\Gamma) < \ell(\Gamma \cup \Pi)$. To say that a set is universally inconsistent is to say that the set has level ∞. And forcing is a relation which preserves this new type of consistency.

Our goal is to derive something like a maximal provability theorem like we have for classical logic. We want a set Γ forces a formula if and only if every 'maximal extension' of Γ forces the formula. The maximal extensions in our case are the MLPE s. One can see from the corollary above that MLPE s only force those formulas which they contain; so they are clearly a generalization of classical maximally consistent sets. However, to do this we need to impose a bit of structure on the logic in question. We must consider a certain class of logics with some special properties.

Recall the discussion of self inconsistent formulas or unit sets with level ∞. These 'absurd' formulae make an appearance in our next definition.

Definition 13 (Denial). *A logic X has Non-Trivial Denial if and only if for each non-absurd formula ψ there is another non-absurd formula ψ'— such that $\overline{\text{CON}_X}(\{\psi, \psi'\})$, where the overline indicates predicate negation.*

The two formulas in this definition are said to deny each other. Without the restriction to non-absurd formulas, the above defines what it is for a logic X to have denial *simplicater*.

We require that denial commutes in the right way with consistency and $\vdash_X$. This is to say that we impose the condition::

$$\text{[Den] } \Gamma \vdash_X \psi \iff \overline{\text{CON}_X}(\{\Gamma, \psi'\}) \text{ where } \psi' \text{ denies } \psi.$$

There does not have to be a unique denial nor is denial necessarily functional in the way that classical negation is. However, denial must be symmetric, so ψ denies φ if and only if φ denies ψ. We need a much stronger property to get to our final destination. This property is shared by many logics like intuitionistic logic, quantum logic, et cetera. We call logics with this property 'productival'.

Definition 14. *A logic X is productival iff, given any finite set Γ there is a formula φ such that $\varphi \vdash_X \psi$ for each $\psi \in \Gamma$ and, $\Gamma \vdash_X \varphi$.*

We can now use our extension lemma to prove a Maximal Forcibility Theorem for the class of logics which satisfy the two properties above.

Theorem 6 (Maximal Forcibility Theorem). *Let X be a productival logic, which has non-trivial denial and $\ell(\Gamma) \in \omega$. Then, $\Gamma [\!\Vdash_X \psi$ if and only if, for each* MLPE Γ^+ *of Γ, $\Gamma^+ [\!\Vdash_X \psi$.*

Proof. (ONLY IF) Suppose $\Gamma [\!\Vdash_X \psi$. Then let Γ^+ be an MLPE of Γ. So, $\ell(\Gamma) = \ell(\Gamma^+)$ by definition. Suppose that $\mathfrak{F} \in NAR(\Gamma^+)$ then $\mathfrak{F}$ is also a cover of Γ because $\Gamma \subseteq \Gamma^+$. But, the width of $\mathfrak{F}$ is also the level of Γ so, $\mathfrak{F} \in NAR(\Gamma)$. Thus by definition of Forcing there must be $\Delta_i \in \mathfrak{F}$ such that $\Delta_i \vdash \psi$. Since $\mathfrak{F}$ was arbitrary $\Gamma^+ [\!\Vdash \psi$.

(IF) By contrapositive. Assume $\Gamma [\!\nVdash \psi$ and $\ell(\Gamma) = m$. Then, there is an $\mathfrak{F} \in NAR(\Gamma)$ which is a partition of Γ where no $\Delta_i \in \mathfrak{F}$ is such that $\Delta_i \vdash \psi$. Thus, by [Den] $\text{CON}(\Delta_i \cup \{\psi'\})$ for each i, ψ' a denial of ψ. For each $\Delta_i \neq \Delta_j$, $\Delta_i \cap \Delta_j = \varnothing$. And $\overline{\text{CON}}(\Delta_i \cup \Delta_j)$. By compactness of CON we have for each pair of cells finite sets Δ_i' and Δ_j', contained in Δ_i, Δ_j respectively, which are inconsistent with one another. Thus we form their respective products and get φ_{ij} and φ_{ji}. So we have $\overline{\text{CON}}(\{\varphi_{ij}\} \cup \{\varphi_{ji}\})$. Since there are only finitely many of these φ_{ij}'s for each i we can get a product for each i, call it φ_i, such that $\varphi_i \vdash \varphi_{ij}$ for each $j \neq i$. The φ_i's are clearly consistent with the Δ_i's and any two distinct φ_i's are inconsistent. Since $\text{CON}(\Delta_i \cup \{\psi'\})$ we can form the product of $\{\varphi_i, \psi'\}$ to get φ_i^* for each i, which will also be consistent with each Δ_i. Form the cover:

$$\mathfrak{F}' = \{\varnothing, \Delta_i \cup \{\varphi_i^*\} : \ \Delta_i \in \mathfrak{F}, \ \& \ 1 \leq i \leq m\}$$

$\mathfrak{F}'$ is a cover of $\Gamma \cup \{\psi'\}$ of width m. By monotonicity of level we get that $\ell(\Gamma^*) = \ell(\Gamma \cup \{\varphi_i^* : 1 \leq i \leq m\}) = \ell(\Gamma)$. Extend this new set Γ^* to a Γ^+ as in the extension lemma. This Γ^+ will have level m and since each φ_i^* proves ψ' and must be contained in a different cell, ψ cannot be added to Γ^+ without increasing its level, which is to say that $\Gamma^+ [\!\!\not\Vdash \psi$ with $\Gamma \subseteq \Gamma^+$. □

Where to go from here? It would be nice to know where the X-forcing relation fits with respect to other inference relations that preserve X-level.

As we mentioned above all of the logics X that we consider have [R], [T] and [M]. However, forcing does not have [M] but a variation of it. It is well known in the literature[7] that classical forcing is monotonic in a restricted sense. The 'restricted sense' is: if $\Gamma [\!\Vdash \varphi$, and $\ell(\Gamma \cup \Delta) = \ell(\Gamma)$ then $\Gamma \cup \Delta [\!\Vdash \varphi$. Thus, if a set can be added without changing the level then the forcing consequences are preserved by the union. We will call this restricted sense of monotonicity [LM]. (Level-Monotonicity, and relative to X, X-[LM].) The generalization from classical-[LM] to X-[LM] follows.

Proposition 6. *If* $\Gamma [\!\Vdash_X \varphi$, Γ *has finite level and* $\ell_X(\Gamma) = \ell_X(\Gamma \cup \Delta)$ *then* $\Gamma \cup \Delta [\!\Vdash_X \varphi$.

Proof. This is a corollary to the first direction in theorem 6 since any level preserving extension will be contained in some MLPE . So, *a fortiori* $\Gamma \cup \Delta [\!\Vdash \varphi$. □

[7]See **?**, p. 312 and Brown and Schotch (1999, p. 275).

Since we wanted the 'base' logics like X to have [R], [T], and [M] we will demand that inference relations which preserve X-level must have [R], [T] and X-[LM]. These restrictions give enough information to see where X-forcing fits into the picture.[8]

Theorem 7 (Level Preservation). *If X is a logic as in theorem 6 and Y is another logic which preserves X-level, obeys [R], [T] and is monotonic with respect to X-level preserving extensions then, for all Γ with finite level, $\mathbb{C}_Y(\Gamma) \subseteq \mathbb{C}_{[\Vdash_X}(\Gamma)$. Thus, X-forcing is the largest relation which preserves X-level and has X-[LM].*

This is not always the case when X is not as in theorem 6. The biggest relation which is like forcing is defined by:

$$\Gamma \vdash_{\text{MLPE}} \varphi \Leftrightarrow \varphi \in \bigcap_{\Gamma^+ \in \text{MLPE}} \Gamma^+$$

where MLPE stands for the class of maximal level preserving extensions of Γ. We abbreviate the right hand side as $\bigcap \Gamma^+$.

Proposition 7. *Given a logic X and a relation Y like the relation Y in theorem 7, then for all Γ $\mathbb{C}_Y(\Gamma) \subseteq \bigcap \Gamma^+$.*

Proof. Assume for reductio that $\varphi \in \mathbb{C}_Y(\Gamma)$ but $\varphi \notin \bigcap \Gamma^+$. Then there is an MLPE of Γ, call it Γ^+_φ such that $\varphi \notin \Gamma^+_\varphi$. By definition $\ell(\Gamma^+_\varphi \cup \{\varphi\}) > \ell(\Gamma^+_\varphi) = \ell(\Gamma)$. And since $\Gamma \subseteq \Gamma^+_\varphi$ we have by hypothesis on Y, $\mathbb{C}_Y(\Gamma) \subseteq \mathbb{C}_Y(\Gamma^+_\varphi)$. Thus $\varphi \in \mathbb{C}_Y(\Gamma^+_\varphi)$ and so $\ell(\mathbb{C}_Y(\Gamma^+_\varphi)) > \ell(\Gamma^+_\varphi)$. Therefore Y does not preserve X-level contrary to hypothesis. □

Finally we have

Proof of Theorem 7. Given a logic X, and Y is as required, we Know that $\mathbb{C}_Y(\Gamma) \subseteq \bigcap \Gamma^+$. But theorem 6 says that for productival logics $\mathbb{C}_{[\Vdash_X}(\Gamma) = \bigcap \Gamma^+$. Hence, by proposition 7, we get $\mathbb{C}_Y(\Gamma) \subseteq \mathbb{C}_{[\Vdash_X}(\Gamma)$. □

[8]Forcing fits in the picture in the following way for finite level. For infinite levels it will not be the largest since the language is countable, since $\bigcap \Gamma^+$ will contain all formulas except the absurd ones. The forcing closure, on the other hand, will collapse into what follows from the unit sets of Γ. That is $\mathbb{C}_{[\Vdash_X}(\Gamma) = \bigcup_{\gamma \in \Gamma} \mathbb{C}_X(\{\gamma\})$.

Five

Forcing and Practical Inference

PETER SCHOTCH

Abstract

In this essay we consider two variations on the theme of forcing. In one of these we restrict the number of possible logical covers of a given premise set by specifying certain subsets, which we call *clumps* and requiring that any cell which contains a member of a clump, must also contain the other members. The other variation allows for redefining the notion of logical cover in order to require that the cells not only be *logically* consistent but also *practically* consistent. In other words in this mode we require of any logical cover that it recognize our theoretical (and perhaps also practical) commitments, by defining the cells to be those subsets (if any) which do not prove (in the underlying logic) any denial of those commitments.

5.1 Introduction

What we might call the *pure* or perhaps *general* theory of the forcing relation is well enough in a theoretical setting. That shouldn't come as much of a surprise since our treatment of forcing tends to be abstract and general. On the other hand, sometimes we want to study inference in something more closely resembling ordinary life, more ordinary than mathematical life at least. In these cases the general theory is too spare. In fact we shall distinguish two ways in which one might wish to come down from the Olympian heights of generality to no greater altitude than a foothill might boast.

First a sketch of the general theory:[1]

We consider logics X which satisfy the usual structural rules:

[R] $\alpha \in \Gamma \implies \Gamma \vdash_X \alpha$

[Cut] $\Gamma, \alpha \vdash_X \beta \,\&\, \Gamma \vdash_X \alpha \implies \Gamma \vdash_X \beta$

[Mon] $\Gamma \vdash_X \alpha \implies \Gamma \cup \Delta \vdash_X \alpha$

For these logics, we understand the notion of consistency in the manner of Post.

> Γ is consistent, *in* or *relative to* a logic X (alternatively, Γ is X-consistent) if and only if there is at least one formula α such that $\Gamma \nvdash_X \alpha$.
>
> Where X is a logic, the associated consistency predicate (of sets of formulas) for X, is indicated by CON_X.

Having a notion of consistency allows us to define a certain relation between pairs of formulas (of the underlying language of the logic X—after this we won't bother to keep mumbling that particular mantra) which we term *non-trivial denial*.

> X has *non-trivial* denial if and only if for every non-absurd formula α there is at least one non-absurd formula β which is not X-consistent with α. Two formulas which are related in this way are said to deny each other. We also say each is *a denial* of the other.

Given a set of formulas Γ we say that an indexed family of sets (starting with $\varnothing$), is a *logical cover* of Γ provided:

[1] The definitive account is presented in the essay 'On Preserving' in this volume.

For every element a of the indexed family, $\text{CON}_X(a)$ and

$\Sigma \subseteq \bigcup_{i \in I} \mathbb{C}_X(a_i)$

where $\mathbb{C}_X(a_i)$ is the (X) logical closure of a_i which is to say $\{\beta | a_i \vdash_X \beta\}$. When $\mathfrak{F}$ is an (X) logical cover for Γ we write $\text{COV}_X(\mathfrak{F}, \Gamma)$. We shall refer to the size of the index set of the indexed family in this definition as the *width* of the logical cover, indicated by $w(\mathfrak{F})$.

And now for the central idea:

The *level* (relative to the logic X) of the set Γ of formulas, indicated by $\ell^X(\Gamma)$ is defined:

$$\ell^X(\Gamma) = \begin{cases} \min\limits_{w(\mathfrak{F})}[\text{COV}_X(\mathfrak{F}, \Gamma)] & \text{if this limit exists} \\ \infty \quad \text{otherwise} \end{cases}$$

In other words: the X-level (of incoherence or inconsistency) of a set Σ in a logic X is the width of the narrowest X-logical cover of Σ, if there is such a thing, and if there isn't, the level is set to the symbol ∞.

Finally we define the relation of X-level forcing which, as its name suggests, is derived from some underlying logic X. The relation is defined:

$\Gamma [\Vdash_X \alpha$ if and only if, for every division of Γ into $\ell^X(\Gamma)$ cells, for at least one of the cells $\Delta, \Delta \vdash_X \alpha$

5.2 Dirty Hands

Now this is all very well in general, at that most abstract plane of logical existence on which epistemology intrudes not at all upon how we reason. However this is seldom a good representation of real inferential life. When we face the world of applications, there may well be non-logical considerations to weigh in the balance.

In real life, we have commitments. Some we clasp to our bosoms after striving to acquire them, while others fall upon us unbidden. At this point, some of our opponents may become existentialist enough to declare that they themselves renounce all commitments and refuse to be bound by anything but their own mighty wills. Such as these have put themselves not only beyond polite society, but also beyond science.

If the mission statement of science includes the aim of framing bold hypotheses and then suffering these to be tested, science involves commitments. We see this directly we begin to think about how hypothesis testing works. It is clear that, whether we construct a new experiment, or merely use data previously gathered, we have to assume that the laws of science, except of course for one that we may be testing, hold. It simply makes no sense to talk about testing all the laws of science at once. Testing them against what? We are committed, in this endeavor to the correctness of whatever bits of science are not being tested.

This is merely one example, though it is a particularly compelling one. But what does it mean to say that we are committed to this or that principle? This much at least: for the duration of our commitment, we shall brook no denial of the principle in question. In other words, we shall treat any such denial as a sort of practical contradiction.[2]

Apart from worrying about commitments there are clearly cases when merely having consistent cells is not enough even if we expand our horizons to include *practical consistency*. Cases that is, in which we want to insist on the integrity of certain of our data. We may be prepared to carve up our premise set into subsets but we wish to place limits upon how much carving can be done.

[2]Such was a popular device in Buddhist logic where a paradigm of absurdity was the sentence 'Lotus in the air' which we might take as an Eastern version of 'Pie in the sky.'

5.3 Σ-Forcing

Whatever else we may mean by commitment, immunity to argument, must be high on the list. There may come a day in which an argument is so convincing that it releases us from this or that of our commitments, but when that happens, it goes without saying we are no longer committed to whatever it was that previously claimed our allegiance. So we aren't saying that commitment is forever, even commitment worthy of the name. Commitments come and go—an ever shifting pattern as we fail to renew our subscription to one theory and subscribe instead to another.

While our commitment is in force however, while we are still bound by it. In any conflict with logic, it is logic that must go to the wall. This is the grand picture at any rate, and like all such, the devil is in the details. One thing to give us pause here is the recognition that our logical commitments are in no way inferior to our other commitments[3] so that in saying that our genuine commitments are immune from argument, we must be careful not to suggest the contrary. What we are saying is more along the lines of *our* logic must take account of our commitments, whether or not *your* logic makes any such provision.

Imagine a case in which there is a physical theory which provides us with plenty of well-confirmed predictions as well as, unfortunately, other predictions which are simply absurd, in practical terms at least. Lets say, for instance, that the theory predicts that some physical constant e has an infinite value. What shall we do? A philosopher will immediately say that we have disconfirmed the theory and that we must get rid of it at once. A natural scientist might well have an entirely different slant on things.

Often a working scientist is apt to regard a theory as rather like a salami. Late at night while working in the lab, a scientist overtaken by hunger might go to the fridge and take out a salami to make herself a snack. Alas, there is some mould on one end of

[3]Though we wouldn't follow Quine in giving them a more central place in our 'web of belief.'

the salami. Does she throw it out? Probably not, after all when faced with a salami which has mould on one end, no law compels us to eat the mouldy part. Cut a few slices from the good end! And the same goes for our theory. Don't use the mouldy part of that either.

In fact, why not cut away the mouldy part of the theory entirely and, in order to make it sound better, we could refer to this operation as 'renormalization.' Of course it would be better if we didn't have to resort to this sort of thing, but the world being the kind of place it is, we often do. Show her a physical theory which does all that her renormalized theory does, without invoking that somewhat disreputable operation, and our scientist will embrace it with open arms. Later, at some learned conference or other, she and her colleagues might have a good laugh at the bad old days of renormalization, and proclaim that the younger folk in the profession have it much easier than they did.

But right now there is no such theory and the lab is waiting for our experiments and the journals our articles. There are promotions to get and prestigious fellowships. There are even prizes to win and often respectable amounts of cash are involved. Little wonder then that nobody is prepared to twiddle their scientific thumbs while awaiting a more worthy theory.

This makes philosophers sound a lot purer in spirit than scientists, but it really isn't so. Consider the ever fascinating realm of moral philosophy. Here we have a rather similar problem. We have two grand approaches to moral theorizing which we might term axiological and deontological. Each of them 'sounds right' in the sense of agreeing with many of our central moral intuitions. But not only are they not compatible, each is refutable by appeal to a central intuition highlighted in the other.[4] In other words, we cannot abandon axiology for deontology or vice versa in order to gain a more secure foundation for morals.

So it seems in moral philosophy as in physics, we make do.

[4] Thus deontological theories are vulnerable to 'lifeboat' counterexamples while axiological theories are vulnerable to 'scapegoat' counterexamples.

We use the non-mouldy part of the theory and pretend as well as we can that the mouldy part doesn't really exist. If we generalize our notion of X-consistency, we can make our logic conform to this behavior. We can agree that it is a stop-gap measure, one to be used as a last resort, but evidently a last resort is needed.

The central idea is that we get all the mould into one set called Σ. Less colorfully, Σ contains the (X) denials of each of our commitments so that every member of Σ is a sentence which we refuse to see in the output of our theory. In order to dodge some complications we stipulate that $\ell^X(\Sigma) = 1$, and the same for all its subsets. Now we define:

> A set Γ is (X) Σ-consistent indicated by CON_X^Σ, provided that
> $\Gamma \vdash_X \alpha \implies \alpha \notin \Sigma$.[5]

Evidently, Σ-consistency implies the usual sort.

From this the definition of Σ-logical cover and Σ-level is obvious. In those definitions we merely replace CON_X with CON_X^Σ. The revised notions will be denoted by COV_X^Σ and ℓ_Σ^X respectively, while the account of forcing which uses Σ will be represented by $[\Vdash_X^\Sigma$. It should be clear that there will be, in general, far fewer Σ-logical covers for a given set Γ than there are logical covers.

Not all of the old rules will hold; there is bound to be some cost associated with banishing certain contingent sentences. The earlier form of [Ref] had it that:

$$\frac{\Gamma \cap \Delta \neq \varnothing}{\Gamma [\Vdash_X \Delta}$$

when sets are allowed on the right, and

$\alpha \in \Gamma \implies \Gamma [\Vdash_X \alpha$

[5] In the more general calculus where sets are allowed on the left, this definition can be stated more simply using $\Gamma \nvdash_X \Sigma$.

when they aren't.

This will no longer hold when either $\Gamma \cap \Sigma \neq \varnothing$ or $\Delta \cap \Sigma \neq \varnothing$. What we require is a more general form of [Ref] viz.:

[Ref$_\Sigma$]
$$\frac{\Gamma \cap \Delta \neq \varnothing \;\&\; (\Gamma \cup \Delta) \cap \Sigma = \varnothing}{\Gamma \big[\Vdash^{\Sigma}_{X} \Delta}$$

or

[Ref$_\Sigma$] $(\alpha \in \Gamma \;\&\; \alpha \notin \Sigma) \implies \Gamma \big[\Vdash^{\Sigma}_{X} \alpha$

We get back The usual form of [Ref] by replacing Σ with $\varnothing$. In fact this move collapses Σ-forcing into vanilla forcing.

Treating Σ as if it were as bad as $\varnothing$ also requires other changes. We cannot inherit quite as many inferences from the underlying logic X as we did. It remains true that any X-theorem will also be an X-Σ-forcing theorem since the members of Σ are contingent by stipulation, but it will no longer be the case that every X-consequence of a unit premise set will be preserved. We must drop all those unit sets the members of which belong to Σ. To use some terminology coined elsewhere, unit sets of members of Σ cannot be regarded as *singular*. Of course this means that such sets cannot figure in the generalization to X-forcing of the rule [Mon] of monotonicity.

Σ-forcing is a true generalization of forcing and not one of the those approaches which use something akin to *counter-axioms*. Neither do we simply add the denials of all members of Σ to every premise set and proceed from that point by ordinary forcing.

The difference is simply that on the counter-axiom approach or the modified forcing method, the denials of members of Σ are, one and all, consequences of every premise set. This is not true for Σ-forcing since, for example, each 'bad-guy' in Σ is contingent, none will be Σ-forced by the empty set and neither will any of the denials (presumably 'good-guys', although not so good that we want to be able to infer them from an arbitrary premise set).

Having said that, we must acknowledge that while Σ-forcing might provide the basis for some operation like renormalization in physics, it can't be the whole story. This is because we don't simply block the unpalatable in the latter case, we also replace it with the palatable. So if the raw theory tells us that the value of e is infinite, we not only snip that out, we replace it with what we take the actual value to be. One can imagine different ways this might happen, including adopting extra 'non-logical' axioms, but more work would need to be done before something convincing could be offered.

5.4 A-Forcing

This is another of the variations on the notion of forcing which reduces the number of logical covers to be considered in calculating whether or not certain conclusions follow from a given premise set. In many applications, unadorned forcing seems to do a bit too much violence in breaking up premise sets (and conclusion sets too, perhaps). If we think of forcing as a candidate for an inference relation to use in reasoning from sets of beliefs, this quickly becomes evident. Representing the belief set of an individual i by B_i, it might be argued that in most cases B_i has a level greater than 1. It might even be argued that B_x must have a level of at least 2 if i is rational.

The conflict between this motivation and ordinary forcing or Σ-forcing is that we usually identify within some B_i natural *clumps* of beliefs. Thus, while the whole set of i's beliefs may have level 2 or more, we expect and allow i to draw inferences from certain consistent subsets of B_i. In other words, we refuse to allow logical covers to prejudice the integrity of any clump of beliefs. In effect, we rule out all those logical covers which do not respect clumps.

For example, if Simon is unable to start his car one morning, he may come to believe, as the result of inference, that his distributor is at fault. In general, we would be willing to allow such reasoning as correct even though $\ell(B_S) = 2$ and there exist covering families of the appropriate width which would prevent the inference:

$B_S [\!\Vdash_X$ Simon's distributor is at fault.

We would say, in this situation, that the set of Simon's beliefs about cars and about his car in particular (or at least a respectable subset of these) is immune. The question of which subsets of B_i are to be counted as immune is not to be conclusively settled on logical grounds. This determination must depend upon many factors, not least the identity and circumstances of i.

For ease of exposition and to avoid unnecessary complications, we stipulate that the immune subsets of any premise set have level 1.

Our formalization of the intuitive idea of immunity involves a function A, defined on sets of sentences and having as value for a given set Γ, a set of subsets of Γ i.e.

For all sets $\Gamma : A(\Gamma) \subseteq 2^{\Gamma}$

with $A(\Gamma)$ being interpreted informally as the set of all immune subsets of Γ. In distinction to the definition of Σ-forcing, we need make no change in the definition of consistency, but rather to the definition of logical cover. To this end we introduce the notion of a logical cover relative to both the underlying logic X and the function A, indicated by $\text{COV}^A_X(\mathfrak{F}, \Gamma)$

$$\text{COV}^A_X(\mathfrak{F}, \Gamma) \iff [\text{COV}_X(\mathfrak{F}, \Gamma) \,\&\, (\forall a \in A(\Gamma) \exists x_i \in \mathfrak{F}) a \subseteq x_i]$$

Thus we allow only those logical covers which do not break up the immune subsets of Γ. It seems most natural in this setting to consider some single conclusion presentation of the inference relation since we do not usually think of conclusion sets as forming clumps. In either case, however, the definition of the relation we shall call "$[\!\Vdash^A_X$" will be obvious.

There are, of course, many conditions which might be placed upon A-functions, each giving rise to a distinctive sort of A-forcing.

Some are of great interest in the studies of modality mentioned earlier in this volume, while others recommend themselves mainly to the inferentially minded. To recover ordinary forcing, we simply set $A(\Gamma)$ empty, i.e.

$$\Gamma\big[\Vdash \alpha(\Gamma\big[\Vdash \Delta) \Leftrightarrow \Gamma\big[\Vdash^{A} \alpha(\Gamma\big[\Vdash^{A} \Delta) \;\&\; A(\Gamma) = \varnothing.$$

The other end of this spectrum is not obtained by setting $A(\Gamma) = \Gamma$ since this might violate our restriction on the level of immune subsets. The right condition is instead:

$$\textsc{cov}_X(A(\Gamma), \Gamma)$$

(which obviously reduces to $A(\Gamma) = \Gamma$ when $\ell(\Gamma) = 1$.) This turns out to be one of the modally interesting relations particularly useful in discussions of deontic logic.

Under A-forcing we cannot break up immune sets, which will have a nice consequence that we can state if the underlying logic X is productival.

[A]

$$\frac{\Gamma\big[\Vdash^{A}_{X} \alpha_1 \;\&\; \ldots \;\&\; \Gamma\big[\Vdash^{A}_{X} \alpha_k \;\&\; \exists a \in A(\Gamma) : \{\alpha_1, \ldots, \alpha_k\} \subseteq a}{\Gamma\big[\Vdash^{A}_{X} \pi(\alpha_1, \ldots, \alpha_k)}$$

where $\pi(\alpha_1, \ldots, \alpha_k)$ is the product (in X) of the α's.

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