KEYNES AND WITTGENSTEIN

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1. Introduction

Three recent books (Davis 1994; Bateman 1996; Coates 1996) have argued that the philosophy behind Keynes's later economics (in particular the *General Theory*) is closer to Wittgenstein's post Tractarian theorising than to his early philosophy as expressed in his *Treatise on Probability*.[1] If Keynes did follow Wittgenstein in the ways suggested it would represent a substantial change from his early neoplatonist epistemology. In this paper I argue that the evidence for this thesis is insubstantial, and the best explanation of the evidence is that Keynes's philosophical views remained substantially unchanged.

There are three reasons for being interested in this question. The first is that it is worthwhile getting the views of a thinker as important as Keynes right. The second is that it would be mildly unfortunate for those of us attracted to Keynes's epistemology to find out that it was eventually rejected by its creator[2]. Most importantly, all parties agree that Keynes thought his philosophical theories had substantial consequences for economic theory. It is a little unusual for philosophical theories to have practical consequences; if one is claimed to it is worthwhile identifying and evaluating the claim.

Section 2 examines Bateman's claim that Keynes abandoned the foundations of his early theory of probability. Bateman's arguments turn, it seems, on an equivocation between different meanings of 'Platonism'. On some interpretations the arguments are sound but don't show what Bateman wants, on all others they are unsound. Section 3 looks at the conventionalist, intersubjective theory of probability Bateman and Davis claim Keynes adopted after abandoning his early objective theory. As they express it the theory's coherence is dubious; I show how it might be made more plausible. Nevertheless, there is little to show that

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1. Davis's views are also set out in his (1995), and Coates's to some extent in his (1997), but I will focus on the more detailed position in their respective books.
2. In the way that, for example, subjective Bayesianism was arguably invented by and eventually rejected by Ramsey. See his Ramsey (1926) and Ramsey (1929/1990).

Keynes adopted it. The only time he talks about conventions is in the context of speculative markets and in these contexts a conventionalist theory will give the same results as an objectivist theory.

Section 4 looks at Coates's quite different arguments for an influence from Wittgenstein to Keynes. Part of the problem with Coates's argument is that the textual evidence he presents is capable of several readings; indeed competing interpretations of the pages he uses exist. A bigger problem is that even when he has shown a change in Keynes's views occurred, he immediately infers the change was at the foundations of Keynes's beliefs. Section 5 notes one rather important point of Wittgenstein's of which Keynes seemed to take no notice, leading to an error in the *General Theory*. This should cast doubt on the claim that Keynes's later philosophy, indeed later economics, was based on theories of Wittgenstein.

2. Bateman's Case for Change

A brief biographical sketch of Keynes is in order to frame the following discussions, though I expect most readers are familiar with the broad outlines[3]. Keynes arrived as an undergraduate at Cambridge in 1902 and was based there for the rest of his life. For the next six years he largely studied philosophy under the influence of Moore and Russell. In 1907 he (unsuccessfully) submitted his theory of probability as a fellowship dissertation; this was successfully resubmitted the following year. His plans to make a book of this were interrupted by work on Indian finance, the war and its aftermath. It appeared as *Treatise on Probability* (hereafter, *TP*) in (1921), after substantial work on it in 1920. Modern subjectivist theories of probability, generally known as Bayesian theories, first appeared in critical reviews of this book (e.g. Borel (1924), Ramsey (1926)). After leaving philosophy for many years, Wittgenstein returned to Cambridge in 1929, and subsequently had many discussions with Keynes. In Keynes's *General Theory* (hereafter, *GT*) of (1936) and in some of the ensuing debate, Keynes referred to some distinctive

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3. For more details see Skidelsky (1983, 1992) or Moggridge (1992).

elements of the *TP*, leading some interpreters to suspect that there was a theoretical link between his early philosophy and his later economics.

There are two distinctive elements of Keynes's early theory of probability for our purposes. The first is its objectivism. Keynes held the probability of *p* given *h* is the degree of *reasonable* belief in *p* on evidence *h*, or, as Carnap (1950) put it, the degree of confirmation of *p* by *h*. These degrees are determined by logic; Keynes held that there was a partial entailment relation between *p* and *h*, of which the ordinary entailment relation (then thought to have been given its best exposition by Russell and Whitehead) was just a limiting case. And these relations are Platonic entities, we discover what they are by perceiving them through our powers of intuition. The second element is that the degrees may be non-numerical. So if the probability of *p* given *h* is α, we may be able to say $\alpha > 0.3$, and $\alpha < 0.5$, but not be able to give any finer numerical limits. As a corollary, there are now two dimensions of confirmatory support. Keynes claimed that as well as determining the probability of *p* given *h*, we could determine the 'weight' of this probability, where weight measures how much evidence we have. The more evidence is in *h*, the greater the weight. Keynes thought the distinction between saying that on evidence *h*, *p* has a low probability, and saying that the weight of that probability is low is important for understanding investment behaviour (*GT*: Ch. 12).

Bateman and Davis both claim that Keynes gave up this theory for an intersubjective theory in the *GT*. I'll focus on Bateman's book, largely because the structure of his argument is more straightforward[4]. Bateman sets himself to offer another solution to 'das Maynard Keynes problem', which he describes as follows.

"[Future theorists] will read *Treatise on Probability*'s account of

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4. All page references in sections 2 and 3 (unless otherwise stated) to Bateman 1996. Space considerations preclude a detailed examination of Davis's arguments, which are quite different to Bateman's. However his conclusions are subject to the same criticisms I make of Bateman's in section 3, and of Coates's in section 5.

the objective nature of probabilities and the way that rational people employ them, and they will wonder at how this person could have turned around 15 years later and written a book [the *GT*] in which irrational people who base their decisions on social conventions cause mass unemployment in the capitalist system" (7)

I doubt this is the right thing to say about the *GT*, but that's another story. For now we might simply note that there's no obvious conflict here. For one thing, if the people in the *TP* are rational, and in the *GT* are irrational, as Bateman allows, it's not too surprising they behave differently. More generally, it's to be expected (sadly) that normative and descriptive theories are different, and by Bateman's lights that should explain the difference between the outlook of the explicitly normative *TP* and the at least partially descriptive *GT*. If the agents in the *GT* are irrational, that book cannot but be a purely normative account of rationality. On the other hand, if the *TP* were taken to be descriptive and not just normative, if it claimed that people really conform to its epistemological exhortations, there could be a conflict. I can't imagine, however, what the evidence or motivation for that reading could be.

If there were a conflict between the *GT* and *TP*, there ought be a greater one between the 'rational people' of the *TP* and the blatantly irrational leaders in *Economic Consequences of the Peace* (*ECP*). These books were published about 15 months apart, not 15 years. And the most memorable parts of *ECP* are the descriptions of the mental failings of President Wilson, who Lloyd George could 'bamboozle' into believing it was just to crush Germany completely, but not 'de-bamboozle' out of this view when it became necessary. Or maybe we should say there's a conflict because the characters in David Hume's histories do not meet his ethical or epistemological norms.

If we give up Bateman's claim that the actors in the *GT* are irrational, and substitute the claim that the norms of rationality in the two books differ, then we have a real conflict. And the most charitable interpretation of Bateman is that this is the conflict he intends to discuss. At

the bottom of page 12 he goes close to saying exactly this, but then proceeds to support his position with evidence that Keynes changed his position on how rational people actually are. Once we are claiming the change of view is with regard to norms, evidence of opinion changes about empirical questions becomes irrelevant. This does mean much of Bateman's case goes, though not yet all of it.

The main problem with Bateman's argument is that it rests on an equivocation over the use of the term 'Platonism'. In *TP* Keynes held that probability relations are objective, non-natural and part of logic. I'll use 'logical' for the last property. When Bateman says Keynes believed probability relations were Platonic entities, he is alternately referring to each of these properties. He seems to explicitly use 'Platonic' to mean 'objective' on page 30, 'non-natural' on page 131, and 'logical' on page 123. But this isn't the important equivocation.

Say a theory about some entities is a 'Strong Platonist' theory if it concords with all Keynes's early beliefs: those entities are objective, non-natural and logical. Bateman wants to conclude that by the time of the *GT*, Keynes no longer had an objectivist theory of probability. But showing he no longer held a Strong Platonist view won't get that conclusion, because there are 3 interesting objectivist positions which are not Strong Platonist. The following names are my own, but they should be helpful.

Carnapian Probability relations are objective, natural and logical. This is what Carnap held in his (1950).

Gödelism Probability relations are objective, non-natural and non-logical. Gödel held this view about numbers, hence the name. I'd normally call this position Platonism, but that name's under dispute. Indeed I suspect this is what Keynes means by Platonism in *My Early Beliefs*. (Keynes 1938b)

Reductionism Probability relations are objective, natural and non-logical. Such positions don't have to reduce probability to something else, but they usually will. Russell held such a position in his (1948).

These categories could apply to other entities, like numbers or moral

properties or colours, but we will be focussing on probability relations here. Say a theory is 'Weak Platonist' if it is Strong Platonist or one of these three types. The most interesting equivocation in Bateman is using 'Platonist' to refer to either Strong or Weak Platonist positions. He argues that Keynes gave up his early Platonist position. These arguments are sound if he means Strong Platonist, unsound if he means Weak Platonist. But if he means Strong Platonist he can't draw the extra conclusion that Keynes gave up objectivism about probability relations, which he does in fact draw. So I'll examine his arguments under the assumption that he means to show Keynes gave up Weak Platonism.

Whatever Bateman means by Keynes's Platonism, he isn't very sympathetic to it. It gets described as 'obviously flawed' (4) and 'fatally flawed' (17), and is given as the reason for his work being ignored by 'early positivists and members of the Vienna Circle' (61). Given that the *TP* is cited extensively, and often approvingly, by Carnap in his 1950, this last claim is clearly false. Most stunningly, he claims writers committed to the existence of Platonic entities cannot 'be considered to be a part of the analytic tradition' (39), though he does concede in a footnote that some 'early analytical philosophers' (he gives Frege as an example) were Platonist. Bateman's paradigm of philosophy seems to be the logical positivism of Ayer's *Language, Truth and Logic*: "nowhere would one less expect to find metaphysics than in modern analytical philosophy" (Ayer 1936, 39).

There is an implicit argument in this derision. Keynes must, so the argument goes, have given up (Weak) Platonism because no sensible person could believe it. If anything like this were sound it should apply to Weak Platonism about other entities. But the history of 'modern analytical philosophy' shows that Weak Platonism (though not under that name) is quite widespread in metaphysical circles. Modern philosophy includes believers in possible worlds both concrete and ersatz, in universals and in numbers. All these positions would fall under Weak Platonism. Even Quine's ontologically sparse *Word and Object* was Weak Platonist about classes, though he probably wouldn't like the label. So by analogy Weak Platonism about probability relations isn't so absurd

as to assume Keynes must have seen its flaws.

Bateman's more important argument is direct quotation from Keynes. This argument is undermined largely because of Bateman's somewhat selective quotation. There are two sources where Keynes appears to recant some of his early beliefs. Which early beliefs, and how early these beliefs were, is up for debate. The two are his 1938 memoir *My Early Beliefs* (hereafter, *MEB*), and his 1931 review of Ramsey's posthumous *Foundations of Mathematics*. *MEB* wasn't published until 1949, three years after Keynes's death, but according to its introduction it is unchanged from the version Keynes gave as a talk in 1938. In it he largely discusses the influence of Moore, and particularly *Principia Ethica*, on his beliefs before the first world war.

There are several connections between Moore's work and Keynes. The most pertinent here is that Keynes's metaphysics of probability in *TP* is borrowed almost completely from Moore's metaphysics of goodness. Not only are probability relations objective and non-natural, they are simple and unanalysable. These are all attributes Moore assigns to goodness. The only addition Keynes makes is that his probability relations are logical. So Moore's position on goodness is, in our language, Gödelian.

As he says in *MEB*, Keynes became convinced of Moore's metaethics, though he differed with Moore over the implications this had for ethics proper. In particular he disagreed with Moore's claim that individuals are morally bound to conform to social norms. Bateman seems to assume that at any time Keynes's metaphysics of goodness and probability will be roughly the same, and with the exception of questions about their logical status, this seems a safe enough assumption.

Bateman quotes Keynes saying that his, and his friends', belief in Moore's metaethics was 'a religion, some sort of relation of neoplatonism' (Keynes 1938b, 438). This is part of the evidence that Keynes meant what I'm calling Gödelism by 'Platonism'. Not only does he use it to describe Moore's position, but comparing Platonism with religion would be quite apt if he intends it to involve a commitment to objective, non-natural entities. The important point to note is that he is using

'religion' to include his metaethics, a point Bateman also makes, though it probably also includes some broad ethical generalisations. Bateman then describes the following paragraph as removing 'any doubt that [Keynes] had thrown over his youthful Platonism as untenable'. (40)

> Thus we were brought up – with Plato's absorption in the good in itself, with a scholasticism which outdid St. Thomas, in calvinistic withdrawal from the pleasures and successes of Vanity Fair, and oppressed with all the sorrows of Werther. It did not pervert us from laughing most of the time and we enjoyed supreme self-confidence, superiority and contempt towards all the rest of the unconverted world. But it was hardly a state of mind which a grown-up person in his senses could sustain literally. (Keynes 1938a, 442).

As it stands, *perhaps* the last sentence signals a change in metaphysical beliefs, as opposed to say a change in the importance of pleasure-seeking. In any case the following paragraph (which Bateman neglects to quote) shows such an interpretation to be mistaken.

> It seems to me looking back, that this religion of ours was a very good one to grow up under. It remains nearer the truth than any other I know, with less extraneous matter and nothing to be ashamed of ... It was a purer, sweeter air than Freud cum Marx. It is still my religion under the surface. (Keynes 1938a, 442).

So was Keynes confessing to 'a state of mind which a grown-up person in his senses couldn't sustain literally'? No; his 'religion' which he held onto was a very broad, abstract doctrine. It needed supplementation with a even general ethical view, to wit an affirmative answer to one of Moore's 'open questions'. And then it needed some bridging principles to convert those ethics into moral conduct in the world as we find it. His early position included all these, and it seems it was in effect his early 'bridging principles' he mocks in the above quote. These relied, the memoir makes clear throughout, on an excessively optimistic view of human nature, so he thought in effect that he could prevent

wrong by simply proving to its perpetrators that they were wrong. Now giving up one's bridging principles doesn't entail abandonment of a general ethical view, let alone one's metaethics. Indeed, let alone one's metaphysics of probability! And as the last quote makes clear, Keynes was quite content with the most general, most abstract parts of his early belief. If this were all Bateman had to go on it wouldn't even show Keynes had abandoned Strong Platonism[5].

There is more to Bateman's case. In Keynes's review[6] of Ramsey (1931), he recanted on some of his theory of probability. This is quite important to the debate, so I'll quote the relevant section at some length.

> Ramsey argues as against the view which I had put forward, that probability is concerned not with objective relations between propositions but (in some sense) with degrees of belief, and he succeeds in showing that the calculus of probabilities simply amounts to a set of rules for ensuring that the system of degrees of belief which we hold shall be a *consistent* system. Thus the calculus of probability belongs to formal logic. But the basis of our degrees of belief – or the *a priori* probabilities, as they used to be called – is part of our human outfit, perhaps given us merely by natural selection, analogous to our perceptions and our memories rather than to formal logic. So far I yield to Ramsey – I think he is right. But in attempting to distinguish 'rational' degrees of belief from belief in general he was not yet, I think, quite successful. It is not getting to the bottom of the principle of induction to merely say it is a useful mental habit. (Keynes 1931, 338–39).

Tellingly, Bateman neglects to quote the final two sentences. I think there is an ambiguity here, turning on the scope of the 'so far' in the fourth sentence. If it covers the whole section quoted, it does amount

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5. The above points are similar in all substantial respects to those made by O'Donnell (1991) in response to an earlier version of Bateman's account.
6. This is often mistakenly referred to as an obituary in the literature, e.g. (Coates 1996, 139).

to a wholesale recantation of Keynes's theory, and this is Bateman's interpretation. But if we take the first sentence, or at least the first clause, as being outside its scope it does not. And there are two reasons for doing this. First, it seems inconsistent with Keynes's later reliance on the *TP* in parts of the *GT*, as (O'Donnell 1989 Ch. 6) has stressed. Secondly, it is inconsistent with Keynes's complaint that on Ramsey's view induction is merely a 'useful habit'. If Keynes had become a full-scale subjectivist, he ought have realised that patterns of reasoning could only possibly be valid (if deductive) or useful (otherwise). Since he still thought there must be something more, he seems to believe an objectvist theory is correct, though by now he is probably quite unsure as to its precise form. So in effect what Keynes does in this paragraph is summarise Ramsey's view, list the details he agrees with (that probability relations aren't logical), notes his agreement with them, and then lists the details he disagrees with (that probability relations aren't objective).

There is more evidence that all this quote represents is a recantation of the view that probability relations are logical. Earlier in that review he notes how little formal logic is now believed to achieve compared with its promise at the start of the century.

> The first impression conveyed by the work of Russell was that the field of formal logic was enormously extended. The gradual perfection of the formal treatment at the hands of himself, of Wittgenstein and of Ramsey had been, however, gradually to empty it of content and to reduce it more and more to mere dry bones, until finally it seemed to exclude not only all experience, but most of the principles, usually reckoned logical, of reasonable thought. (Keynes 1931, 338).

More speculatively, I suggest Keynes's change of mind here (for this shows he had surely given up the view that probability relations are logical) might be influenced by Gödel's incompleteness theorem. In the *TP* Keynes had followed Russell is saying mathematics is part of logic (Keynes 1921, 293n). That view was often held to be threatened by Gödel's proof that there are mathematical truths which can't be proven, and that the consistency of mathematics can't be proven. But no one suggested this meant mathematics is merely subjective, or that mathematical Platonism was therefore untenable. If this response to Gödel is right, it shows there are objective standards of reasoning (i.e. mathematical standards) that are not part of logic. This makes it less of a leap to say there are objective principles of reasonable thought that are not 'logical' in the narrow sense we've been using.

So would Keynes have known of Gödel's theorem when he wrote this review? I think it's possible, though some more research is needed. Keynes's review was published in *The New Statesman and Nation* on October 3, 1931. This was a weekly political and literary magazine of which Keynes was chairman. So we can safely conclude the piece was drafted not long before publication. Gödel's theorem was first announced at a conference in Vienna in September 1930 (Wang 1987), and was published in early 1931. While Keynes would certainly have not read Gödel's paper, its content could easily have reached him through Cambridge in that 12 month 'window'. Since the explicit aim of Gödel's paper was to show the incompleteness of *Principia Mathematica*, it would have immediately had some effect in Cambridge, both in philosophy and mathematics. Given this evidence, the probability Keynes knew of Gödel's theorem when he wrote the review of Ramsey still mightn't be greater than one-half, but it mightn't be less than that either.

In sum, I conclude that Keynes had given up his earlier belief that all rules of reasonable belief are logical. This is what he yields to Ramsey. This concession would be supported by the 'drying up' of formal logic that Keynes notes, perhaps most dramatically expressed in Gödel's theorem. But he hadn't given up the belief that there are objective rules which are extra-logical, and given the identification of probability with degree of reasonable belief, he had no reason to reject Gödelism or Reductionism about probability. Hence Bateman's argument that he rejected objectivist theories of probability fails.

3. Conventionalism

Bateman and Davis each argue that Keynes adopted a convention-alist, intersubjectivist theory of probability. In Davis this is explicily attributed to Wittgenstein's influence, however in Bateman it is less clear what the source of this idea is. It isn't obvious what they mean by an intersubjective theory. In particular, it isn't clear whether they mean this to be an empirical or a normative theory; whether Keynes is claiming that we ought set our degrees of belief by convention or that we in general do. Since the empirical theory would be consistent with his objectivist norms, and they stress the change in his views, I conclude they are claiming this is a new normative view. According to this view being reasonable is analysed as conforming to conventions. This is not a very standard epistemological position, but something similar is often endorsed in ethics. Bateman marshals the evidence that Keynes moves from an objectivist to a conventionalist position in ethics as evidence for this epistemological shift, but this doesn't seem of overwhelming significance[7].

Here's the closest Bateman gets to a definition of what he means by an intersubjective theory of probability.

> When probabilities are formed according to group norms, they are referred to as intersubjective probabilities ... I take it to be the case that in a world of subjective probabilities some individuals will form their own estimates and others will form them on the basis of group norms (50n).

This makes it look very much like an empirical theory, as it refers to how people actually form beliefs, not how they ought. So his inter-subjectivism looks perfectly consistent with Keynes's objectivism. I am

completely baffled by the 'world of subjective probabilities'. I wonder what such a world looks like, and how it compares to our world of tables, chairs and stock markets?

Fortunately there is a theory that does the work Bateman needs. Ayer (1936) rejects orthodox subjectivism about probability on the grounds that it doesn't allow people to have mistaken probabilistic beliefs. But he can't admit Keynesian probability relations into his sparse ontology. The solution he adopts is to define probability as degree of rational belief, but with this caveat.

> Here we may repeat that the rationality of a belief is defined, not by reference to any absolute standard, but by reference to part of our own actual practice (Ayer 1936, 101).

The 'our' is a bit ambiguous; interpreting it to refer to the community doesn't do violence to the text, though it is just as plausible that it refers to a particular agent. The 'part of our practice' referred to is just our general rules for belief formation. These aren't justified by an absolute standard; they are justified by the fact they are our rules, and presumably by their generality. Given Bateman's views about metaphysics, it seems quite reasonable to suppose he'd follow Ayer on this point.

The evidence Keynes adopted such a position is usually taken to be some passages from the *GT* and the 1937 *QJE* paper in which he replied to some attacks on that book. Here's the key points from the two quotes Bateman uses to support his view.

> In practice we have agreed to fall back on what is, in truth, a *convention*. The essence of this convention – though it does not, of course, work out quite so simply – lies in assuming that the existing state of affairs will continue indefinitely, except in so far as we have specific reasons for expecting a change (*GT*: 152).

> How do we manage in such circumstances to behave in a man-ner which saves out faces as rational, economic men? We have devised for the purposes a variety of techniques, of which much

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7. If Keynes had adopted a framework which implied a tight connection between epistemological and ethical norms, such as a form of utilitarianism that stressed maximisation of expected utility, this would be important, since he couldn't change ethics and keep his epistemology. But such frameworks aren't compulsory, and given the vehemence with which Keynes denounced utilitarianism (Keynes 1938b, 445) it seems he didn't adopt one.

the most important are the three following: ...
(3) Knowing that our own individual judgement is worthless, we endeavour to fall back on the judgement of the rest of the world which is perhaps better informed. That is, we endeavour to conform with the behaviour of the majority or the average. The psychology of a society of individuals each of whom is endeavouring to copy the others leads to what we may strictly term a *conventional* judgement (Keynes 1937, 115).

There are two problems with using this evidence the way Bateman does. The first is the old one that they seem expressly directed to empirical questions, though perhaps appearances are deceptive here. The more important one is that Keynes is attempting to answer a very specific question with these passages; in ignorance of the question we can easily misinterpret the answer.

How much ought one pay for a share in company X? Well, if one intends to hold the share come what may, all that matters is the expected prospective yield of X's shares, appropriately discounted, as compared to the potential yield of that money in other uses. But as Keynes repeatedly stresses (*GT*: 149; (Keynes 1937, 114)) we have no basis for forming such expectations. Were this the only reason for investing then purely commercial investment may never happen.

There is another motivation for investment, one that avoids this problem. We might buy a share in X today on the hope that we will sell it next week (or next month or perhaps next year) for more than we paid. To judge whether such a purchase will be profitable, we need a theory about how the price next week will be determined. Presumably those buyers and sellers will be making much the same evaluations that we are. That is, they'll be thinking about how much other people think X is worth.

> We have reached the third degree where we devote our intelligences to anticipating what average opinion expects the average opinion to be. And there are some, I believe, who practice the fourth, fifth and higher degrees (*GT*: 156).

There is simply no solution to this except to fall back on convention. That is, we are forced into a conventionalist theory of value, at least of investment goods. But this doesn't mean that we have a conventionalist epistemology. On the contrary, it means that our ordinary (objectivist) empiricism is unimpeded. For the question that Keynes has us solve by reference to convention is: What is the value of X? This is equivalent to, what will be value of X be, or again, to what are the conventional beliefs about X's value? We need to answer a question about the state of conventions, and as good empiricists we answer it by observing conventions.

An analogy may help here. Here's something that Hempel believed: to gain rational beliefs about the colour of ravens, one has to look at some birds. Did this mean he had an ornithological epistemology? No; he had an empiricist epistemology which when applied to a question about ravens issued the directive: Observe ravens! Similarly Keynes's belief that to answer questions about value, i.e. about conventions, one has to look at conventions, does not imply a conventionalist epistemology. It just means he has an empiricist epistemology which when applied to a question about conventions issues the directive: Observe conventions!

There might be another motivation for using conventions, again consistent with Keynes's objectivist empiricism. Sometimes we may have not made enough observations, or may not have the mental power to convert these to a theory. So we'll piggyback on someone else's observations or mental powers. (This seems to be what's going on in the quote from Keynes (1937).) Or even better, we'll piggyback on everyone's work, the conventions. To see how this is consistent with an objectivist epistemology (if it isn't already obvious) consider another analogy.

What is the best way to work out the derivative of a certain function? Unless your memory of high-school calculus is clear, the simplest solution will be to consult an authority. Let's assume for the sake of argument that the easiest authorities to consult are maths texts. It seems like the rational thing to do is to act as if the method advanced by the

maths texts is the correct method. Does this mean that you have adopted some kind of authoritarian metaphysics of mathematics, where what it is for something to be correct is for it to be asserted by an authority? Not at all. It is assumed that what the textbook says is correct, but the authoritarian has to make the extra claim that the answer is correct *because* it is in the textbook. This is false; that answer is in the textbook because it is correct. In sum, the authoritarian gets the direction of fit wrong.

Similarly in the 'piggyback' cases the intersubjectivist gets the direction of fit wrong. We are accepting that *p* has emerged as 'average opinion', then it is reasonable to believe *p*. But we aren't saying with the intersubjectivist it is reasonable to believe *p* because *p* is average opinion; rather we are assuming *p* is average opinion because it is reasonable to believe *p*.

The evidence so far suggests Keynes's statements are consistent with his denying intersubjectivism. We might be able to go further and show they are inconsistent with his adopting that theory. After the quote on *GT* page 152 he spends the next page or so defending the use of conventions here. The defence is, in part, that decisions made in accord with conventions are reversible in the near future, so they won't lead to great loss. If he really were an intersubjectivist, the use of conventions would either not need defending, or could be defended by general philosophical principles. Secondly, there is this quote which in context seems inconsistent with adopting a conventionalist view.

> For it is not sensible to pay 25 for an investment which you believe the prospective yield to justify a value of 30, if you also believe that the market will value it at 20 three months hence (*GT*: 155).

The context is that he is discussing why reasonable professional investors base their valuations on convention rather than on long-term expectation. Hence the 'you' in the quote is assumed to be reasonable. Hence it is reasonable, Keynes thinks, to believe that an investment's prospective yield justifies a value of 30, and that conventional wisdom

is that its prospective yield is much lower. But if all reasonable beliefs were formed by accordance with conventional wisdom, this would be inconsistent. Hence Keynes cannot have adopted a conventionalist epistemology.

4. Keynes and Vagueness

What a terrible state Keynes interpretation has got into! From the same few pages (the opening of *GT* Ch. 4) Coates (1996) reads into Keynes a preference for basing theory on vague predicates, Bradford and Harcourt (1997) read Keynes as denying that predicates which are unavoidably vague can be used in theory, and O'Donnell (1997) sees Keynes as holding a position in between these.

Coates's theory is that Keynes abandoned the narrowly analytic foundations of his early philosophy because of the problems of vagueness that were pointed out to him by Wittgenstein. He has Keynes in 1936 adopting a middle way between analytic and Continental philosophy, which gives up on analysis because of unavoidable vagueness, but which doesn't follow Derrida in saying all that's left after analysis is 'poetry'. He also wants to argue for the philosophical importance of this theory. In this essay I'll focus on his exegetical theories, though there are concerns to be raised about his philosophy.

As in Bateman, analytic philosophy gets very narrowly defined in Coates[8]. Here it includes the claim that truth-value gaps are not allowed (xii). This excludes from the canon some of the most important papers in analytical philosophy of the last few decades (e.g. Dummett (1959), Fraassen (1966), Fine (1975), Kripke (1975)), and hence must be a mistake. To use one of Coates's favourite terms, 'analytic philosophy' is a family resemblance concept, not to be so narrowly cast. In particular, as we'll see, analytic philosophers don't have to follow Frege in being nihilist about vagueness.

Even more bizarrely, Coates defines empiricism so it includes both psychologism in logic and utilitarianism in ethics (72-3). Since Ayer

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8. All page references in this section (unless otherwise stated) to Coates (1996).

(1936) opposes each of these doctrines, does that makes Ayer an anti-empiricist? If Ayer is a paradigm empiricist (as seems plausible) Keynes's rejection of psychologism and utilitarianism can hardly count as proof of opposition to empiricism, as Coates wants it to do. Apart from the fact that Mill believed all three, there is no interesting connection between empiricism, psychologism and utilitarianism.

Coates's story is that in the *GT* Keynes allowed both his units and his definitions to be quantitatively vague so as to follow natural language. This constitutes a new 'philosophy of social science' (85) that is based on the ordinary language philosophy of the later Wittgenstein. There are several problems with this story. The first is that most of Coates's evidence comes from *obiter dicta* in early drafts of the *GT*; by the time the book was finished most of these suggestions are expunged. The second is that it's quite possible to accept vagueness within a highly analytic philosophical framework. The third is that the way Keynes uses vagueness is only consistent within such a framework.

The first part of the story focuses on how Keynes derided his predecessors for using concepts that were vague as if they were precise. Coates adduces evidence to show Keynes in this context used 'vague' as a synonym for 'quantitatively inexact'. The most important concept misused by Keynes's predecessors in this way was the general price level. Of course this was hardly a new point in the *GT*; Keynes (1909) says similar things. Coates claims that Keynes's reaction to this misuse was to 'criticise formal methods' (83), and to conclude that 'economic analysis can do without the "mock precision" of formal methods' (85). This is all hard to square with Keynes's explicit comments.

> The well-known, but unavoidable, element of vagueness which admittedly attends the concept of the general price-level makes this term very unsatisfactory for the purposes of a causal analysis, which ought to be exact (*GT*: 39).

Further, Keynes then defends his choice of units of quantity (quantity of money-value and quantities of employment) on the grounds that they are not quantitatively vague. Coates is surely right when he says

that Keynes's analysis of vagueness here is 'not very controversial'; although it is perhaps misleading to say it is controversial at all.

The second, and central, part of the story focuses on how Keynes allowed his definitions to be vague, but defended this on the grounds of conformity to ordinary language. This 'introduces what is distinctive about his later philosophy of the social sciences' (85). The bulk of Coates's evidence comes from Keynes's commentary on his own definitions; usually this includes a claim that he has captured the ordinary usage of the term. Since he uses 'common usage' to explicitly mean 'usage amongst economists' (*GT*: 79) the support these *dicta* give to Coates's theory might be minimal, but we'll ignore that complication. The real problem is that this commentary extends to cases where he has changed his mind over the best definition. For example, Coates quotes Keynes writing in a draft of the *GT* about the definition of income.

> But finally I have come to the conclusion that the use of language, which is most convenient on a balance of considerations and involves the least departure from current usage, is to call the actual sale proceeds *income* and the present value of the expected sale proceeds *effective demand* (Keynes 1934, 425).

Coates comments:

> By choosing definitions on the ground that they correspond with actual usage Keynes was formulating an ordinary language social science, one that bears a resemblance to those argued for by philosophers of hermeneutics (90).

He then goes on to note some comments from the *GT* apparently about this definition, and how it relates to common usage. The problem is that this isn't the definition of income Keynes settles on in the *GT*. There he defines income of an agent as "the excess of the value of his finished output sold during the period over the prime cost" (*GT*: 54), and *net income* (which Coates fails to distinguish) as income less supplementary cost. Given that at every stage Keynes justified his current definitions by their (alleged) conformity with common usage, even

when he changed definitions, it is hard to believe that these justifications are more than rhetorical flourishes. After all, who will deny that *ceteris paribus* technical definitions should follow ordinary usage?

If Keynes's early choice of definitions showed an adherence to a 'philosophy of hermeneutics', perhaps his abandonment of those definitions constitutes abandonment of that philosophy. One change doesn't necessarily mean a change in foundations, so it is worth looking at those foundations.

As I mentioned, allowing that vagueness exists doesn't mean abandoning the Russellian program of giving a precise analysis of language. There are two reasons for this. First, contra Wittgenstein it is possible to analyse vague terms. Secondly, there are semantic programs very much in the spirit of Russell which allow vagueness. I'll deal with these in order.

In *Philosophical Investigations*, Wittgenstein (1953) argued that the existence of vagueness frustrated the program of analysis (ss. 60, 71). The argument presumably is that analyses are precise, and hence they cannot accurately capture vague terms. (See also his comments about the impossibility of drawing the boundaries of 'game' in s. 68.) This is a simple philosophical mistake. We can easily give an analysis of a vague term, we just have to make the analysans vague in exactly the same way as the analysandum.

To see this in action, consider that paradigm of modern philosophy, Lewis's analysis of subjunctive conditionals or counterfactuals. Lewis (1973) says that the conditional 'If p were the case, it would be that q' is true iff q is true in the most similar possible world in which p. He considers the objection that 'most similar' is completely vague and imprecise.

> Imprecise it may be; but that is all to the good. Counterfactuals are imprecise too. Two imprecise concepts may be rigidly fastened to one another, swaying together rather than separately, and we can hope to be precise about their connection Lewis (1973).

Whatever the fate of Lewis's theory, his methodology seems uncon-

testable. Wittgenstein's claim that analysis must be abandoned because of vagueness is refuted by these observations of Lewis. Hence Coates's claim that allowing vagueness (as Keynes does) means giving up on analytic philosophy is mistaken.

The second problem with Coates's comments on vagueness is that he hasn't allowed for what I'll call 'orthodox' responses to vagueness. The aim of the early analytics drifted between giving a precise model for natural language, and replacing natural language with an artificial precise language. The latter, claims Coates, ought be abandoned because of the pragmatic virtues of a vague language. Let's agree to that; can the spirit of the early aim of giving a precise analysis of language be preserved?

Two approaches which seem to meet this requirement are the supervaluational and epistemic theories of vagueness. The supervaluationist says language can't be represented by a precise classical model, but it can be represented by a set of such models. The epistemic theorist says that there is a precise model of language, but we cannot know what it is[9]. Call a theorist who adopts one of these approaches 'orthodox'. The name is chosen because supporters and critics of orthodoxy agree that these positions represent attempts to minimise deviations from the classical, Russellian program.

Clearly Keynes did not explicitly adopt an orthodox theory of vagueness. Williamson (1994) attempts to trace the epistemic theory back to the Stoics, but general consensus is that these approaches were all but unknown until recently. What I want to argue is that Keynes's intuitions are clearly with orthodoxy. Coates, on the other hand, wants to place Keynes in a tradition that is critical of classical analysis, and perhaps finds its best modern expression in the exponents of fuzzy logics. To see this is wrong, note that the following beliefs are all in the *GT*.

1. All goods are (definitely) investment goods or consumption goods.

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9. See Williamson (1994) for the best epistemic account, Fine (1975) and Keefe (2000) for the best supervaluationist accounts.

2. For some goods it is vague whether they are an investment or consumption good. (*GT*: 61)

3. The yield of an investment, q, is vague.

4. The carrying cost of an investment, c, is vague.

5. The net yield of an investment, $q - c$, can be precisely determined. (*GT*: 226)

Since Keynes believed (1) to (5) we can safely conclude he believed they were consistent. More importantly, since the *GT* has been analysed more thoroughly than any other economic text written this century, and no one has criticised the consistency of (1) to (5), it seems many people agree with him. Hence if conformity with pre-theoretic intuitions of consistency is a central desideratum of a theory of vagueness, we can discard any theory that does not say they are consistent. However, of those theories on the market, only orthodox theories meet this requirement. It might also be noted that (1) and (2) are repeated in just about every introductory macro textbook, again without to my knowledge any question of their consistency.

We can quickly see that these propositions are all consistent on either orthodox theory. The supervaluationist says there is a set of classical models for a language; a sentence is true iff it is true on all models, false iff it is false on all models, and truth-valueless otherwise. Vague terms have different meanings on different models. So for a particular good, say a car, about which it is vague whether it is an investment or consumption good, the supervaluationist says it is an investment good on some models and a consumption good on others. So (2) is satisfied; however on all models it, like everything else, is either a consumption or investment good, so (1) is satisfied. Similarly because it is vague whether some costs should be counted as deductions from the yield of an investment or increments to its carrying cost, the values of q and c will be different on different models. Hence (3) and (4) are true, but $q - c$ is constant across models[10], so (5) is true.

10. A particular cost will either remove an amount from q or add an equal amount to c, depending on how it is categorised.

The epistemic theorist says that vagueness is just ignorance. As we can know that a car is an investment or consumption good without knowing which, (1) and (2) can be satisfied. Similarly, since we can know that a cost is incurred without knowing how to account for it in Keynes's terms, we can know $q - c$ precisely without knowing q or c precisely, and hence (3) to (5) can be satisfied.

The heterodox theorist has a harder time. The theorist who, following Russell (1923), says that vagueness is infectious, if a part is vague so is the whole, will deny that (1) and (2) can be true together. Unless it's definitely true that a car is an investment or definitely true it's a consumption good it can't be definitely true that it's one or the other. This also seems to be the position taken by Wittgenstein (1953).

The nihilist about vagueness, who follows Frege in saying vague terms can't be used coherently, similarly can't endorse both (1) and (2). On that view, if p and q are both vague, then their disjunction can't be true. Arguably, on this position the disjunction of p with anything cant be true, as it is nonsense, but we don't need anything that strong.[11]

The extra truth-values approach to vagueness (of which fuzzy logic is a variant) also can't make (1) and (2) consistent. On any such approach (whether 3-valued, n-valued or continuum-valued) the degree of truth of a disjunction can't be higher than the degree of truth of each of the disjuncts. So if neither 'This is an investment' nor 'This is a consumption good' is absolutely true (true to degree 1), 'This is an investment or consumption good' can't be absolutely true. Yet this is just what Keynes asserted to be possible, and what several generations of readers have found perfectly consistent. I have only remarked about the problem the consistency of (1) and (2) poses for heterodox theories. These remarks apply, *mutatis mutandis*, to (3), (4) and (5), but as theorists rarely discuss quantitative vagueness (as opposed to truth-value vagueness) these cases involve a bit more speculation as to what heterodoxy says.

11. Compare the logic in Bochvar (1939), where $p \lor q$ is truth-valueless if p is true and q truth-valueless. Summaries of this and many other many-valued logics are in Haack (1974).

Hence Keynes did not belong to a heterodox tradition *vis a vis* vagueness, and heterodox theories fail to capture a crucial pre-theoretic intuition about vague terms. So Coates's claims that Keynes followed Wittgenstein into heterodoxy here, and that he ought have, are both mistaken.

Even if all of the above is mistaken, there remains serious doubt that Keynes had in mind anything like what Coates attributes to him. Coates makes the chapters on definitions in the *GT* into the foundations of a new philosophy, and constituting an important revolution in theory. This is crucial to Coates's story about the influence of Wittgenstein on Keynes. But this attribution is totally at odds with Keynes's comments on these chapters, comments that not only reveal his attitudes towards his definitions but also seem a fair commentary on them.

> I have felt that these chapters were a great drag on getting on to the real business, and would perplex the reader quite unnecessarily with a lot of points which really do not matter to my proper theme (Keynes to Roy Harrod, 9 August 1935, quoted in (Keynes 1971 XIII: 537)).

> But the main point I would urge is that all this is *not* fundamental. *Being clear* is fundamental, but the choice of definitions of income and investment is not (Keynes to Dennis Robertson, 29 January 1935, quoted in (Keynes 1971 XIII, 495, italics in original)).

5. Keynes on Rules and Private Language

Had Keynes followed Wittgenstein in the ways suggested by either Bateman or Coates he would have been led into error. Fortunately he was not tempted. There was, however, one point on which Keynes clearly did not follow Wittgenstein, and sadly so for Wittgenstein was right. If Kripke (1982) is correct and this is the crucial point in the later Wittgenstein's thinking, Keynes's failure to observe it provides strong evidence that Wittgenstein's influence on him was at best slight.

Keynes, as we saw above, thought we dealt with uncertainty by

assuming that the future would resemble the present. Call this Keynes's maxim. But this, points out Wittgenstein, gets us nowhere. We know that the future will resemble the present; what we don't know is how it will do so. Wittgenstein illustrates this with examples from mathematics and semantics, but we can apply it more broadly.

Say that a particle in a one-dimensional Euclidean space is now at position d, travelling at velocity v under acceleration a. Assuming things stay the same, where will the particle be in 1 unit of time? This question simply can't be answered, until we know what in what respect things will 'stay the same'. If it is in respect of position, the answer is d, in respect of velocity it is $d + v$, in respect of acceleration $d + v + a/2$. Perhaps our Newtonian intuitions make us prefer the second answer, perhaps not.

The same story applies in economics. When we assume things will stay the same, does that mean we are assuming the unemployment rate or the rate of change of the unemployment rate to be the same; real growth or nominal growth to be constant? At the level of the firm, we can ask whether Keynes's maxim would have us assume real or nominal profits to be constant, or perhaps the growth rate of real or nominal profits, or perhaps sales figures (real or nominal, absolute or variation), or perhaps one of the variables which play a role like acceleration (rate of change of sales growth)? In some computing firms we might even take some of the logarithmic variables (growth of logarithm of sales) to be the constant. We can't in consistency assume more than one or two of these variables to be unchanged, yet Keynes provides us with nothing to tell between them.

More importantly, it looks like Keynes hasn't even seen the problem. The mechanical example above looks very similar to some of the paradoxes of indifference (*TP*: Ch. 4). For example, in von Kries's cube factory example, we know that a factory makes cubes with side length between 0 and 2cm. If that's all we know, what should we say is the probability that the next cube's side length will be greater than 1cm? According to Laplace's principle of indifference we should divide the probabilities equally between the possibilities, which seems to give an

answer of 1/2. However we could have set out the problem by saying that the volume of cubes produced is between 0 and 8cm^3 and we want to know the probability the volume of the next cube is greater than 1cm^3. Now the answer (to the same problem) looks to be 7/8. And if we set out the problem in terms of surface area we seem to get the answer 3/4. The conclusion is that the principle of indifference could only be saved if we have a small designated set of predicates to which we can exclusively apply it. But now it seems Keynes's maxim can only work if we have a small designated set of predicates to which we can exclusively apply it, and if we do that we can avoid the paradoxes of indifference. Keynes explicitly adopts his maxim to avoid the paradoxes of indifference (*GT*: 152). He would hardly have done this if he knew structurally similar problems beset the maxim as best the principle of indifference. As further evidence he just missed this point, note that while he was not averse to wielding philosophical tools in economic writing (like the paradoxes of indifference), Wittgenstein's point is not mentioned; not in the *GT*, not in any of its drafts and not in any of the correspondence after it was published.

For Kripke, this point is central to Wittgenstein's private language argument. All that we can know about the meaning of a word is how our community has used it in the past. We must assume they'll use it the same way in the future. But what is to count as using it the same way? *A priori* it looks like any usage of a word could count; the only thing that could make usage of a word wrong is the user has a different way of using the word 'the same way' to everyone else. Hence if there is no community to set such standards there are no bars on how words can be used. And if there are no such bars, there is nothing that can properly be called a language. Hence there can't be a private language.

Given the importance of that conclusion to Wittgenstein's later philosophy, if Kripke is even close to right in his reconstruction then it is central to the later Wittgenstein that Keynes's maxim is contentless. As Keynes clearly didn't think this (witness the central role it plays in summaries of the *GT* like Keynes 1937) he hasn't adopted a central tenet of the later Wittgenstein's work. This puts a rather heavy burden

on those who would say he became a Wittgensteinian. The arguments presented so far do nothing to lift that burden.

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