Looting Liars Masking Models

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1 Introduction

It must have been in the late 1970s that Graham first wrote to me. He was one of those planning the big black paraconsistency book [24] and collected some more papers. I sent mine around 1980. In 1982 we first met, both occasionally being in Pittsburgh, Pa. Since then, we became friends. Now and then Graham passed by in Belgium, alone or with family. He was there in 1997 for the organization of the First World Congress of Paraconsistency, an event he had proposed to our research group on a previous visit. In 2000 I spent part of a sabbatical in his Brisbane department; Graham was a great help. Since then we regularly met, most often in conferences.

It will not come as a surprise that I was enthusiastic about the initiative that led to the present book. It is truly meet and just that Priest's work and Priest as a scholar be honoured with this book. Yet, there are two warnings that should prevent the reader from wrong expectations concerning my contribution. The first warning is that, while Priest and I have contributed to paraconsistent logic as a discipline, our views are rather different. One example concerns global *vs.* local paraconsistency. Priest is convinced that there is a 'true logic' and that it is paraconsistent. My enthusiasm for paraconsistency is caused by the insight that scientists as well as philosophers of science need paraconsistent logics, actually several of them, in specific situations.

Disagreements may appear to come in handy for a contributor to this book. They offer an opportunity to highlight Priest's position and to discuss weaknesses and strengths. This is not without difficulties and my second warning pertains to them. Intellectual discussions are clearly essential to improving human knowledge and action. Yet, most public discussions, whether written or oral, are frustrating rituals. They are boring and inefficient sham fights, relying on a mistaken view of the value of research programmes and on a mistaken view of attaining knowledge. That, at the present moment, certain problems are unsolved and some objections unanswered need not mean much for the viability of an approach or for its future.[1] Unsolved problems may be solved in the future. The solutions may cause adjustments of the research program and its theories,

*I am grateful to the two referees for helpful comments.

[1]Newton's achievement is not diminished by the fact that, from his days to the advent of

even adjustments in some rather central tenets. So even valid criticism that will never be retorted may be fully insignificant after all. Knowledge is attained by creative work, not by winning quarrels.

In which way can I sensibly contribute to the present volume? My plan is to describe some alternatives to aspects and parts of Priest's program and to ask questions on the program with the alternatives as background. If I succeed, the description should have some interest for the reader, independent of Priest's reaction, and should enable Priest to pick some topics which he might care to address for clarifying or pointing out a direction or presenting a solution.

While disagreeing, sometimes severely, with Priest in several central respects, I am deeply impressed by what he achieved over the years. It is hardly exaggerated to say that he developed dialetheism from a bunch of ideas plus a little bit of formal logic into a multifaceted, rich and connected set of philosophical and meta-mathematical theories, sided by a number of examples of paraconsistent mathematical theories. Next, while the disagreements will soon surface, as will the reasons for my doubts on the viability of his program, I consider it of utmost importance that he continues to carry it out. If Priest succeeds, my view will need drastic readjustment and, even if it survives in changed form, I shall learn much about its weak spots and their circumvention. If Priest fails, it will be interesting and important to learn to which extent his views can be upheld and which problems can be given a solution in agreement with those views.

In what follows, I shall avoid digressions on the justification of my own views—this book is about Priest's—but I should still try to avoid misunderstandings. Occasionally, I shall have to refer to adaptive logics, but this is not the place to present background on those. Readers who do not believe me and are interested may follow the references. A particular difficulty is that Priest and I have criticized each other in previous papers. There were some misunderstandings, but commenting on those papers would only increase the confusion, besides being boring for the reader. So I have done my utmost to make the present paper self-contained, except that I expect the reader to be familiar with Priest's views or, where useful, to have a look at the passages from Priest's work that I refer to. And I hope I have learned from earlier misunderstandings and will avoid them better in the present paper.

The central topic of the present paper pertains to Priest's semantics for the logic **LP** and the connected understanding of semantics. The questions raised should not be read as criticism, but as a request for being instructed. As we all know, questions are not sensible and perhaps not even meaningful without some background. Everything one says, especially if it concerns philosophical background rather than, say a train timetable, has a variety of presuppositions. All of these cannot be made explicit, were it only because some are prejudices, but not making them explicit engenders misunderstanding. So, before getting to semantics, I shall comment on two background issues on which Priest and I have conflicting views. One concerns epistemological pluralism and its effect on logical pluralism, the other natural languages. I shall try to somewhat clarify both issues. More importantly, I shall attempt to show that the background views are sensible and viable. The aim is not to show that Priest's corresponding

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relativity theory, several severe problems remained unsolved. Next, however significant the problems solved by early relativity theory, they were outnumbered by the problems solved by Newtonian mechanics. A sane view on scientific problem solving was elaborated by Larry Laudan [14].

background views are mistaken, but merely that they are not obvious.

Let me end this section with a warning. I shall, as usual, write in a classical metalanguage. This means that an unqualified "not" stands for the negation of **CL** (Classical Logic) and that "false" stands for the classical negation of "true". In Section 4, however, on Priest's semantics, I shall use true and false as Priest does. To proceed differently would be too confusing. I shall remind the reader when we come to that section.

2 Logical Monism

This aspect of Priest's view is one of the hardest to make sense of for me. So I tried to avoid it. Yet, it is so central that I have to go into it in order to eliminate even the most elementary misunderstandings. I have stated my position and argued for it elsewhere and prefer to refer the reader there rather than repeating myself [2, 5]. Still, I shall state here what is required for a correct understanding.

Taken in the broad sense of the term, a logic **L**, defined for a language $\mathcal{L}$, is a function that maps every set of sentences of $\mathcal{L}$ to a set of sentences of $\mathcal{L}$. So, where $\mathcal{W}$ is the set of sentences of $\mathcal{L}$ and $\wp(\mathcal{W})$ the power set of $\mathcal{W}$, $\mathbf{L}\colon \wp(\mathcal{W}) \to \wp(\mathcal{W})$. $\mathcal{L}$ is either a formal language or a fragment of a natural language. Aristotle's theory of the syllogism illustrates the latter.[2]

Once **L** is decently delineated, by a syntactic or semantic or procedural method, or by another good method that decently delineates **L**, a normative realm has been established. If **L** is, for example, **CL**, then it is correct to infer $A \supset B$ from $\neg(B \supset A)$ and it is not generally correct to infer A from the premises B and $A \supset B$. If **L** is intuitionistic logic, neither inference is generally correct. Other logics settle the meaning of $\supset$ as detachable in both directions, and then the second inference is correct. So I list for future reference:

(1) Every logic defines its own normative realm.

Most logics in the aforementioned broad sense cannot even be delineated—there are uncountably many and only countably many can be delineated. Most of those that can be delineated are useless, for example because some of their symbols are tonk-like.[3] Interesting logics fix the meaning of (at least some) interesting logical symbols and moreover have interesting metatheoretic properties. Interesting logics are logics in a more narrow sense than the one considered, but this changes nothing to (1).

Presumably Priest agrees with (1) in as far as it concerns formal systems or closure operations. Still, his view is that there is also logic in a more serious sense and for this he rejects (1). He coined the expression *logica ens* [23] to refer to that sense: the facts about consequence that are independent of our theories and of the way people actually reason; "what *is* actually valid: what

[2]Where a fragment of a natural language is involved, there is a striking structural circularity: the fragment comprises certain occurrences of sentences; the sentences need to have certain forms; in the occurrence certain words need to have the meaning fixed by the theory. The importance of the theory will depend on properties of the fragment. The theory may be invoked as a stipulative definition.

[3]But caution is advisable. The logic **UCL** extends **CL** in that a **CL**-model is a **UCL**-model iff, in the model, every predicate has either the domain D or the empty set as its extension (interpretation). Although no **UCL**-model 'fits' the real world, **UCL** is a sensible technical entity in some adaptive logics of inductive generalization [6].

really follows from what." The reference to this entity is (at least implicitly) present in nearly any of Priest's papers and books. I really tried to understand what *logica ens* might be, but it remains a mystery.

I surmise, but nothing more, that in order to reject (1) for *logica ens*, Priest locates *logica ens* not in a formal language, but in 'the vernacular'. I write that in single quotes because I find the claim baffling. In some publications "the vernacular" is used exclusively [23], in others it only occurs occasionally and the standard term is "English" [20]. I do not see that English might be constructed as a formal-system-like entity, an entity that resembles a formal language plus a logic fixing the meaning of the logical symbols, but that is more complex and more sophisticated. Neither does English seem to display much stability over time, or even space.[4] I rather see natural languages as malleable communication instruments, by which we understand each other, in small communities even on specialized matters like Priest's writings, mainly because we are able to clarify what we mean within the very language.

To some extent, Priest admits these complications [23, §4.2]. He states (i) that the words involved in propositions about validity may change their meanings, whence the truth values of the involved sentences change rationally and (ii) that the meaning of logical constants in the vernacular may change rationally and hence that the propositions the vernacular is able to express may also change rationally. But in which way can such changes be combined with a *logica ens*?

Suppose for a moment that there is a vernacular that agrees with Priest's view. Next, suppose that the set of sentences of this vernacular remained the same over the years, but that the implication of the vernacular has today a meaning different from the one it had twenty years ago. Let "old vernacular" and "new vernacular" refer to the vernacular before and after the meaning change. Let A be a sentence of this vernacular and Γ a set of such sentences. Suppose that A *really followed* twenty years ago from Γ in the old vernacular, whereas A does not *really follow* from Γ in the new vernacular. Does this not entail that A still *really follows* from Γ in the old vernacular? If so, has logical monism evaporated? Incidentally, even if the old and new implication can be combined into a single logic, this does not save logical monism.

It seems useful to briefly return to syllogistic.[5] To cut a long story short, every *categorial proposition* stands for an equivalence class of occurrences of sentences from the vernacular. Drunk logicians aside, no one ever says "All humans are mortal." Even those talking the vernacular would utter "Sooner or later a person dies" or "We'll all die" or "No one escapes death" or another decent English sentence. However, each such sentence S has also meanings that are not identical to the meanings of the categorial propositions in the equivalence class in which the occurrence of S belongs. This holds even for the categorial proposition itself. I'll spare you the examples of categorial propositions involving negations that cause trouble in most natural languages, idiolects and other vernaculars. Summarizing the central claim:

[4]Priest argues that *logica ens* may be modified [23], but it seems to me that the dynamics of natural languages resides at a 'deeper' level than the change Priest has in mind.

[5]The English Wikipedia page clarifies that the application of syllogistic to the vernacular requires a set of extremely artificial constraints. Comparing with Wikipedia pages in other languages is even more instructive. On the Plattdüütsch (Low Saxon) page, "mütt" (should) is used as a copula in one of the examples.

(2) Logics claimed to pertain to the vernacular actually pertain to a fragment of the vernacular that is selected by the normative realm of those logics.

A last but weighty topic considered in this section concerns the relation between logical monism and epistemic monism. The traditional Western idea, that knowledge is a unified and monolithic body, is hardly more than a very distant ideal. Sometimes it is proposed as an aim or program—examples are the Vienna Circle's *Einheitswissenschaft* or some physicists' "theory of everything". Often, however, one reasons as if the ideal is already realized or nearby. Or one reasons as if it were the only sensible form of knowledge to pursue, or the only viable idea of 'finished knowledge'. And such unified knowledge body presumably presupposes a unique logic.

Actual knowledge is clearly very different from the so-called ideal. As I have argued for this elsewhere [5], I heavily summarize. The present body of knowledge forms a patchwork of partial, incomplete and non-axiomatized theories. These theories stem from a large number of disciplines and subdisciplines. Most such theories are fully unrelated to other theories in the patchwork. Next, for nearly all mathematical theories and for most empirical theories, alternatives are available. Moreover, even a unique *concrete* framework for building a unified body of knowledge is absent—even for integrating economic theories, let alone for integrating them with cognitive psychology, let alone for integrating all that with quantum mechanics, ... The situation is dramatically complicated *further* by the crucial role of methods and cognitive values.[6] There are theories about these as well as practices and both require a justification, which is about the theories and practices, not part and parcel of them. Next, the methodological theories often cause changes within descriptive theories and *vice versa*; this interaction may generate a complex dynamics involving many hard problems. Descriptive theories, methodological theories, and conceptual systems or languages, including logics, are modified and replaced. All such changes are unpredictable. What I mean is that it is plainly impossible to delineate beforehand a set to which the modified entities will belong.[7]

Suppose for a moment that our knowledge will evolve in the direction of a unified and monolithic body and that **L** will be the logic of this body. Is there any warrant that we, plodding and dabbling in our patchwork, should or even may do so—plodding and dabbling, I mean—in terms of **L**? I do not think so. All we can do is proceed in terms of the logics that underlie the diversity of theories and practices of our patchwork.

The supposition from the previous paragraph seems a bit unrealistic. One of the few things we seem to know is that the complexity of reality is thus that we shall get stuck forever with a patchwork and with a diversity of logics, even deductive logics. Presumably and hopefully, the patchwork will be much more integrated than it is today. This may be accomplished by specifying in a precise way the information flows between the different patches—these need neither be

[6]So, yes, methods and values belong to our knowledge. A few positivist fossils aside, everyone agrees on that.

[7]A theory will presumably be required (i) to be a set of statements from an fully unspecified language and (ii) to be presented in a further unspecified way judged acceptable by the future competent community. Had Aristotle anything more specific to delineate the set from which 21th century theories would be taken? And "theory" is an easy case as it need not involve meanings of 'referring' terms. Try redoing the exercise for the conceptual systems of social psychology or string theory.

one-sided nor one-shot—and in which way the consequence sets of the enriched chunks are defined. Together with Bryson Brown, Priest has devised a rather general and embracing methodology [7, 8] which does precisely that and he has shown [22] that logical pluralists may apply the mechanism to their profit. Priest still remains faithful to logical monism. Yet, I wonder how he can be sure that humanity will not be stuck with a patchwork forever.

Until now, I was considering the position that the true logic might be determined by the hypothetical end state of our knowledge, provided this evolves into a unified and monolithic body. If our best knowledge at the end state is such a body, including methods and values and so on, this body should be considered our best hypothesis about the true structure of the world in all its aspects. The language in which the body will be expressed will be our best guess for a suitable language to codify our knowledge. Let us call this the empirical dimension of language.[8] So in this sense there is an empirical aspect to the formal logic of the suitable language and to its conceptual system; the latter settles the informal logic of the language.[9]

Actually, Priest seems committed to a position that comes close to the one from the previous paragraph, except that he seems to think that the true logic can change over time [19, Ch. 10]. I find this puzzling. Suppose that Priest's arguments for **LP** hold water. Let him throw in his preferred relevant implication. And let us not complain about all the logical terms (counterfactual implications and other conditionals, non-standard connectives, non-standard quantifiers and so on) that occur in natural languages—sloppily supposing that a conservative extension of **LP** takes care of all of them. But that does not make **LP** into the right logic on that position. Today we still have the patchwork. Where **CL** is taken to be the underlying logic, Priest might argue that the underlying logic is actually an inconsistency-adaptive logic—all of them define the same consequence set as **CL** if the theory is consistent [3]. But what about quantum mechanics? And what about many other patches, including methodological ones? What about constructive mathematical theories that clearly are patches of our present knowledge system? And so on. Take even set theory. Priest justly considers Zach Weber's Fregean set theory [30, 29, 31] as the best available paraconsistent set theory. This set theory, however, does not proceed in terms of Priest's favourite relevant implications. So in order to integrate this set theory into the knowledge body, Priest needs a mechanism that would just as well work for the patchwork, possibly one of the aforementioned 'chunk and permeate' mechanisms. Or else Priest needs to show that Weber's set theory survives the addition of *all* relevant implications that Priest allows within his language, viz. in the vernacular.[10] Moreover, Priest will still need to consider other set theories, like **ZF** and the other supposedly consistent ones, because

[8]As in all language use that relates to the world, there is an interaction between conventional elements and properly empirical elements in the empirical dimension *of language*. This is not the place to spell that out.

[9]Informal logic is meant in the traditional sense here. Carnap [9] tried to push informal logic into meaning postulates. This heavily restricts the possible meaning relations because it requires a theory rather than an underlying algebraic structure.

[10]The central equivalence sign in the abstraction axiom will still be Weber's, but every implication of the language may occur within the open formula in that axiom. This will impose restrictions on the interaction between the implications. Thus, where $\rightarrow$ is Weber's implication and $\Rightarrow$ *any* implication Priest allows in his language, the inference from $A \rightarrow (A \Rightarrow B)$ to $A \Rightarrow B$ should be invalid.

these are not fragments of Zach Weber's. So, even if Priest is right in other respects, I do not see that he would, according to his own criteria, have identified 'the true logic' or even that he would have shown that such an entity exists.

There is an excellent reason to believe that we shall never get beyond the patchwork stage. We need to think *about* our knowledge in order to improve it or even to find out whether it is justified. Such thinking requires that we give up, at least for the sake of this argument, the knowledge elements about which we are thinking.[11] It is not essential for my argument, but let me add that this also applies to logic—the paragraph on the new and the old vernacular illustrates that. Anyway, we need the patchwork and better hope that it remains available forever. Having a unified and monolithic theory locks one in a straitjacket nailed to a dungeon wall. It equates the perfect dogmatic closure.

3 Natural Languages

There are many convincing arguments for the view that we need paraconsistent logics. Yet, are Liar arguments really exciting? I shall not mention the common suspicions against them. In order to show, as Priest candidly does, by a Liar argument that explosive negations are tonk-like operators, one needs some presuppositions. One of Priest's presuppositions is clearly that natural languages are formal-system-like entities. The analogy seems fundamentally wrong to me. Is it not more appropriate to see natural languages as malleable communication instruments, as stated in Section 2? Again, I shall not discuss the topic, but rather stress that the view may be given a formal underpinning. In this way I hope to show that there is a sensible alternative for Priest's view on natural language and that Priest's view is not obviously correct.

The idea of a malleable communication instrument is that the words of the language do not have a stable meaning, but may be given new meanings to express things that could not be expressed by the language as previously used. If a phrase is available that the 'speaker' knows to be ambiguous, it may be disambiguated. If no phrase (or word) is available, the speaker may give a special meaning to an existing one, possibly through a metaphor, or coin a new one. This may require an amount of communication about meanings rather than about the topic under discussion, but we all know how to do that. If speaker and addressee can both interfere, the communication about meaning can often be drastically restricted. A reader of a text has to proceed more carefully, especially if the text is not written for this type of reader or is phrased clumsily.

It is quite obvious that communication about meanings takes place. That atomistic ideas, like those of Democritos, had remained part of the culture, made it easy for people like Dalton to propagate atomistic chemistry as an alternative for the existing qualitative chemistry. Galileo had a more difficult job to explain relative motion to his contemporaries. Ideas that did not belong to the logical space previously may originate and spread. In this way, *new possibilities* are generated.[12] While this malleability of non-logical terms is obvious from the history of the sciences, the situation is similar for logical terms. The word "and"

[11] This is a grave understatement. It is well known that creative scientific work requires a patchwork that is not only complex but also dynamic [16, 17, 18, 15].

[12] These are informal logical possibilities in the sense that they depend on meanings of non-logical terms. Nice relevant work on possibility was done by Nicholas Rescher [25, 26, 1].

may have a temporal meaning, in which case A-and-B is not a consequence of $\{A, B\}$. The word “if” is often meant as an equivalence, which is just one of the many reasons to be careful in interpreting Wason test results. And many more examples may be given.

Is it possible to communicate if words have no fixed meaning? We can because we do. This will be more convincing: it is possible to formally make sense of such languages and their use as I have shown in writing [4] and shall now explain. That words do not have a fixed meaning does not prevent them from having a likely meaning, or a couple of them, possibly ordered. Let us proceed stepwise.

By way of a purely pragmatic decision, take **CL** as our preferred logic and hence as the norm. For the subsequent reasoning, start thinking about **CL**-models and allow them gradually to be transformed. If a model M allows for $v_M(\neg A) = 1$ while $v_M(A) = 1$, I shall say that M displays a *negation glut*.[13] A *negation gap* to the contrary is obtained when $v_M(A) = 0$, but nevertheless $v_M(\neg A) = 0$. A *conjunction glut* occurs in M if either $v_M(A) = 0$ or $v_M(B) = 0$ but nevertheless $v_M(A \wedge B) = 1$. A *conjunction gap* occurs in M if $v_M(A) = v_M(B) = 1$ but nevertheless $v_M(A \wedge B) = 0$. And so on for the other connectives, for the quantifiers and for identity. Summarizing, where ξ is the central logical symbol of A, if the model M is such that the conditions are sufficient for $v_M(\mathsf{A}) = 0$ in a **CL**-model M, but nevertheless $v_M(\mathsf{A}) = 1$, then there is a ξ-glut;[14] if the model M is such that the conditions are sufficient for $v_M(\mathsf{A}) = 1$ in a **CL**-model M, but nevertheless $v_M(\mathsf{A}) = 0$, then there is a ξ-gap.

It is possible to consider all logics that allow for gluts or gaps with respect to a logical symbol or for the combination of any selection of gluts with any selection of gaps. With respect to referring terms, a similar step may be taken, as was first suggested by Guido Vanckere [28]. The idea is simple: replace within the language every non-logical term ξ by a numerically superscripted one ξ^i and next replace every $\Gamma \vdash A$ by $\Gamma^\dagger \vdash A^\ddagger$, obtained by replacing in every member of Γ and in A every non-logical term ξ by an indexed non-logical term ξ^i in such a way that no two identical non-logical terms ξ^i occur in the translation $\Gamma^\dagger \vdash A^\ddagger$—the accurate transformation is just a trifle more complicated.

So now we have logics that allow for gluts or for gaps or for ambiguities or for combinations of those—there is a naming system that identifies each combination. The logic that allows for all gaps and gluts and ambiguities is called **CL∅I**. This is defined over the language in which the non-referring terms are superscripted. Zero logic, **CL∅**, is defined over the standard language: $\Gamma \vdash_{\mathbf{CL}\emptyset} A$ iff $\Gamma^\dagger \vdash_{\mathbf{CL}\emptyset\mathbf{I}} A^\ddagger$.

Note that (i) $p \wedge q \nvDash_{\mathbf{CL}\emptyset} p$ because there are models M, allowing for conjunction gluts, such that $v_M(p \wedge q) = 1$ and $v_M(p) = 0$, but also that (ii) $p \nvDash_{\mathbf{CL}\emptyset} p$ because $p^1 \nvDash_{\mathbf{CL}\emptyset\mathbf{I}} p^2$. So **CL∅** is justly called *zero logic*: nothing follows from anything, not even a premise from itself.

The fascinating bit happens when one goes adaptive, viz. when one moves to

[13] Allowing for negation gluts in an indeterministic semantics is simple enough: remove the clause “if $v_M(A) = 1$, then $v_M(\neg A) = 0$” and keep “if $v_M(A) = 0$, then $v_M(\neg A) = 1$”. In a deterministic semantics, the valuation function v_M is determined by the model, which here is $M = \langle D, v\rangle$ with D a set and v an assignment function. I refer to literature for details [4].

[14] This may be phrased in a perfectly unambiguous way [4]. As the reader expected, A is a metametalinguistic variable. If you do not like meta talk: it is a variable for variables for formulas.

the logic $\mathbf{CL}\emptyset^{\mathsf{m}}$. Here abnormalities are minimized and in this case abnormalities are the different gluts and gaps, and the ambiguities.[15] For any specific premise set, respectively set of non-logical axioms of a theory, $\mathbf{CL}\emptyset^{\mathsf{m}}$ delivers a minimally abnormal interpretation of the premise set. And so do some other adaptive logics of the described family—those logics allow only for a selection of gluts or gaps or ambiguities.[16] Moreover, the application of $\mathbf{CL}\emptyset^{\mathsf{m}}$ reveals which of the other logics of the family are suitable. Put differently, the application of $\mathbf{CL}\emptyset^{\mathsf{m}}$ reveals the sensible minimally abnormal interpretations of premise sets. Some premise sets have minimally abnormal interpretations in logics that allow for one of a few selections of abnormalities; other premise sets have more minimally abnormal interpretations.[17]

Here are two of the many interesting facts about the logics in this family.

(3) If Γ has **CL**-models, then $\mathrm{Cn}_{\mathbf{L}^{\mathsf{m}}}(\Gamma) = \mathrm{Cn}_{\mathbf{CL}}(\Gamma)$ for every adaptive logic $\mathbf{L}^{\mathsf{m}}$ of the family.

(4) If Γ has no **CL**-models, whence $\mathrm{Cn}_{\mathbf{CL}}(\Gamma)$ is trivial, then, some border cases aside,[18] there are adaptive logics $\mathbf{L}^{\mathsf{m}}$ of the family such that $\mathrm{Cn}_{\mathbf{L}^{\mathsf{m}}}(\Gamma)$ is not trivial, but is a minimally abnormal interpretation of Γ.

The significance of these results is that one is able to handle languages in which the meaning of symbols is an empirical matter, viz. a matter determined by the text one is interpreting. So one may see this as a formal hermeneutics. Applying this to a natural language, logical words have a normal meaning, but may be used with a different meaning within a text. Non-logical words and phrases have normally the same meaning throughout a text, but some texts are ambiguous. When reading a text, we take it to be normal. If it turns out to be abnormal—the normal interpretation is then trivial—we shall try to find a maximally normal interpretation. It is very natural that there are several of these. All this nicely agrees with the behaviour of $\mathbf{CL}\emptyset^{\mathsf{m}}$.

With respect to the application to natural languages, some people, especially those with sympathies for relevant implications, will complain that I chose **CL** as a 'the standard' for logical symbols. That choice was merely conventional. One does not need a new idea, only more work, to adjust the construction to another standard.

More work is required in other respects as well. For example non-logical linguistic entities are handled rather simplistically by $\mathbf{CL}\emptyset^{\mathsf{m}}$, viz. as either ambiguous or unambiguous. A more realistic approach will associate several meanings with each such entity, and will give each of the meanings a definite weight. Such weights will obviously depend on the 'speaker', or rather on what we know about the speaker, on what we know about whom the speaker considered to

[15]Intuitively, there is an ambiguity in a model M when, in the superscripted language, non-logical symbols that merely differ in their superscript, like ξ^i and ξ^j, have a different meaning in M.

[16]The same holds for many adaptive logics outside the family. Infinitely many logics allow for certain gluts or gaps and there are several strategies to minimize abnormalities.

[17]Consider a premise set that has a minimally abnormal interpretation allowing for negation gluts and another one allowing for existential gaps. Any adaptive logic (from the family) that moreover allows for further abnormalities will obviously also define a minimally abnormal interpretation. However, those interpretations will be less interesting. Their consequence sets will contain many disjunctions of formulas where adaptive logics that allow for a smaller selection of abnormalities will have the disjuncts as consequences.

[18]One of the border cases is where Γ is itself the trivial set.

be the addressee, and all the further good old stuff from pragmatics and text pragmatics. This requires a sophistication of $\mathbf{CL}\emptyset^{\mathsf{m}}$, but nothing that cannot be handled by an adaptive logic.

So considering a natural language as a malleable communication instrument is not just a muddy idea but may be explicated by formal means. Note that a lot of malleability is possible and that the approach allows for languages that are shared by innumerable linguistic communities, defined by region, specialization as well as by socioeconomic factors. Similarly, the approach makes it easier to handle such phenomena as dialect continua, for example the one spreading from the Walloon region in the North to Portugal in the West and Southern Italy in the East, circumscribing five or more official 'languages'. The approach makes it easier to handle a number of further phenomena: that natural languages can be learned in the way that children learn them, that they evolve, and so on. It also makes it easier to understand the sciences. For example, that we can handle the patchwork formed by our knowledge as described in Section 2. Another example is that drastically new concepts may be spread so rapidly within the relevant scientific community.[19]

All this obviously does not prove that the formal-system-like approach to natural languages is mistaken. It does show, however, that there are other approaches and that some are capable of solving certain problems that were not yet solved on the formal-system-like approach.

Hopefully the alternative approach also clarifies why I do not see the Liar as a major problem. Natural languages have several sorts of odd *occurrences* of sentences, that we keep outside the scope of our truth theory.[20] Adding another sort to house certain occurrences of the Liar sentence might be not too high a cost. The sort may be delineated by an adaptive criterion—concepts such as 'disjunctions of abnormalities' and several results may just be copied from the metatheory of adaptive logics. We still need paraconsistency of course, for so many reasons. Yet, we should be less afraid of explosive negations within a pluralistic setting.

4 Models

Let me remind the reader that, in this section, I shall follow Priest's way of using "true" and "false".

Since a long time, I felt a certain uneasiness with Priest's **LP**-models and tried to articulate it. There are two main elements to this. First, the crucial reference to natural language for the attack, in terms of the Liar, on explosive negation. A typical statement [20, p. 16] comes to this: If "notions necessary for the formulation of the [extended Liar] paradox [. . .] are not expressible in the language in question [of the Valuegappist]", then "the language for which the semantics has been given is not English, since these notions obviously are expressible in English." The second main element is a certain tension between on the one hand a classical or inconsistency-adaptive understanding of the semantics and on the other hand a paraconsistent understanding.

[19] To explain in which way new concepts originate is a different matter. As $\mathbf{CL}\emptyset^{\mathsf{m}}$ is described here it is not helpful for that purpose.

[20] It is a matter of occurrence indeed. Imagine "Mary loves Bill" written on the wall, and underneath it "This sentence is false", especially when written by another hand.

The original **LP**-semantics proceeded in terms of functions that assign to formulas values from $\{\{1\}, \{1, 0\}, \{0\}\}$, respectively denoting true, false and true, and false.[21] This is represented on the two first lines of the table.

Priest's semantics	$v(A) = \{1\}$	$v(A) = \{0, 1\}$	$v(A) = \{0\}$
Priest's terminology	A is true	A is true and false	A is false
classical terminology	$M \Vdash A$ $M \nVdash \neg A$	$M \Vdash A$ $M \Vdash \neg A$	$M \nVdash A$ $M \Vdash \neg A$
bivalent semantics	$\mathsf{v}(A) = 1$ $\mathsf{v}(\neg A) = 0$	$\mathsf{v}(A) = 1$ $\mathsf{v}(\neg A) = 1$	$\mathsf{v}(A) = 0$ $\mathsf{v}(\neg A) = 1$

The logic **LP** has also a bivalent semantics, assigning to formulas a value from $\{0, 1\}$. In the bivalent semantics, $\mathsf{v}(\neg A)$ is obviously not a truth-function of $\mathsf{v}(A)$. Given the properties of **LP**, one has to characterize a deterministic model by specifying $\mathsf{v}(A)$ as well as $\mathsf{v}(\neg A)$ for every sentential letter A.[22] Semantic consequence is defined by Priest as follows: $\Gamma \vDash A =_{df}$ for all v, if $1 \in v(B)$ for all $B \in \Gamma$, then $1 \in v(A)$. The classical definition, proceeding in terms of the bivalent semantics, is very similar: $\Gamma \vDash^c A =_{df}$ for all v, if $\mathsf{v}(B) = 1$ for all $B \in \Gamma$, then $\mathsf{v}(A) = 1$.[23]

Both semantic systems are equivalent from a technical point of view.[24] To see that, it helps to realize that, in Priest's semantics, $1 \in v(\neg A)$ ($\neg A$ is true) iff $0 \in v(A)$ and $0 \in v(\neg A)$ ($\neg A$ is false) iff $1 \in v(A)$. The classical description of either semantics is transparent if one remembers that $M \Vdash A$ is read as "M verifies A" or "A is true in M" and $M \nVdash A$ is read as "M falsifies A" or "A is false in M". This "false" has a different meaning from Priest's, who considers the classical meaning wrong. There may be a similar difference with respect to "falsifying", but both descriptions can agree on "verifying". Note that the slash in $\nVdash$ stands for a classical negation, which is a tonk-like operator for Priest.

In the sequel of this section I shall only talk about Priest's semantics and shall only use Priest's terminology, except where I explicitly state not to do so. I shall also, as in the above table, identify a model M with a valuation v.

Relational semantics Where L is "L is false only",[25] L is true in the actual model, the model that corresponds to the state of reality, just in case its truth-value is $\{0\}$. By the T-schema and given what L states, $1 \in v(L) \leftrightarrow v(L) = \{0\}$. This readily gives one $1 \in \{0\}$ and $1 = 0$ and triviality. In 1993 Priest points out [27] that triviality is avoided when the conventional choice for functions is replaced by the choice for a relation. So, $x \in v(L)$ will be replaced by $\mathrm{Rel}(L, x)$ where Rel is a binary relation between the set of formulas and $\{0, 1\}$. What was formerly noted as $1 \in v(L) \leftrightarrow v(L) = \{0\}$ and caused triviality, becomes

[21] True and false here obviously stand for true-in-a-model and false-in-a-model.

[22] Priest's semantics has positive and negative extensions for predicates and these require no complications for the transition to the bivalent semantics. And the same trick would work for sentential letters.

[23] Actually, the implications in both definitions are different. For readers not familiar with the matter, just neglect that for the present paper.

[24] Actually, they are equivalent—for all Γ and A, $\Gamma \vDash A$ iff $\Gamma \vDash^c A$—provided Priest's semantics is consistent with respect to $\vDash$: there is no Γ and A such that $\Gamma \vDash A$ and $\Gamma \nvDash A$.

[25] That is: L is false and not true. Some will prefer "$\ulcorner L \urcorner$ is false" in which $\ulcorner L \urcorner$ is a name for L. I shall continue to use formulas as names for themselves as this causes no confusion in the present paper.

$$\mathrm{Rel}(L,1) \leftrightarrow (\mathrm{Rel}(L,0) \wedge \neg\mathrm{Rel}(L,1))\,. \tag{SL}$$

Call this (the modified or corrected formulation of) the Semantic Liar—it proceeds in terms of truth-in-a-model rather than in terms of truth. Priest requires that $\mathrm{Rel}(L,1) \vee \mathrm{Rel}(L,0)$ and that the equivalence in (SL) is a relevant contraposable equivalence, which here stems from the T-schema.

Priest points out [20, pp. 288-289] that, with this reformulation, a contradiction, viz.

$$\mathrm{Rel}(L,1) \wedge \neg\mathrm{Rel}(L,1) \wedge \mathrm{Rel}(L,0)\,, \tag{5}$$

is still derivable, but that triviality is not.[26] I refer the reader to the original paper [27], but especially to Section 20.3 of the second edition of *In Contradiction* [20]. There one obvious point is mentioned: that Disjunctive Syllogism is invalid with respect to the paraconsistent negation $\neg$. There also is a more informative point: that even when (dialetheic) sets like $\{x \mid \mathrm{Rel}(A,x)\}$ are invoked, which by all means is a legitimate move, triviality does not follow. Originally, I was very enthusiastic about the reformulation. For one thing, it also answered some complaints I had had and had raised in writing. But then doubts returned.

Let us consider a graphic representation of the old **LP**-models with respect to a formula A—I'll keep the discussion at the propositional level as long as possible. Call this the *O-representation*:

A-type-1	A-type-2	A-type-3
$v(A)=\{1\}$	$v(A)=\{0,1\}$	$v(A)=\{0\}$

One might naively think to translate this to the relational approach as follows:

A-type-1	A-type-2	A-type-3
$\mathrm{Rel}(A,1) \wedge \neg\mathrm{Rel}(A,0)$	$\mathrm{Rel}(A,1) \wedge \mathrm{Rel}(A,0)$	$\neg\mathrm{Rel}(A,1) \wedge \mathrm{Rel}(A,0)$

However, this is clearly mistaken. A formula A may in the same model relate as well as not relate to a truth value. So there are eight A-types rather than three. Call the following the *R-representation*:

	1.1	1.2	2.1	2.2	2.3	2.4	3.1	3.2
$\mathrm{Rel}(A,1)$	$\times$	$\times$	$\times$	$\times$	$\times$	$\times$		
$\neg\mathrm{Rel}(A,1)$		$\times$			$\times$	$\times$	$\times$	$\times$
$\mathrm{Rel}(A,0)$			$\times$	$\times$	$\times$	$\times$	$\times$	$\times$
$\neg\mathrm{Rel}(A,0)$	$\times$	$\times$		$\times$		$\times$		$\times$

Where $x \in \{0,1\}$, at least one of $\mathrm{Rel}(A,x)$ and $\neg\mathrm{Rel}(A,x)$ holds in a model. Exactly one of them holds in models that are Rel-consistent with respect to A. Note that A-type-2.1 models are Rel-consistent but that both A and $\neg A$ are true in them.[27]

In the old semantics, A is true as well as false in some **LP**-models. However, the semantic machinery itself is apparently consistent: $v(A)=\{1\}$ and $0 \in v(A)$ jointly engender triviality; and few readers will have considered the case where

[26] The contradiction is an obvious **LP**-consequence of $\mathrm{Rel}(L,1) \vee \mathrm{Rel}(L,0)$, (SL), and the **LP**-theorem $\mathrm{Rel}(L,1) \vee \neg\mathrm{Rel}(L,1)$.

[27] The numbering is of course conventional. In an A-type-$x.y$ model hold all statements true on the classical description of the A-type-x model (but now read paraconsistently).

$v(A) = \{1\}$ and $1 \notin v(A)$.[28] In order to avoid that an extended liar causes triviality, Priest moved to the relational semantics. This allowed him to take advantage of the paraconsistent negation within the semantic machinery and not only with respect to 'object-language' statements. For all we know, the move is effective. Moreover, it is natural from Priest's viewpoint, which considers the vernacular as a single entity, not as a hierarchy of languages. However, the move has also some unexpected effects.

Priest claims [27, p. 51]: "The catastrophic results [viz. the triviality derived on the old approach] therefore appear as spin-offs of a conventional form of the representation used, not of the facts themselves." It certainly is correct that Rel-free formulas can be false, true or both in a model, just as before; and that the old 'representation' misrepresented 'the facts', as I now further clarify.

Trouble? What follows is put under a separate heading in order to introduce some conventions that pertain only to stuff within this division. The only entities considered here are propositional **LP**, the language of propositional **LP**, and the relational semantics of propositional **LP**. $\mathcal{S}$ will denote the set of sentential letters, $\mathcal{W}$ the set of propositional formulas built from $\mathcal{S}$.[29] Unless specified otherwise, $A, B \in \mathcal{W}$, $\Gamma \subseteq \mathcal{W}$ and $\Delta \subseteq \mathcal{W}$. An x as in $\mathrm{Rel}(A, x)$ is always a member of $\{0, 1\}$. The $\pm$ as in $\pm\mathrm{Rel}(A, x)$ will stand either for $\neg$ or for the empty string. Define $\mathrm{Rel}^{\mathcal{S}}_M$ as the set of all expressions $\pm\mathrm{Rel}(A, x)$, in which $A \in \mathcal{S}$, that hold in M; define $\mathrm{Rel}^{\mathcal{W}}_M$ as the corresponding set for $A \in \mathcal{W}$.

Where $A \in \mathcal{S}$, expressions $\pm\mathrm{Rel}(A, x)$ are semantically primitive. Provided that at least one of $\mathrm{Rel}(A, x)$ and $\neg\mathrm{Rel}(A, x)$ holds in M and that at least one of $\mathrm{Rel}(A, 0)$ and $\mathrm{Rel}(A, 1)$ holds in M, nothing simpler forces $\pm\mathrm{Rel}(A, x)$ to hold in M or prevents it from holding in M. The following fact results from this.

Fact 1 *For all M and for all $A \in \mathcal{S}$, there is a M' such that* $\mathrm{Rel}^{\mathcal{S}}_{M'} = \mathrm{Rel}^{\mathcal{S}}_M \cup \{\pm\mathrm{Rel}(A, x)\}$.

The usual induction leads from Fact 1 to Fact 2.

Fact 2 *For all M and for all $A \in \mathcal{W}$, there is a M' such that* $\mathrm{Rel}^{\mathcal{W}}_{M'} \supseteq \mathrm{Rel}^{\mathcal{W}}_M \cup \{\pm\mathrm{Rel}(A, x)\}$.

Definition 3 $\Gamma \vDash A$ *iff A is true in all models in which all members of Γ are true.*

In line with Priest, I take $\Gamma \nvDash A$ to obtain iff A is untrue in a model in which all members of Γ are true—A is *untrue* in M iff $\neg\mathrm{Rel}(A, 1)$ holds in M. Fact 2 entails the second halves of Facts 4 and 5.

Fact 4 *For some Γ and A, $\Gamma \vDash A$; for all Γ and A, $\Gamma \nvDash A$.*

Fact 5 *There are logical truths; nothing is a logical truth (for all A, $\nvDash A$).*

Let reflexive, monotonic and transitive be defined as usual for consequence relations.

[28] Yet paraconsistent set theories have some inconsistent sets, viz. sets of which certain entities are members as well as non-members.

[29] So I shall never write expressions like $\mathrm{Rel}(\mathrm{Rel}(A, 1), 1)$. Many results proven for $\mathcal{W}$ are provable in general, but this is a worry for later.

Fact 6 **LP** *is reflexive and non-reflexive, monotonic and non-monotonic, transitive and non-transitive.*

Actually the matter is more extreme: $A \nvDash A$ for all A; for all Γ, Δ and A, if $\Gamma \vDash A$, then $\Gamma \cup \Delta \nvDash A$; for all Γ, Δ and A, if $\Gamma \vDash B$ for all $B \in \Delta$ and $\Delta \vDash A$, then $\Gamma \nvDash A$.

Definition 7 Γ *is non-trivial iff there is a formula* A *such that* $\Gamma \nvDash A$.

Fact 8 *Some* Γ *are trivial and all* Γ *are non-trivial.*

Definition 9 Γ *is inconsistent iff, for some* A, $\Gamma \vDash A$ *and* $\Gamma \vDash \neg A$.

Fact 10 *Some* Γ *are inconsistent and all* Γ *are consistent.*

Is any of these results bad? Maybe not. Maybe they clarify certain concepts from a dialetheic point of view. If dialetheism is right, we have to change certain insights and what precedes are just some more?

Of course, much of what precedes comes rather unexpected, definitely for some. Moreover, certain concepts seem to lose content. If, for all Γ and A, there is a countermodel for $\Gamma \vDash A$, then apparently the notion of a countermodel does not mean much. And what is the point of constructing a countermodel if the skies rain down countermodels? Does non-triviality mean much if every set of formulas is non-trivial? And most people speaking English definitely expect that the notion of consistency is consistent to the effect that no inconsistent set is also consistent.

Soundness also obtains odd properties. If $\Gamma \nvDash A$, then soundness seems to require that $\Gamma \nvdash A$ in some or other sense and I do not see any such sense for all Γ and A. Nothing in **LP**-proofs or in **LP**-tableaux seems to correspond to the universality of the absence of the semantic consequence relation. Is this unavoidable?

The matter is embarrassing in a personal way. Did I miss something? I have read most of Graham's writings and what precedes seems heavily contradicted there—I mean pragmatically contradicted. Even the proof of Fact 1 from *In Contradiction* is mistaken if what precedes is correct.[30] And what is the point of painstaking proofs of the non-triviality or consistency of a theory or fragment iff those concepts mean what they turn out to mean in Facts 8 and 10?

Another reason why the present results seem mistaken is that they overlook certain distinctions that anyone can see. While $\Gamma \nvDash A$ holds for all Γ and A, there are Γ and A for which $\Gamma \vDash A$ holds and there are other Γ and A for which $\Gamma \vDash A$ doesn't hold. The problem is to express this distinction while remaining faithful to the dialetheic tenets. There is a similar problem in identifying or describing models—see below under the heading "Describing the semantics".

So is there a way around the present results? It seems relevant that, apparently, Priest wants to introduce only concepts that do not in any possible circumstances engender triviality themselves. Can this be realized without warranting non-triviality by definition? And how well can such concepts resemble the concepts available within the vernacular?

[30] The proof stems from the first edition and obviously has to be adapted to the relational semantics. However, precisely this is impossible in general. As, for every A, $\neg\text{Rel}(A, 1)$ holds in some model M, there cannot be a method to turn M into a **CL**-model in which A is false. Next, even meta-theoretic proofs new to the second edition are in terms of the old set theoretic semantics.

An infinite hierarchy? Complications may not end here. Call A a formula of thickness 0 if "Rel" does not occur in A and, where A is of thickness n, call $\mathrm{Rel}(A,x)$ and $\neg\mathrm{Rel}(A,x)$ formulas of thickness $n+1$. As the *R-representation* shows, $\neg\mathrm{Rel}(A,1)$ is not only not a function of $\mathrm{Rel}(A,1)$. It is not a function of $\{\mathrm{Rel}(A,1), \mathrm{Rel}(A,0)\}$ either. Apparently, $\mathrm{Rel}(A,x)$ entails $\mathrm{Rel}(\mathrm{Rel}(A,x),1)$ and conversely.[31] Yet, by analogy to the above reasoning, $\neg\mathrm{Rel}(\mathrm{Rel}(A,1),1)$ is not a function of $\{\mathrm{Rel}(\mathrm{Rel}(A,1),1), \mathrm{Rel}(\mathrm{Rel}(A,1),0)\}$. This may be repeated *ad nauseam.* In other words, the *O-representation* of a **LP**-model concerns only formulas of thickness 0, and hence is terribly incomplete. Yet, the *R-representation* is hardly more complete. It fails as representation of the *full* truth status of A with respect to a **LP**-model.

Priest wants double negation and de Morgan properties to apply. So he presumably also wants to reduce the present infinity of $\neg$Rel-concatenations. A full reduction is obviously impossible. If, for example, one were to stipulate, for $\leftrightarrow$ contraposable and detachable, $\neg\mathrm{Rel}(A,x) \leftrightarrow \mathrm{Rel}(A,1-x)$, then all A-type-2 models, which are characterized by $\mathrm{Rel}(A,1) \wedge \mathrm{Rel}(A,0)$, would by definition be inconsistent with respect to Rel-expressions: $\mathrm{Rel}(A,1) \wedge \neg\mathrm{Rel}(A,1)$ as well as $\mathrm{Rel}(A,0) \wedge \neg\mathrm{Rel}(A,0)$ would hold.[32] Similarly, A-type-1 models and A-type-3 models would *by definition* be consistent with respect to Rel-expressions; contraposing: whether x is 0 or 1, $\mathrm{Rel}(A,x) \wedge \neg\mathrm{Rel}(A,x)$ would entail $\mathrm{Rel}(A,0) \wedge \mathrm{Rel}(A,1)$. But all that seems wrong.

If $\mathrm{Rel}(A,1) \wedge \neg\mathrm{Rel}(A,1)$, or $\mathrm{Rel}(A,0) \wedge \neg\mathrm{Rel}(A,0)$, there is clearly something unexpected the matter with the relation between A and its truth-value in the model. This situation is obviously possible, but it is different from, and definitely not a mere consequence of, A and $\neg A$ being both true in the model, which Priest codes as $\mathrm{Rel}(A,1) \wedge \mathrm{Rel}(A,0)$. The latter conjunction entails that something unexpected is the matter with A in the considered model, viz. that A behaves inconsistently. The preceding conjunctions entail that something unexpected is the matter with truth-in-a-model. A dialetheist should clearly allow for the possibility that our definitions of models result in inconsistent models. As noted before, that a contradiction is true in some models need not entail that truth-in-a-model is inconsistent in them. Similarly, considering a model of a consistent set of Rel-free formulas does not provide a warrant for the consistency of the set of Rel-formulas that hold in the model. Suppose, for example, that arithmetic—the set of formulas verified by the standard model—is inconsistent. In this case, there is an inconsistency in the method by which infinite predicative models are defined[33] and every infinite **CL**-model will presumably be trivial—even finite such models may be affected because the standard language schema has infinitely many schematic letters. So stipulating $\neg\mathrm{Rel}(A,x) \leftrightarrow \mathrm{Rel}(A,1-x)$, for $\leftrightarrow$ detachable, does not seem justifiable. The same obviously holds for stipulating $\neg\mathrm{Rel}(A,x) \leftrightarrow \mathrm{Rel}(\neg A,x)$. Priest's view on untruth [20, §4.9] agrees with this conclusion.

While I have argued against a certain way of reducing the infinity of $\neg$Rel-concatenations, a different way to reduce infinite concatenations to finite ones seems harmless. What I mean is requiring $\neg\mathrm{Rel}(A,x) \leftrightarrow \mathrm{Rel}(A,1-x)$, or

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[31]Formulas in which Rel is iterated are required. Remember indeed that we are devising a theory about the semantics of English. The **LP**-semantics is the part that handles the traditional logical terms.

[32]One of them still holds if $A \leftrightarrow B$ is the 'material equivalence' $(\neg A \vee B) \wedge (A \vee \neg B)$.

[33]I am considering classical models. Yet, going paraconsistent does not remove the problem.

even the slightly stronger $\neg\mathrm{Rel}(A,x) \leftrightarrow \mathrm{Rel}(\neg A,x)$, whenever A is itself a pure Rel-formula—a formula in which every non-logical symbol occurs within a first argument of Rel. The result of this change is that we shall still have the eight A-type models from the *R-representation*, but that there will be no further A-type models. Inconsistencies will spread upwards in nested formulas. Thus A-type-1.2 models will verify $\mathrm{Rel}(\mathrm{Rel}(A,1),1) \wedge \neg\mathrm{Rel}(\mathrm{Rel}(A,1),1)$ as well as all other entailed inconsistencies, such as $\mathrm{Rel}(\neg\mathrm{Rel}(A,1),1) \wedge \neg\mathrm{Rel}(\neg\mathrm{Rel}(A,1),1)$, but no further inconsistencies will be verified by any A-type-1.2 models. In as far as I can see, this approach still prevents an extended Liar from generating triviality.

Is the reduced approach to be preferred over the one that allows for infinite concatenations of consistency choices? This depends on what one wants. I prefer the infinite concatenation. The reason is that I am convinced that one can talk consistently about many an inconsistent theory T_1 and that, even if the 'theory', say T_2 in which one describes T_1 *needs* to be inconsistent, then, more often than not, there will be a consistent T_3 that describes T_2. As I see it, whatever T_1, there is always a sufficiently high i such that T_i is consistent. Consider again A-type-1.2 models. Some of them verify an infinity of contradictions made up from A, $\mathrm{Rel}(A,x_1)$, $\mathrm{Rel}(\mathrm{Rel}(A,x_1),x_2)$, $\mathrm{Rel}(\neg\mathrm{Rel}(A,x_1),x_2)$, $\ldots$, with $x_1,x_2,\ldots \in \{0,1\}$. However, some model M_1 verifies all of the following formulas[34]

$$\begin{array}{cc}
\mathrm{Rel}(\neg\mathrm{Rel}(A,0),1) & \neg\mathrm{Rel}(\neg\mathrm{Rel}(A,0),0) \\
\mathrm{Rel}(\mathrm{Rel}(A,1),1) & \neg\mathrm{Rel}(\mathrm{Rel}(A,1),0) \\
\mathrm{Rel}(\mathrm{Rel}(A,0),0) & \neg\mathrm{Rel}(\mathrm{Rel}(A,0),1) \\
\mathrm{Rel}(\neg\mathrm{Rel}(A,1),1) & \neg\mathrm{Rel}(\neg\mathrm{Rel}(A,1),0)
\end{array}$$

and no contradiction outside $\mathrm{Rel}(A,1) \wedge \neg\mathrm{Rel}(A,1)$. If A has thickness 0 and nothing requires that there is an inconsistency in formulas of thickness 2 or more, then my preference would be to go for the consistent description of thickness 2.

The reader may note that the formulas of the different thicknesses belong to the same theory, viz. the theory describing a specific **LP**-model. That is correct. However, every formula that holds in the model and the thickness of which is i or less, is entailed by the set of formulas of thickness i that hold in a model. So, given the considered reduction, this set can be considered as a theory about the model. The general claim, in terms of $\{T_1, T_2, \ldots\}$ concerns also cases where these T_i are not themselves fragments of a single theory.

Priest will presumably prefer the aforementioned reduction to thickness 1 because of its similarity with the behaviour of negation within **LP**-formulas (of thickness 0). Presumably, he will disagree with my claims on the existence of a consistent T_i provided i is chosen large enough. Indeed, one can construct a Liar in any T_i. The day that I would be interested, I would do so and have my consistent description of it in T_{i+1}. Is that the true and final description? No. Will it do in all circumstances? No. Our knowledge is in a patchwork state anyway. And one can always accommodate sensible problems by moving up to the next theory. Or so I think.

Top-down spreading of inconsistency. A few paragraphs ago, I argued for the independence of two *loci* of inconsistency, inconsistency of formulas that contain typical terms from the semantic metalanguage and inconsistencies in

[34]Each formula to the left is entailed by the one to the right in Priest's understanding.

other formulas. Priest agrees that there is no equivalence between both, but still holds that there is an implication.[35] In terms of Rel and a detachable implication, Priest accepts (6) but rejects (7).

$$\neg\mathrm{Rel}(A,x) \to \mathrm{Rel}(A,1-x) \tag{6}$$
$$\mathrm{Rel}(A,x) \to \neg\mathrm{Rel}(A,1-x) \tag{7}$$

Needless to say, I shall not defend (7) either. Inconsistent models verify for some A, $\mathrm{Rel}(A,1)$ as well as $\mathrm{Rel}(A,0)$. So affirming $\mathrm{Rel}(A,x) \to \neg\mathrm{Rel}(A,1-x)$ comes to spreading inconsistency without need. I think to have sufficiently clarified this before.

But does the same not hold for (6) too? Obviously, if A is a linguistic entity of the right sort, in the present case a closed formula, we have

$$\mathrm{Rel}(A,1) \vee \mathrm{Rel}(A,0)\,. \tag{8}$$

So, if the model verifies $\neg\mathrm{Rel}(A,1)$ *consistently*, it is bound to verify $\mathrm{Rel}(A,0)$. However, some models verify $\neg\mathrm{Rel}(A,1)$ inconsistently—they verify $\mathrm{Rel}(A,1) \wedge \neg\mathrm{Rel}(A,1)$. In the presence of (7), this entails $\mathrm{Rel}(A,1) \wedge \neg\mathrm{Rel}(A,1) \wedge \mathrm{Rel}(A,0)$ and hence comes to spreading inconsistency. So the reasons for rejecting (7) seem to apply just as well to (6); the first spreads inconsistency 'upwards', the second does it 'downwards'.[36]

In discussing the infinite hierarchy, I linked statements in which Rel occurs to our method to devise models, or to define models, whereas A was an arbitrary statement. Some will judge that, if our method to devise models goes wrong, then a general disaster should happen. This has a certain appeal. The method may very well cause triviality. So in that case (6) is quite all right. I doubt, however, that Priest will take this stand. The blazon of paraconsistency is precisely inconsistency without triviality—apologies for the infantile metaphor. So the last argument for (6) is weak.

Describing the semantics Moving to the relational semantics generates a problem to identify models and groups of models within the **LP**-language. Take the *O-representation*. It seems to depict three possibilities with respect to a formula A: consistent truth, inconsistency, and consistent falsehood. Of course, one can say so in English. One can also say that A is true and not false in A-type-1 models, both true and false in A-type-2 models, and false and not true in A-type-3 models. What do these English statements mean? Given the kind of statements we are considering, these meanings have to be spelled out by logicians. That is precisely what devising the logic **LP** and describing its semantics is about. Remember the quote from the beginning of this section: "the language for which the semantics has been given is not English, since these notions obviously are expressible in English."

The distinction between the three A-types is rendered by $\mathrm{Rel}(A,1)\wedge\neg\mathrm{Rel}(A,0)$, $\mathrm{Rel}(A,1) \wedge \mathrm{Rel}(A,0)$ and $\neg\mathrm{Rel}(A,1) \wedge \mathrm{Rel}(A,0)$ respectively. The second one of these does its job perfectly—neglect for a moment formulas of thickness 2 and

[35] The matter may be phrased in terms of truth, falsity and untruth.

[36] Maybe Priest means (8) when he writes (6). In chapters stemming from the first edition of *In Contradiction* [20], $\leftrightarrow$ sometimes denotes a non-detachable implication and sometimes a detachable one; (7) spreads inconsistency on both readings.

more. It identifies unambiguously models in which A is inconsistent. The two other 'descriptions' fail to do so. $\mathrm{Rel}(A,1) \wedge \neg\mathrm{Rel}(A,0)$ intends to describe A-type-1 models, but $\mathrm{Rel}(A,1) \wedge \neg\mathrm{Rel}(A,0)$ is also true in some A-type-2 models, viz. in A-type-2.2 models and A-type-2.4 models. Whichever convention one follows to handle formulas of higher thickness—the conventions were discussed before—moving there does not help to identify A-type-1 models. Adding formulas of thickness 1 that are verified by all A-type-1 models does not help either; these are also verified by A-type-2.2 models and A-type-2.4 models.

Incidentally, pointing to the graphic representation of the model would not help either. We have seen that the representations are not unambiguous; different interpretations are possible. So one should be careful and require a clear description of which properties of models are represented by the representations. This includes a description of the models.

The complaint that Priest cannot identify the A-type-1 models and the A-type-3 models suggests that others can. One readily thinks of logicians that have no objections to explosive negations, like classical logicians or intuitionists. Priest has often pointed out [19, §6.3] [20, §20.4] [21, §10.4.2] that (what I briefly call) explosive logicians cannot *guarantee* consistency, or *force* consistency, or *rule out* inconsistency. Affirming the classical negation of A, the explosive logician would be committed to everything if she were to affirm A as well. And Priest continues to point out that the paraconsistent logician can do exactly the same thing, viz. by affirming $A \to \bot$, in which $\to$ is relevant, and hence detachable, and $\bot$ is the *falsum*, for which $\bot \to B$ is valid. Moreover, Priest invokes this possibility [21, §10.4.2] in a case similar to the one under discussion, which is to identify A-type-1 models and A-type-3 models. So let us try to complete the description of the A-types in those terms.

A first try might consist in modifying the models in such a way that $\neg A \to \bot$ holds in A-type-1 models and that $A \to \bot$ holds in A-type-3 models. But there is a problem. As we have three types of models, the following formula now holds: $\neg A \to \bot \vee (A \wedge \neg A) \vee A \to \bot$. So we can define a Liar formula Z such that $\mathrm{Rel}(Z,1) \leftrightarrow \mathrm{Rel}(Z \to \bot, 1)$. Z-type-1 models and Z-type-2 models both verify $\mathrm{Rel}(Z,1)$. But then $\mathrm{Rel}(Z \to \bot, 1)$ as well as $\mathrm{Rel}(\bot, 1)$ hold in these models.[37] In Z-type-3 models holds $\mathrm{Rel}(Z \to \bot, 1)$. So also $\mathrm{Rel}(Z,1)$ as well as $\mathrm{Rel}(\bot, 1)$. So triviality results.

It is rather straightforward that the reasoning is mistaken. In order for A-type-1 models to verify arrow formulas, we need to update the semantics to a worlds semantics with a binary, or on Priest's present view ternary, accessibility relation, and we would have to adjust Rel accordingly. It is rather easy to see, however, that $\neg A \to \bot$ will not hold in the real world G of some A-type-1 models and that $A \to \bot$ will not hold in world G of some A-type-3 models. This is of course as it should be, because otherwise we would have $A \vDash \neg A \vee (\neg A \to \bot)$ as well as $\neg A \vDash A \vee (A \to \bot)$. Now this is a touchy matter. Indeed, someone who combines the paraconsistent **LP**-negation $\neg$ with the classical negation $\sim$ definitely wants both $A \vDash \neg A \vee {\sim}\neg A$ as well as $\neg A \vDash A \vee {\sim}A$.[38] And someone who combines the paraconsistent **LP**-negation $\neg$ with a *detachable* classical implication $\supset$ will definitely want to have $A \vDash \neg A \vee (\neg A \supset \bot)$ as well as

[37] The Liar formula itself does not entail triviality because Contraction (or Absorbtion) is not valid for the arrow. Triviality is engendered by (i) the decision that arrow-bottom formulas hold in the A-consistent models and (ii) the fact that the arrow is detachable.

[38] Where $\sim$ is intuitionistic negation, the expressions hold for finitistic A.

$\neg A \vDash A \vee (A \supset \bot)$.[39]

Priest is willing to suppose counterfactually that classical negation makes sense [19, §6.3] and derives from this supposition that the classical logician—and forcibly also someone who takes classical negation to be sensible in certain contexts—can link affirming an inconsistency to a commitment to triviality, and actually cannot do more than this. This conclusion is clearly correct. If classical negation $\sim$ makes sense, introducing it in the *O-representation* is sufficient to identify the three types of models flawlessly. I mean, identify the models in terms of what they verify. Here is what the A-types of the *O-representation* verify if $\sim$ makes sense.[40]

A-type-1	A-type-2	A-type-3
A	A	$\sim A$
$\sim\neg A$	$\neg A$	$\neg A$

Obviously, every **LP**-model that is of two of the types is trivial: it assigns 0 as well as 1 to every formula. Priest claims that dialetheists can, just as much as explosive logicians, turn a commitment to inconsistency into a commitment to triviality. In view of what precedes, I do not see that this can be done in the present case.

I am *not* arguing that explosive logicians can do something that dialetheist cannot do. Explosive logicians separate theories from each other and separate languages from each other.[41] They have to pay a high price and the limitative theorems are part of that price. Dialetheists want to have one big theory, containing all knowledge in unified form and phrased, at least in principle and ultimately, in the vernacular. This, it seems to me, is the reason why they cannot identify the three A-type models. So $A \to \bot$ allows one, just as much as classical negation if it makes sense, to connect a formula to triviality,[42] but the dialetheist cannot use it in the present context because the unrestricted language allows for a Liar that engenders triviality.

Can one not apply the arrow-*falsum* method about the models rather than within the models? Is it possible to identify A-type-1 models by stating, for example, $\mathrm{Rel}(A, 0) \to \bot$ instead of letting the models verify $A \to \bot$, which comes to stating $\mathrm{Rel}(A \to \bot, 1)$. It seems to me that the question cannot be answered in the positive. Indeed, if $\mathrm{Rel}(A, 0) \to \bot$ is true about A-type-1 models, then $(\mathrm{Rel}(A, 0) \to \bot) \vee \mathrm{Rel}(A, 0)$ is true about all three types of models. But then, it seems to me, this must be a truth about Rel, because the semantics is intended to be the semantics of the vernacular. The statement "either A is

[39] In the presence of $\bot$, with its intended meaning that $\bot \vDash A$ holds, $A \supset \bot$ defines classical negation in case the implication is classical and defines intuitionistic negation in case the implication is intuitionistic. In the absence of $\bot$, it is possible to have a detachable classical or intuitionistic implication without having the disadvantages of an explosive negation. This is illustrated by work of da Costa [10, 11, 12], Jaśkowski [13], and others including myself. Priest and other dialetheists (and most relevantists) consider this a minor detail.

[40] Obviously $\sim\neg A \vDash A$ and $\sim A \vDash \neg A$. Moreover, the matter may be phrased in terms of classical implication and bottom. I leave it to the reader to figure out this as well as the intuitionistic case.

[41] It seems to me that the separation between object-language and metalanguage can be avoided, at least to some extent, provided one is willing to accept that theories are formulated in restricted fragments of a language.

[42] The difference, viz. that $A \vee \sim A$ is valid whereas $A \vee (A \to \bot)$ is not, is immaterial for this purpose.

false or A is false implies everything" is not a true English statement.[43]

Consistency I now come to the most difficult point. In Chapters 16 and 18 of *In Contradiction*, Priest makes a number of claims that on the one hand resolve some of the questions I raised in the present section, but on the other hand seem to turn some of the other questions into insurmountable obstacles. In order to state the technicalities involved in Priest's point, I would have to explain the notion of collapsed models of Zermelo-Fraenkel set theory, **ZF**, and I do not have room to do so here. So I refer the reader to those chapters for details and for checking some of my claims. Moreover, let us suppose that there is a way to get around Facts 4–10, for otherwise the chapters are largely pointless anyway.

Let me start with a quote. "One may think of the metatheory of the logic, including the appropriate soundness and completeness proofs, as being carried out (as we know it can be) in **ZF**. According to the model-theoretic strategy, the results established in this way can perfectly well be taken to hold of the universe of sets, paraconsistently construed. The paraconsistent logician can, therefore, simply appropriate the results." [20, p. 259]. So, according to Priest, the metatheory of **LP** can be carried out in **ZF**. Next, he claims that there is a paraconsistent set theory, call it **PZF**, that is obtained by 'collapsing' the **ZF**-models. Everything that holds in the original **ZF**-model, holds in the collapsed model, but more formulas, possibly some inconsistent ones, may hold in the collapsed model. The effect of the collapse is that the metatheory, which was phrased in **ZF**, is now phrased in **PZF**. It seems to me that there may be some loose ends, but let us suppose that Priest is right.

One still has to show that the theory has the same force. Phrased in **ZF**, the metatheory had material implication as its implication—write it as $\neg A \vee B$ for perspicuity. In **PZF**, however, Disjunctive Syllogism is invalid. Priest goes on to point out that the metatheory is consistent and that, reasoning in terms of **LPm**, which is basically an inconsistency-adaptive logic, "the disjunctive syllogism is perfectly acceptable provided the situation is consistent. Provided we do not have" $M \Vdash A$ and $M \nVdash A$, we can get from $M \Vdash A$ and $(M \nVdash A) \vee (M \Vdash B)$ to $M \Vdash B$.[44] It is worth spelling out the underlying reasoning and I shall spell it out in my words. (i) From $M \Vdash A$ and $(M \nVdash A) \vee (M \Vdash B)$ follows $((M \Vdash A) \wedge (M \nVdash A)) \vee (M \Vdash B)$. (ii) If interpreting the premises as consistently as possible allows one to consider the contradiction $(M \Vdash A) \wedge (M \nVdash A)$ as false, one may conclude to $M \Vdash B$. Let me rephrase this in semantic terms.[45] Consider the *models* that verify such expressions as $M \Vdash A$, the *models* of the **LP**-semantics.[46] If no minimally inconsistent such *model*[47] verifies the contradiction $(M \Vdash A) \wedge (M \nVdash A)$, then all minimally inconsistent models verify $M \Vdash B$.

[43] And fortunately so. If it were true, then, given what it is talking about, it should be valid. So it should be true in every model, which it is not.

[44] The expression $M \Vdash A$—Priest actually writes $\mathcal{I} \Vdash \alpha$—is read as "$A$ holds in M" by Priest. The same expression occurs as classical terminology in the very first table of the present section.

[45] The easiest way to understand the rest of the paragraph is to read it as a statement in classical terminology—so if a model both verifies and falsifies the same formula, then it is trivial.

[46] If the **LP**-semantics is indeed part of the semantics of English, then those *models* are simply the models from the **LP**-semantics. Yet, I shall keep italicizing "models" for perspicuity.

[47] A model of Γ is minimally inconsistent iff the set of inconsistencies that hold in it is not a proper superset of the set of inconsistencies that hold in another model of Γ.

Step (ii) is the adaptive one. The motivation for it is that inconsistencies are normally false, so that we may consider them as false unless and until proven otherwise. In some cases, as the one Priest invokes here, the premises are consistent. So the consequence set coincides with the **CL**-consequence set: the minimally inconsistent **LP**-models of the premises are the consistent ones and these are the **CL**-models.[48]

The argument supposes that **ZF** is consistent, a claim that is obviously not provable in any absolute way. Of course, Priest may argue that he is not supposing more than the classicist is supposing, for example when the latter devises the metatheory of **CL**. That is a sensible defense for a dialetheist, but obviously not more than a defense. And, as argued before, stating that a model is consistent remains a problem, even if it does not prevent one from applying an inconsistency-adaptive logic. There are, however, other problems, of which I shall mention the most constructive one.

As Priest notes, that both $M \Vdash A$ and $M \Vdash \neg A$ hold does not make the semantics inconsistent, but $M \Vdash A$ and $M \nVdash A$ does. In which way does $M \Vdash A$ relate to other semantic expressions? It is typical that Priest reads (what I write as) $M \Vdash A$ as "A holds in M" and not, for example, the rather common "M verifies A". As Priest claims $\Vdash$ to behave consistently, he apparently wants $M \Vdash A$ whenever $\mathrm{Rel}(A, 1)$. However, when does he want $M \nVdash A$? The easiest approach would go as follows, where Rel denotes truth-in-M.

$$\begin{array}{rcl} \text{If } \mathrm{Rel}(A,1) & \text{then} & M \Vdash A \\ \text{If } X & \text{then} & M \nVdash A \end{array}$$

But what should X be? Clearly X=$\mathrm{Rel}(A, 0)$ is not all right, because then $\mathrm{Rel}(A, 1) \wedge \mathrm{Rel}(A, 0)$ would cause the semantics to be inconsistent, which is not what Priest wants. Setting X=$\mathrm{Rel}(A, 0) \wedge \neg\mathrm{Rel}(A, 1)$ would be all right *provided* Rel-expressions behaves consistently. But the Rel-version of the semantics was precisely introduced in order to allow inconsistent Rel-expressions and thus to escape the extended Liar provoked by the functional version of the semantics. So I see no X that would do. A wholly different possibility would be to define

$$M \Vdash A \quad \text{iff} \quad \mathrm{Rel}(A,1) \tag{9}$$

in which "iff" is a non-contraposable equivalence—if it were contraposable, then $\mathrm{Rel}(A, 1) \wedge \neg\mathrm{Rel}(A, 1)$ would imply that $\Vdash$ behaves inconsistently. But how can one be sure that (9) forces $\Vdash$ to behave consistently? Suppose that $\mathrm{Rel}(A, 1)$ and $\mathrm{Rel}(A, 0)$. What prevents both $M \Vdash A$ and $M \nVdash A$ from being true?

Maybe Priest would argue that one should select the minimally inconsistent *models* of the **LP**-semantics—see above for the italics. In those *models* $\Vdash$ indeed behaves consistently. Nevertheless, the argument might be too swift.

As we have seen after introducing the semantic Liar (SL), the semantics of **LP** is not consistent. It verifies $\mathrm{Rel}(L, 1) \wedge \neg\mathrm{Rel}(L, 1)$. But then how can the semantics have been carried out in **ZF**? The more as the Disjunctive Syllogism was explicitly stated, in the original paper [27] as well as in the reworked version [20], to be invalid and that this was done to avoid triviality. The 'classical

[48] In more interesting cases the premises require certain but not all contradictions to be true. The **CL**-consequence set is then trivial, but the inconsistency-adaptive consequence set is, a few odd premise sets aside, non-trivial and moreover considerably richer than any consequence set defined by a paraconsistent Tarski logic. See the literature for details [3].

recapture' is indeed possible because the semantics can be carried out in **ZF**. But this is only possible because classicists are willing to operate within the patchwork. More specifically, and as stated before, they are willing to handle a restricted language in which no Liar can be formulated. An unrestricted language, however, seems to trivialize the semantics as phrased within **ZF**. If that is correct, a full recapture is excluded.

That the semantics is inconsistent does not prevent inconsistency-adaptive logics from efficiently doing their job. The Disjunctive Syllogism cannot be applied to some Rel-formulas because some of them are inconsistent. Yet Disjunctive Syllogism may be applied to other Rel-formulas (and perhaps to all $\vDash$-formulas), provided they are consistent in all minimally inconsistent *models*—basically, provided no extended Liar can be defined in terms of them.

5 In Conclusion

Allow me to repeat that my aim was not to argue, but to present some potentially interesting questions, some of which have hopefully not a standard or other ready answer. And, in case I have raised some interesting questions, that I leave it to Priest to select the ones he considers ripe for an answer.

The central difficulties outlined, especially in Section 4, seem to turn around a specific mixture of ingredients: a unified and monolithic knowledge system, a view on natural language and its logical terms as changeable but nevertheless formal-system-like, the choice of natural language, so conceived, as the language of the knowledge system, and the effects of Liars in such a scenery. Most dramatic seem the nasty effects of Liars on the semantics, notwithstanding the paraconsistent character of the environment. Actually, two different effects seem crucial.

The first effect is the inconvenience—it is not much more—that Liars apparently cause the semantics to be unavoidably inconsistent. The matter is related to the fact that several central metatheoretic concepts—properties of formulas, sets of formulas, models, validity, the consequence relation, . . . —turn out to be rather remote from the usual concepts as well as from the concepts implicit in Priest's writings. Given the complexity of the scenery, there may be changes that neutralize the inconvenience as well as the distance to the vernacular. And there is the alternative, to approach the inconsistent semantics by inconsistency-adaptive means. This approach seems free of foreseeable difficulties. Yet, the approach requires one to rework the semantics and the proof of its theorems. I see no way to 'translate' the version in terms of **ZF** and the set theoretic **LP**-semantics.

The second effect concerns the possibility to identify models and groups (or types) of models and to express certain distinctions that are clearly there. One example is that, even if all sets of formulas are non-trivial in the sense of Fact 8, then nevertheless some Γ are trivial whereas others are not. The latter negation is different from the one in Fact 8. Means to remove this second effect seem in the same range as means to remove the first effect, but a single sweep might not be at hand.

Independent of the questions I raised and of their appropriateness, I would like to express my great appreciation for Graham's work. We have been raised in very different intellectual environments and our philosophical views are far apart, but contacts with Graham and with his writings have always been challenging and inspiring. It was a pleasure to find common tenets. It was a greater pleasure and a source of deeper insight to find points of disagreement, to consider them and to discuss them.

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