# 6. Australian theories: Messing with Logic

In this chapter, we look at two Australian theories that purport to solve the problems associated with vagueness by making some distinctive changes to logic. Both of them take the supervaluational theory as their starting point, but they develop the idea in surprising and interesting ways.

## 6.1. Burgess and Humberstone

Burgess and Humberstone (1987) suggest a semantics for vague sentences that draws partly on the supervaluational semantics suggested by Fine (1975), and partly on the tradition established by the degree theorists. The aim of the theory is to keep the law of non-contradiction (LNC), and the law of double negation elimination (DNE), but not the law of excluded middle (LEM). They argue, convincingly enough, that vagueness does seem to threaten LEM, but not the other two.

A *model*, in their theory is a set of points, a reflexive, transitive, anti-symmetric relation on those points ≥, and a valuation function that, within certain constraints, assigns to each atomic proposition two exclusive (but not necessarily exhaustive) sets of points. Intuitively, the two sets are the extension and the anti-extension of the atomic proposition. So an atomic proposition $p$ is true at a point $x$ iff $x$ is in the extension of $p$, false at $x$ iff $x$ is in the anti-extension of $x$, and undefined otherwise. Say that a point $x$ is complete iff for any atomic proposition $q$, $x$ is in the extension or anti-extension of $q$. There are two constraints on the valuation function.

*Persistence*: For all points $x$, $y$ and atomic propositions $p$, if $y \geq x$ then if $x$ is in the extension of $p$ then so is $y$ and if $x$ is in the anti-extension of $p$, so is $y$.

*Two-Sided Resolution*: For all points $x$ and atomic propositions $p$, if $p$ is undefined at $x$ then there are points $y$, $z$ such that $y, z > x$ and $p$ is true at $y$ and false at $z$.

So far this is all familiar from Fine's supervaluational account. And the truth conditions for negation and conjunction should also seem familiar.

> $\neg A$ is true at $x$ iff $A$ is false at $x$.
> $\neg A$ is false at $x$ iff $A$ is true at $x$.
> $A$ & $B$ is true at $x$ iff $A$ is true at $x$ and $B$ is true at $x$.
> $A$ & $B$ is false at $x$ iff $(\forall y \geq x)(\exists z \geq y)$($A$ is false at $z$ or $B$ is false at $z$).

The difference appears in how Burgess and Humberstone (hereafter, BH) handle of disjunction. They offer the following rules:

> $A \vee B$ is true at $x$ iff $A$ is true at $x$ or $B$ is true at $x$.
> $A \vee B$ is false at $x$ iff $A$ is false at $x$ and $B$ is false at $x$.

As BH note, the truth conditions for disjunctions are more like those offered by degree theorists than those offered by supervaluationists like Fine. Assuming that English corresponds to a point that is in neither the extension nor the anti-extension of *Louis is bald*, then we get the nice result that all instances of LNC are true, while some instances of LEM are neither true nor false. Thus BH propose their theory as a systematic way of handling the logical anomalies generated by vagueness.

BH's theory has many interesting aspects, and the rough sketch I have given here glosses over some important subtleties in their presentation. There are, however, three important objections to using their theory as a solution to the puzzle posed by LNC and LEM. (And since these objections are not removed by a more careful formulation of their theory, the rough sketch I have made here should suffice.) The first objection, which BH note, is that the logic contains some failures of congruentiality. The second is that it seems hard to explain on their theory why sentences like LEM sometimes *do* seem true, despite the presence of vagueness. The last reason is that they cannot explain the apparent acceptability of some complex contradictions.

Let $A$ and $B$ be any formulae such that $A \vdash B$ and $B \vdash A$ are provable in a given logic. Let $D$ be a formula that has $B$ as a subformula, and let $C$ be a formula which can be generated by replacing some occurrences of $B$ with $A$. The logic is congruential iff, whenever $C$ and $D$ are chosen in this way, $C \vdash D$ is provable. In BH's logic, a sequent is valid iff it is truth-preserving at all points in all models, and BH provide natural deduction rules that are sound and complete with respect to this definition of validity, so we can safely call valid sequents provable. Most many-valued logics will have failures of congruentiality. The provability of $A \vdash B$ and $B \vdash A$ just shows that these two formulae are true in the same circumstances. This need not imply that the formulae are *false* in the same circumstances, so, for example, $\neg A \vdash \neg B$ might not be valid. So, as BH note, the fact that there are failures of congruentiality in their system should come as no surprise. One such failure is that we can prove $p \,\&\, \neg p \dashv\vdash \neg(p \vee \neg p)$, since neither formula is ever true. However, we cannot prove $\neg(p \,\&\, \neg p) \vdash \neg\neg(p \vee \neg p)$, since the LHS is a logical truth, but the RHS is equivalent to LEM. That there are some failures of congruentiality like this in a system that allows for truth-value gaps is not surprising. What is surprising is just how widespread these failures are. Note that $A \,\&\, A$ is false at $x$ unless there is some point $y$ ($y \geq x$) where $A$ is true. This means that it is possible for $A \,\&\, A$ to be false when $A$ is undefined. When $A$ is the formula $\neg(p \vee \neg p)$, and $p$ is undefined, then $A$ will be undefined, but $A \,\&\, A$ will be false. Conversely, $\neg(A \,\&\, A)$ will be true, even though $\neg A$ is undefined, so $\neg(A \,\&\, A) \vdash \neg A$ will not be valid. Since we have, in general, $A \dashv\vdash A \,\&\, A$ this is another failure of congruentiality. As BH notes, this particular failure of congruentiality does look like a cost of the system.

There are occasions, in particular when we are trying to apply formal methods in social sciences, when it seems worthwhile to abstract away from vagueness. To take just one prominent case, in most macroeconomics textbooks there will be a warning that the traditional division of goods into investment goods and consumption goods is fraught with vagueness, cars are often mentioned as being a penumbral case, but this is explicitly taken to be consistent with the assumption that all goods are investment goods or consumption goods. For example, Keynes (1936: 59-63) explicitly mentions that the line between investment goods and consumption goods is vague, but then proceeds to run a technical argument that clearly has as a premise that all goods are investment goods or consumption goods[1]. And the common distinction between goods and services is attended by similar vagueness, I guess takeaway food is these days the clearest penumbral case, but economists are frequently willing to divide all sales into purchases of goods and purchases of services, simply because that exhausts the possibilities.

[1] In an earlier draft of *The General Theory* (Keynes 1934/1973) this premise is more explicit: "[F]inished goods fall into two classes according as the effective demand for them depends predominantly on expectations of consumers' demand [i.e. they are consumption goods] or partly on expectations of consumers' demand and partly on another factor conveniently summed up as the rate of interest [i.e. they are investment goods]." (428) Two pages later Keynes explicitly acknowledges that any division of real-world goods into categories such as these will be more than a little 'arbitrary', but claims that anything that is true on any arbitrary way of drawing the line is after all, true. It is unfortunate, but surely insignificant, that this premise was not left explicit in the final draft. Keynes's position, of acknowledging the distinction to be vague but reasoning as if a line had been drawn is repeated in many economics texts.

The epistemic merits of building a scientific discipline on vague terms while assuming the logic appropriate to that discipline is classical could be debated, but that is not our topic. The fact remains that various social scientists are prepared to *talk* in a certain way, accepting disjunctions even when they know they could not, in principle, have reason to accept either disjunct. This is an important piece of data that theorists of vagueness must explain. In these cases, it seems appropriate to adopt conventions that make sentences like LEM true. There are differences of opinion on this question, but it seems like the adoption of such conventions has been a benefit to the development of the social sciences in the last hundred or so years. It is hard to see how this could be justified if BH have provided the correct semantics for vague languages. These conventions apparently license assertions that are simply not true. So I take it to be a cost of BH's theory that it cannot explain why we accept instances of LEM in formal contexts, even when we would not accept either disjunct because of the attendant vagueness.

Finally, BH's theory is completely unable to explain why we are prepared to accept some compound contradictions. Say that a simple contradiction is a sentence such as ⌜$a_1$ is *F* and $a_2$ is not *F*⌝, where *F* is an atomic predicate, and expressions $a_1$ and $a_2$ which are known by all parties to the conversation to co-refer. Following Williamson, I argued in chapter 1 that the only such sentences that are accepted by speakers are idiomatic. However, when we stop dealing with simple contradictions, we find the data becomes somewhat more problematic. Let a compound contradiction be any contradiction that is not a simple contradiction. It seems that vagueness can cause us to accept some compound contradictions. I think that (1), for example is a legitimate, if slightly long-winded, way to communicate that Louis is a penumbral case of baldness.

(1) It is not the case that Louis is bald, but nor is it the case that he is not bald.

Assuming the last *not* in (1) can be viewed as a sentential connective, then (1) is a contradiction. Even if it cannot be so viewed, (1) might still count as being a contradiction under a more liberal definition of what constitutes a contradiction. Yet it seems like a perfectly accurate thing to say about Louis. Any infelicity associated with it is due to its length, not its apparent falsehood. Unlike *Louis is bald and Louis is not bald*, or phrases like "He is and he isn't", (1) does not behave like an idiom. Replace the pronoun with a term known to refer to Louis, such as "His Majesty", and the message conveyed by (1) stays the same.

(1a) It is not the case that Louis is bald, but nor is it the case that His Majesty is not bald.

So some contradictions, such as (1) can be properly asserted on account of vagueness, and this is not due to their being idiomatic. This striking piece of data needs to be explained.

The problem for BH's theory is not that (1) is true and that their theory says that it is false. (Recall that BH's theory says that all contradictions, including compound contradictions, are false.) Since (1) is a contradiction it is surely false, so to that extent the theory provides the right answer. The problem is more methodological than that. Once we have accepted that our initial intuitions about sentences like (1) can be mistaken, and not because these sentences are idioms, this raises serious doubts about intuitions about similar sentences. In particular, it seems rather unstable to base one's theory on the 'data' provided by the intuition that instances of LEM fail due to vagueness, while 'explaining away' the very similar intuition that (1) is true.

So far this might seem more like an awkwardness than an objection, but it can be sharpened into a serious challenge to BH's theory. The problem here is not merely guilt by association; since some intuitions about the truth value of vague compound sentences is unreliable so are others. Rather, the problem is that everyone must explain why we think, falsely, that (1) is true. The problem is not that BH cannot do this. As we will see in chapter 8, there is a rather attractive theory of the pragmatics of compounds that lets them do just this. The problem is that the best explanation of why we think (1) is true also explains why we think instances of LEM are false, even if instances of LEM are in fact all true. So the problem is not just that BH accept some intuitions as data and explain away others, it is that the best way to explain away the problematic intuitions works equally well to explain away the intuitions they take to be data. To my mind, this seriously undermines the argument for their theory.

## 6.2. Subvaluations

In the standard version of supervaluational theory, a sentence is held to be true iff it is true on *all* precisifications. This approach leads to truth value *gaps*, because some sentences are neither true nor false. Dominic Hyde pointed out that if we keep the machinery of supervaluationism, but say that a sentence is true iff it is true on *some* precisifications, then we get an interesting new theory that has truth value *gluts*. Hyde calls this theory subvaluationism. Subvaluationism produces truth value gluts because when a sentence is true on some precisifications and false on others, it will be both true and false according to the subvaluationist. Hyde's main argument is that subvaluationism is as good a theory as supervaluationism. Since we have already argued that supervaluationism is not a successful theory of vagueness, the importance of this relative claim is perhaps questionable, but we shall investigate it, as well as the more interesting question of whether subvaluationism is a plausible theory of vagueness.

As Hyde notes, there are several traditions in which vagueness is thought to produce truth value gluts. This idea forms a central theme in various formal versions of Marxism. (Well, if the Marxists believe it then it *must* be true.) More seriously, there has been a lot of work done by Brazilian logicians developing glutty theories of vagueness. These theorists were not trying to develop subvaluational theories *per se*, but Hyde shows that subvaluationism captures most of the intuitive motivations for their theories.

Hyde's main argument for the relative quality of subvaluationism (i.e. that it is as good as supervaluationism) rests on drawing several parallels between the supervaluational and subvaluational theories. To set out these parallels, Hyde introduces a multiple-consequence entailment relation. This is a fairly familiar logical device. In this notation, we have $\Gamma \vdash \Delta$ iff whenever all elements of $\Gamma$ are true, then at least one element of $\Delta$ are true. Obviously, a familiar single-consequence entailment $\Gamma \vdash A$ holds iff the 'multiple-consequence' relation $\Gamma \vdash \{A\}$ holds. Using this terminology, we can define a few properties of entailment relations, as follows:

*Complete*: $\vdash$ is complete iff $\forall A$: $\varnothing \vdash A, \neg A$
*Consistent*: $\vdash$ is consistent iff $\forall A$: $A, \neg A \vdash \varnothing$
*Paracomplete*: $\vdash$ is paracomplete iff $\exists A, B$: $B \nvdash A, \neg A$
*Paraconsistent*: $\vdash$ is paraconsistent iff $\exists A, B$: $A, \neg A \nvdash B$

As Hyde points out, the entailment relation $\vdash_{SpV}$ recognised by supervaluationism is paracomplete. And the entailment relation $\vdash_{SbV}$ recognised by subvaluationism is paraconsistent. These two facts follow from the following two non-entailments.

$$A \vee \neg A \nvdash_{SpV} A, \neg A$$
$$A, \neg A \nvdash_{SbV} A \wedge \neg A$$

In supervaluationism, the truth of one or other disjunct does not follow from the truth of the disjunction. As it is often put, the principle of *subjunction* fails in this logic. In subvaluationism, the truth of a conjunction does not follow from the truth of the two conjuncts. (If $A$ is true on some precisifications and false on others, then $A$ and $\neg A$ will both be true, but $A \wedge \neg A$ is true on no precisification, so it is not true.) As it is often put, the principle of *adjunction* fails in this logic. Despite these results, both theories produce logics that parallel classical logic in some respects. Letting $\vdash_{CL}$ stand for the classical entailment relation, and $A$ and $B$ stand for any formula (rather than for a set of formulae), the following three claims are equivalent:

$A \vdash_{CL} B$
$A \vdash_{SpV} B$
$A \vdash_{SbV} B$

Subvaluationism diverges from its classical heritage when there is more than one premise. Supervaluationism diverges when there is more than one conclusion. Hyde presents this as a parallel between subvaluationism and supervaluationism, but as Keefe points out, it is not so clear that there is a real parallel here. After all, single-consequence arguments are a foundation of everyday reasoning, while multiple-consequence arguments are an artificial device introduced by logicians for purely formal purposes. Intuitions about the latter seem fairly negotiable, because it is not that clear we even have intuitions (as opposed to strongly held theoretical beliefs) about these formalities. Intuitions about single-consequence arguments, on the other hand, seem to be an important data point. And the intuition that adjunction holds really looks like something a theory should respect.

The best counter for the subvaluationist is to note that the failure of subjunction in supervaluationism has consequences even within the theory of the single-consequence entailment relation. The main consequence is that argument by cases ceases to be valid. If we look at how they diverge from classical logic in terms of which rules of proof they accept, it seems supervaluationism and subvaluationism do just as well. Each of them abandons one rule concerning the conditional (conditional proof) and negation (*reductio ad absurdum*). Supervaluationism also abandons one rule concerning disjunction (argument by cases) while subvaluationism abandons one rule concerning conjunction (adjunction). In neither case is this a happy record, but at the level of rules it does seem that the parallel is restored.

There are many amusing consequences of the failure of adjunction. These should not be viewed as *new* objections to the theory, but just as ways of drawing out the strange consequences of the view. Recall the p-cats, the precise cat-shaped objects perched on the mat. It seems that for each of them, there is a precisification on which it is *the* cat on the mat. Hence, for each of the p-cats, it is true according to subvaluationism that it is a cat on the mat. However, it is also true that there is exactly one cat on the mat, since this is true on all precisifications. That such examples can be multiplied indefinitely seems to count against the theory.

Hyde wants to stress that subvaluationism is as *good* as supervaluationism, though the parallels between the theories can help us see that the theories are also just as *bad* as each other. Three of the problems with supervaluationism seem especially worthy of note here. One of these we have already noted, that it violates several classically admissible rules of proof. Secondly, it also gives up on the T-schema, though in the reverse direction to supervaluationism. In subvaluationism, the material conditional *If S is true then S* is not always simply true. (It is always true, but it sometimes false as well.) There are two differences here that may be salient. First, when the T-schema fails in subvaluationism, it fails by being both true and false, in supervaluationism it is neither true nor false. Secondly, the direction in which it fails differs. This *might* make a difference in practice. Say that it is background knowledge that Nixon said that grass is white. Then the supervaluationist must reject (2), while the subvaluationist must reject (3).

(2) If grass is white, then what Nixon said is true.
(3) If what Nixon said is true, then grass is white.

I am inclined to think that (3) represents a more important piece of natural reasoning, so this counts a little more against the subvaluationist than the supervaluationist. Finally, just as supervaluationism has no plausible theory of higher order vagueness, neither does subvaluationism.

While there are interesting formal parallels between subvaluationism and supervaluationism, there is of course an important philosophical difference: supervaluationism posits gaps and subvaluationism posits gluts. If there are independent reasons to think that there are such gluts (gaps) then we might have some methodological reasons for holding a theory of vagueness based around gluts (gaps). This argument is made by J. C. Beall and Mark Colyvan, who suggest the second of the cases below that support gluts. So let's look at three possible sources (other than vagueness) of truth value gluts.

(a) Inconsistent Fictions

It is true in the Holmes stories that Watson was shot in the shoulder while serving as a military doctor in Afghanistan - it says so in *A Study in Scarlet*. It is also true in the Holmes stories Watson was shot in the leg while serving as a military doctor in Afghanistan - it says so in *The Sign of Four*. It also seems true, from the way Watson talks of his wound, that he was only shot once. So is some contradiction true in the story. No, each of these claims is true, though their conjunction is not true.

This is an interesting case for Hyde, because it not only looks like a case of gluts, but a failure of adjunction. As I presented it, the inconsistent stories are all prefixed with "It is true in the Holmes stories that". To get a better parallel to the case Hyde needs, note that the prefix does not need to be stated. If we say, in a normal context, *Watson was shot in the shoulder in Afghanistan*, we will normally be taken to speak truly. If this is because we really do speak truly, this is a great help for the glut theorist. The problem for this line of reasoning is that there are several explanations of what might be going on when we are taken to speak truly by saying *Watson was shot in the shoulder in Afghanistan*. It could be that we are speaking fictionally, as Stuart Brock suggests, or pretending that this sentence expresses a truth, as Kendall Walton suggests, or even that the sentence has an unarticulated prefix, as David Lewis seems to suggest. So there may not be much parallel between sentences like *Watson was shot in the shoulder in Afghanistan* and *Louis is bald*. Even if the former can, in a sense, be both true and false, this does not show the latter can be both true and false unless it can be interpreted *in that sense*, which it obviously cannot if there are distinctive ways of interpreting fictional discourse.

(b) Semantic Paradoxes

Beall and Colyvan point out that gappy solutions to the liar paradox are not normally well thought of these days. An obvious thing to say about the sentence *This sentence is false* is that it is neither true nor false. This move is intended to block the contradictions that can be inferred either from the assumption that it is true or that it is false. Unless the theory is extended in some way, this move cannot handle the sentence *This sentence is not true*, the so-called strengthened liar. If it is neither true nor false, then it is not true, but that is just what it says, so it is true, and the contradiction is regained. Beall and Colyvan argue that this kind of problem affects even sophisticated gappy solutions to the paradox, and this provides motivation for a glutty solution. If they are right, then we have a reason to accept gluts because of the paradoxes, and this means that handling vagueness by admitting gluts is less costly than we may have thought.

This argument rests heavily on the claim that the best solutions to the semantic paradoxes are glutty. Beall and Colyvan suggest that the argument for this claim goes by in two steps. First, gappys solutions to the semantic paradoxes fall to the strengthened liar. Secondly, if gappy solutions fail, we should adopt glutty solutions. Both steps can be questioned. I've always thought that Kripke's theory of truth had more resources to handle the strengthened liar than it was given credit for, and Vann McGee's work, where a consistent supervaluational theory of truth is developed in detail, seems to bear out this claim. But it would take us too far afield to properly debate that claim. And it may be irrelevant, since the failure of gappy theories would not prove that glutty theories of the paradoxes are correct. If gappy theories fail, it may be that we should look to bivalent solutions, such as those endorsed by Timothy Williamson and Michael Glanzburg. Without going into all the details, I'll just note two reasons for thinking these bivalent solutions should be given most attention if gappy theories really are refuted. First, we might just have a reflexive prejudice against contradictions (they are all *false*, you know) and hence against theories that endorse contradictions. Secondly, we might suspect that even if the glutty theories can handle the liar, at least by their own lights, they still cannot handle the other semantic paradoxes, especially Curry's paradox. David Lewis develops the first reason, and Greg Restall the second.

(c) Ambiguity

The least attractive feature of Kit Fine's version of supervaluationism is the way he attempts to extend the theory to ambiguity. Field claims that disambiguations play the same semantic role as precisifications. So ambiguous sentence like (4) or (5) are true only if true on all disambiguations.

(4) Driving sheep can be boring.
(5) Sheep are slow.

This is obviously a mistake. On its most natural interpretation, (4) is just true. (Sheep do not make a particularly fast pace, so trying to move them can be rather tedious.) On a less natural interpretation, (4) is making a claim about sheep who drive vehicles. I don't know of any such sheep, but I doubt they would be boring. Terrifying, perhaps, since sheep aren't the smartest creatures in the world, and safe driving takes at least a little intelligence, but certainly not boring.

The way in which Fine's theory fails seems to naturally support subvaluationism. When we have an ambiguous sentence that is true on some disambiguations, we normally take it to be true. Maybe Fine was *right* to stress the analogies between precisifications and disambiguations, and *wrong* merely in his theory of the semantic role of precisifications. This appearance is misleading. The reason we normally take sentences with *one* true disambiguation to be true is that we normally interpret the sentence to be making the true claim. We are, after all, charitable interpreters. If for some reason it was clear that this was not the appropriate interpretation, we would not think the sentence was true, the true disambiguation notwithstanding. For example, imagine that we discovered that, appearances to the contrary notwithstanding, sheep really are rather smart. Someone who uttered (5) intending to object to our discovery would be speaking *falsely*, although it is beyond dispute that the lack of pace of sheep means that (5) has one true disambiguation.

Perhaps this is not exactly the point that subvaluationists are intending to make. Perhaps the point is that all the talk about truth and falsity of sentences is wrong-headed, because truth and falsity are not properties of sentence types. Examples of ambiguity are a good way to bring this out. The idea *may* be that if we (foolishly) try to make sense of the application of truth values to sentence types, the best we can do is subvaluationism. We will return to this point when we discuss Hyde's treatment of the Sorites, which explicitly appeals to this connection with ambiguity, and again in the next chapter, when we look at other theories that seem to draw connections between ambiguity and vagueness.

Beall and Colyvan's argument has the potential to cut both ways. If there are reasons independent of vagueness to hold that there are truth-value gaps, but no reasons to hold that there are truth-value gluts, this points to a methodological advantage for supervaluationism over subvaluationism. Again, there are three cases that seem to support the gap hypothesis.

(a) Generics

Despite their famous role in theories of truth, it is rather difficult to provide a semantic theory for generics. Part of the problem is that even getting clear on the data is non-trivial. For example, is either (6) or (7) true?

(6) Americans like wine.
(7) Americans don't like wine.

My intuition, backed up by little more than first-hand observations of American drinking habits, is that neither (6) nor (7) is true.[2] Since many Americans clearly do like wine, (7) certainly fails to be true. But since so many do not like wine, and wine drinking is so infrequent relative to other salient countries, it also seems (6) fails to be true. If you take these intuitions at face value, and you think that (7) is the negation of (6), and you think that a sentence is false iff its negation is true, then you should think that (6) is neither true nor false. There are a few 'ifs' there, but for now I just want to note the *prima facie* case for the existence of a truth-value gap.

(b) Mathematics

A great many claims in set theory seem to be such that both they and their negations are consistent with the standard axioms for set theory, even if we take choice to be such an axiom. To give just the most famous such case, the claim that the size of the continuum is aleph-2 is neither entailed by nor inconsistent with these axioms. There is some temptation to regard neither the sentence (8) nor (9) as being true.

(8) The size of the continuum is aleph-2.
(9) The size of the continuum is larger than aleph-2.

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[2] I decided to do a bit more research, and surprisingly enough it turned out that my intuitions about this were correct. Americans are not exactly averse to wine, and in fact in absolute terms they are the third largest consumers of wine in the world. However, this is largely due to America's large population. In per capita terms, America ranks merely 25th of 30 OECD countries in terms of per capita wine consumption. The average Briton consumes twice as much wine as the average American, the average Australian two and a half times as much, and the average Frenchman (or woman) *eight times* as much. The French go through more than a bottle of wine per person per week, which is mighty impressive when you factor in all the children who presumably do not drink *that* much. These stats seem to bear out the claim that both (6) and (7) would be misleading, at best, and potentially false. The statistics are from the Wine Institute (www.wineinstitute.org).

Just as the fictional case seemed to support subvaluationism because it indicated how adjunction might fail, this case seems to support supervaluationism because it indicates how subjunction might fail. Even if neither (8) nor (9) is true, their disjunction certainly is true: the size of the continuum is greater than or equal to aleph-2.

Just as in the case of fictional discourse, we can 'explain away' the troubling intuition here. Perhaps in all these claims, there is an implicit "In all models for standard set theory" in front of each of the claims. This would be surprising - some people who have uttered claims like (8) and (9) did not intend to be prefixing their claims with such a qualifier. (When Gödel endorsed (9), as he frequently did, even after Cohen proved (8) was consistent with the standard axioms he certainly did not intend there to be such a qualifier.) The parallel claim for fictions is plausible. Anyone who says *Watson was shot in the shoulder in Afghanistan* presumably *does* intend to be talking about the Holmes stories. So this case might provide a stronger case for gaps than the case of fiction does for gluts.

(c) Fieldian indeterminacy

As previously discussed, Hartry Field argued that Newton's term *mass* is semantically indeterminate between two concepts in relativity theory: relativistic mass and proper mass. Field argues that when Newton uttered sentences that are true if *mass* is given one of these interpretations, and false on the other, then it is indeterminate whether what he said was true. Now some might, though Field wouldn't, and I wouldn't either, take this to be direct evidence for the existence of sentences that are neither true nor false. This case is more contentious than the previous cases (not that they were beyond dispute!), but I think it provides some intuitive support for the gappy theorist over the glutty theorist.

This little survey is far from conclusive, but it seems to me that the kind of considerations Beall and Colyvan raise point more towards supervaluationism than subvaluationism. There is one final point to note. Keefe argues that subvaluationism cannot explain the Sorites paradox. Since this will be relevant to the next chapter, we will conclude with this.

There is something a little strange about the subvaluational treatment of the Sorites. Keefe notes that if we do not use quantified premises, but rather use a list of conditionals (or disjunctions or negated conjunctions, it doesn't matter for present purposes) then the premises of the argument are all true, but the argument is *invalid* according to the subvaluational theory. (Some of the premises are also false, so the argument is quite bad from this perspective.) However, if use a quantified premise, or if we conjoin all the premises, then the argument is *valid*, but some of the premises are not true. So it seems the subvaluationist treats differently arguments that you might have thought should be treated alike. This is an unfortunate feature of the theory, but not fatal. After all, there is a multiple-consequence version of the Sorites that is classically valid but invalid according to supervaluational theory, so the loss of parallel with supervaluationism is not too deep. (I think *here* the fact that multiple-consequence relations are a mere technical device is not a great problem: the worry that subvaluationism does not provide a unified treatment to the various Sorites paradoxes is to an equal extent a technical worry.)

It is one thing to say that the Sorites argument is invalid, it is another thing to justify it. After all, it is fairly intuitive that modus ponens is truth preserving. Hyde recognises this, and aims to explain the invalidity of the argument by saying that it contains a fallacy of equivocation. It is not entirely clear just what Hyde means by this. The simplest thing to say is that none of the premises are true *simpliciter*, but each of them are true on some disambiguation or other. If this were the case, there would be a puzzle about why we can not, and more particularly *do* not, disambiguate. After all, we do not normally use ambiguous words like *slow* without disambiguating them.[3] Perhaps there is some reason that we *cannot* disambiguate these words. Borrowing a phrase from Lewis, subvaluationism really is a theory for equivocators. And perhaps this is why Hyde does not take what I called the 'simplest thing'. Rather, he says that being true on some disambiguation or other *just is* what it is for a sentence to be true *simpliciter*. As Keefe points out, it is a bit of a stretch in these cases to say that the argument really contains an equivocation. What we don't have is a change in meaning, but a change in the precisification in virtue of which the sentence is true. It seems implausible to me that this should count as a change of meaning. In the next chapter, we will look at some theories that take further this idea that vagueness consists essentially in a flexibility of meaning, though most of them use this idea to promote more conservative logical views than does Hyde.

[3] I have used *slow* as an example of an ambiguous word here, though I should note I am not entirely sure it is ambiguous. The two meanings, lack of physical pace and lack of mental pace, are close enough that it might be thought of as a kind of indeterminacy rather than ambiguity. But if this is true, it makes the point in the text sharper, since we do normal disambiguate (or perhaps determine) *slow* whenever we use it.